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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1858, *TO JULY* 1859.

VOL. XIX.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

LONDON:

PRINTED BY

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1859.

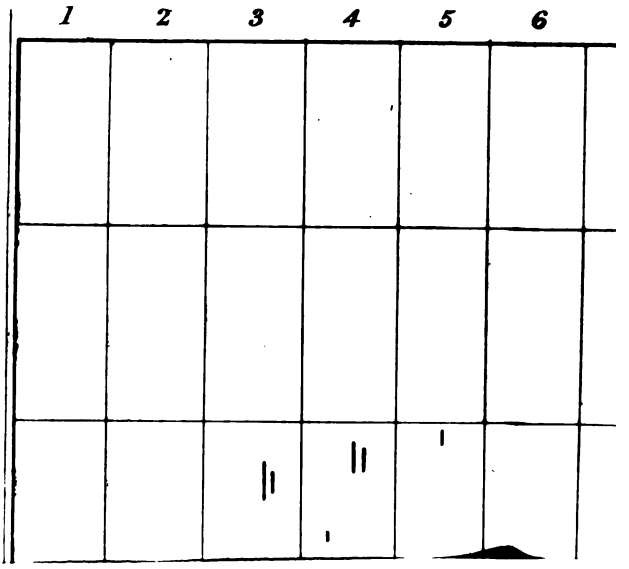
roughly set at between 20° and 40°, with exceptions in favour of the old equatoreal region. The tendency at the present time appears to be to contraction of the parallels.

The method of reduction employed having already been explained and sufficiently illustrated in these pages, it is only necessary on this occasion to explain the arrangement of the accompanying map.

In a mean solar day the sun rotates through an angle of 14° 11′ very nearly, and the heliographical longitudes of spots, as compared with that of the centre of the disk, increase by the synodic quantity corresponding. If an observer plots down on each day the positions reduced to noon of the spots recorded on the 180° viewed, and lays the successive sheets so as to superpose the positions of the same spot, his plotting sheets will advance daily to the left by 13° 12′ nearly. And if he then agrees to adopt one figure of a spot as typical, to be laid down on a single continuous sheet, he will obtain a result such as I have deduced by computation (with the exception that for convenience my map is reversed left for right), and such as might be supposed to be produced by imagining the sun to be the central cylinder of one of Applegarth's printing-machines. In this state the graphical result would be of too extreme dimensions in length for the eye to take it in as a whole; and, accordingly, in the map lithographed the scale in longitude is reduced in the proportion of 72 to 1, as compared with the scale in latitude, and the delineations of spots compressed to mere vertical lines. The uncertainty of the exact period of rotation affects the result only in this way, that if the true period be somewhat longer than that adopted, the vertical lines which indicate the commencement of successive rotations, should be drawn somewhat wider apart, and *vice versâ;* a matter of no importance, obviously, for our present purpose.

The variation of the limiting parallels being established, the inquirer next desires to turn back to such past records as may throw light on their changes and their relation to epochs. In this respect I would call attention to a short, but very condensed and important paper, communicated to me by Dr. C. H. F. Peters, formerly of Naples, and lately of Albany, and published, I believe,* by the American Association for the Advancement of Science. Dr. Peters observed the sun with an object and purpose very similar to my own, from September 1845 to October 1846 inclusive, and obtained 813 places of 286 spots, which he subsequently reduced with a skill and exactness in which I place great confidence. He has given a plate exhibiting the gross distribution in latitude during the period of his labours, in which I find the limits laid down as 40° N. and 30° S. with a desolate region from 8° N. to 5° S., and with, on the whole, a preponderance of action in the north

* I have by me only the sheets of the volume containing this particular paper, sent by post.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hemisphere. We know now that at that time the sun was passing from a period of minimum activity to a maximum, as now. The distribution is very similar to that which now holds, excepting that at the present time there appears an excess of activity in the south. The records of Dr. Peters are not all which leisure and research may make available in this branch of the history of solar action, there are those of Sœmmering and others; nevertheless, it is difficult to express the degree of regret which a student of the sun feels when evidence such as the present meets him of the state of maturity his subject might have attained ere this, had not the opportunities of two centuries been neglected, by his predecessors condemning the research as one of idle curiosity, fit matter for a University thesis, but below the level of Philosophy.

The most cursory consideration will show that success in educing such conclusions in the case of the sun depends mainly on the continuity of the labours of the observer. The conclusion which an observer would have arrived at from a discussion of the observations made during the years 1854 and 1855 would have been exceedingly imperfect, though apparently borne out by a tolerably extensive experience; and the conclusion which I draw from the four years' results now accumulated is, that our knowledge of the sun's action is but fragmentary, and that the publication of speculations on the nature of his spots would be a very precarious venture.

I am very anxious to know what the magnetic observers have to produce, corresponding or not corresponding with the results of my map; and I would take the opportunity of remarking that the question of the correspondency of the solar and magnetic disturbance phenomena is in the curious and imperfect state of a correspondency established in the aggregate, but not for particulars.

I shall shortly offer some conclusions on the independent movement of spots, and on the divergence of neighbouring nuclei, a very singular and marked action, in the detection of which I find, however, that Dr. Peters has anticipated me.

New Variable Star (R *Sagittarii*). By Norman Pogson, Esq.

(*Communicated by Dr. Lee.*)

On the night of August 7th, 1849, Professor Argelander observed a star of the 8·5 magnitude, south preceding a fine but widely-spread group, which has recently proved to be variable. In August 1856, when commencing my chart of Hour 19, the star in question was missing; but supposing an erratum in the published zone, no suspicion was entertained of

its variability. However, on July 3d of this year, while planet-seeking with the Smythian telescope, I was struck by finding it in the position assigned by Argelander, and a full 8th magnitude. Since that date the following changes have been recorded, not simply by estimation, but by the more certain and satisfactory plan now usually adopted of comparing with adjacent stars of assumed magnitudes:—

1858, July 3,	8·2 magnitude	1858, Aug. 8,	9·6 magnitude
11,	8·0 ,,	31,	11·2 ,,
19,	9·0 ,,	Sep. 5,	11·4 ,,
29,	9·3 ,,	Oct. 5,	12·8 ,,
Aug. 1,	9·4 ,,	Nov. 9,	invisible (under 12·5)

It appears most probable that if the variation be tolerably regular, seven maxima have occurred between the years 1849 and 1858; in which case the period will not differ much from 465 days, and the next maximum will fall due in September or October, 1859.

The position of R *Sagittarii*, as determined by micrometrical comparison with A.Z. 227·120, are found to be, when reduced to 1860, R.A. $19^h\ 8^m\ 28^s{\cdot}62$; South Dec. $19°\ 32'\ 59''{\cdot}2$. That given by Argelander, reduced to the same epoch, being R.A. $19^h\ 8^m\ 28^s{\cdot}49$; and South Dec. $19°\ 32'\ 57''{\cdot}4$.

2 *South Parade, Oxford, Nov.* 11, 1858.

On the Value of the neglected Terms in the Ordinary Expression for the "Equation of Equal Altitudes." By J. Riddle, Esq.

In order to elucidate what follows it will be necessary to refer for a moment to the expression erroneously used in the calculation of the correction, known under the designation of the "Equation of Equal Altitudes," viz.,

$$-e = c \cot p \cot h - c \cot l \operatorname{cosec} h \quad \ldots\ldots\ldots \quad (1)$$

Where p = polar distance of the sun, l = colatitude of the place of observation, h = half the elapsed time between the A.M. and P.M. observations of equal altitudes of the sun, c the change of the sun's declination in the time h, and $-e$ the equation of equal altitudes.

This expression (1) is obtained directly by the differentiation of the fundamental one in Nautical Astronomy.

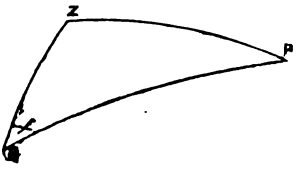
$$\left\{\begin{array}{c} \cos z = \cos p \cos l + \sin p \sin l \cdot \cos h \\ z = \text{zenith distance: and } h = \text{hour-angle or meridian distance.} \end{array}\right\}$$

Considering p and h only to change, it (1) is in fact only the value of $\frac{dh}{dp} \cdot c$, or the first term in the total difference which the change of the sun's declination produces in the hour-angle h. And this, in all our elementary treatises on Astronomy, is employed as a sufficient correction of the middle instant indicated by the chronometer, between the A.M. and P.M. observations of equal altitudes of the sun, to deduce the time shown by the chronometer at noon; and this, too, without any allusion to the value or *no* value of the neglected terms.

If, however, the next term be required,

$$\frac{d^2h}{dp^2} \cdot \frac{c^2}{2},$$

it is by no means so easily obtained, in a practicable form, by a second differentiation of the already determined value of $\frac{dh}{dp}$; a convenient mode of attacking it has therefore some interest. There is not much difficulty in reducing the value of $\frac{dh}{dp}$ to the more concise form of $-\frac{\cot S}{\sin p}$; where S represents the angle at the sun between the zenith distance and polar distance; and looking at the geometry of the problem, this is the simplest form in which it presents itself, and therefore



$$-\epsilon = -\frac{\cot S}{\sin p} \cdot c = \frac{dh}{dp} \cdot c \quad \ldots\ldots\ldots \quad (2)^*$$

Commencing then with this,

$$\frac{dh}{dp} = -\frac{\cot S}{\sin p}$$

and differentiating

$$\frac{d^2h}{dp^2} = \frac{\operatorname{cosec}^2 S \cdot \sin p \cdot \frac{dS}{dp} + \cot S \cdot \cos p}{\sin^2 p}$$

But since $\frac{dh}{dp} = -\frac{\cot s}{\sin p}$ $\therefore$ $\frac{ds}{dp} = -\frac{\cot h}{\sin p}$

(for S and h are the two angles of the spherical triangle adjacent to p),

$$\therefore \frac{d^2h}{dp^2} = \frac{-\operatorname{cosec}^2 S \cdot \cot h + \cot S \cdot \cos p}{\sin^2 p}$$

$$= -\frac{(1 + \cot^2 S) \cot h - \cot S \cos p}{\sin^2 p}$$

* See the note at the end of this paper.

$$= -\frac{\cot h}{\sin^2 p} - \frac{\cot^2 S}{\sin^2 p} \cdot \cot h + \frac{\cot S}{\sin p} \cdot \cot p$$

And multiplying by $\frac{c^2}{2}$

$$\frac{d^2 h}{dp^2} \cdot \frac{c^2}{2} = -\frac{\cot h}{\sin^2 p} \cdot \frac{c^2}{2} - \frac{\cot^2 S}{\sin^2 p} \cdot \frac{c^2}{2} \cot h + \frac{\cot S}{\sin p} \frac{c^2}{2} \cdot \cot p$$

but $-\frac{\cot S}{\sin p} \cdot c = -e \therefore \frac{\cot^2 S}{\sin^2 p} \cdot \frac{c^2}{2} = \frac{e^2}{2}$

And therefore

$$\frac{d^2 h}{dp^2} \frac{c^2}{2} = -\frac{\cot h}{\sin p} \cdot \frac{c^2}{2} - \frac{e^2}{2} \cdot \cot h - \frac{ec}{2} \cot p$$

or,
$$= -\tfrac{1}{2} \cot h \left\{ \frac{c^2}{\sin^2 p} + e^2 \right\} - \frac{ec}{2} \cot p \quad . \quad . \quad (3)$$

And this multiplied by sin 1″ will be the value of the second term of the difference when c and e are estimated in seconds.

In attempting to seek the largest values of the expressions (1) and (3), we are at once forced to consider the practical limits of the application of the problem. For at the poles cot l, and therefore e, are infinite; and this points, however vaguely, to a limit of latitude beyond which the problem is not fairly applicable. I am disposed to fix this limit at 70°, as being quite high enough, at any rate, for ordinary navigation, leaving the higher latitudes for a special polar legislation. (Even though I have been informed by an indefatigable observer that he has used this method several degrees farther north.) This limitation of latitude also includes a maximum limit of h.

A second and inferior limit of h is pointed out in the expression (1); for as cot h enters into it h ought not to be very small, 2^h or 30°, I think, would be an extreme limit here.

But a third, and for the present purpose, an infinitely better limiting condition, is furnished by equation (2) which is the second form of equation (1), viz.,

$$e = \frac{\cot S}{\sin p} \cdot c \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For if the angle S, between the zenith distance and the polar distance be very small, it will give rise to an "ill-conditioned" triangle, which is at all times to be avoided. S should hardly be less than 10°. This condition has also the advantage of including those before mentioned.

We are now prepared to enter upon the discussion of the values of terms (2) and (3).

For the first, viz., $\frac{\cot s}{\sin p} \cdot c = e$, to find its largest value we must take the smallest values of S and p and the largest

possible value of c. For these we have $S = 10°$ and $p = 66\frac{1}{2}°$, and as far as c is concerned the following considerations. The greatest hourly change of the sun's declination is about $60''$ in March and September; and taking half the elapsed time, or h, between the A.M. and P.M. observations as 6^h (a large value in actual practice), we have $c = 360'' = 24$ seconds of time. These values of S, p, and c give $e = 148{\cdot}4$ seconds of time, or about *two minutes and a half*, certainly an excessively large value for the equation of equal altitudes.

Next for the examination of the several parts of the expression,

$$\sin 1'' \frac{d^2 h}{dp^2} \cdot \frac{c^2}{2} \quad \left\{ -\tfrac{1}{2} \cot h \left(\frac{c^2}{\sin^2 p} + e^2 \right) - \frac{ec}{2} \cdot \cot p \right\} \sin 1'' \quad . \quad (3)$$

The large values of e and c above found give

$$ec = 3561{\cdot}6 \text{ seconds}$$

and the greatest value of $\frac{1}{2} \cot p \,.\, \sin 1'' = {\cdot}000001$, and therefore

$$\frac{ec}{2} \,.\, \cot p \,.\, \sin 1'' = {\cdot}0035616 \text{ seconds of time,}$$

and may be dismissed as of no appreciable value in practice.

There remains still

$$-\tfrac{1}{2} \cot h \left\{ \frac{c^2}{\sin^2 p} + e^2 \right\} \sin 1''$$

Or, since $\sin^2 p$ varies only between the values 1 and ·8,

$$-\tfrac{1}{2} \cot h \left\{ c^2 + e^2 \right\} \sin 1'' \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Here again $(c^2 + e^2)$, with the large values found for c and e, amounts to 22598·6 seconds, and taking h at 15° (only one-half of the inferior limit which I am disposed generally to assign to it),

$$\tfrac{1}{2} \cot h \,.\, \sin 1'' = {\cdot}000009$$

And hence the final value of (4) is ·2035; or nearly $\frac{2}{10}$ seconds of time is found for an excessively large value of the second term of the difference, or secondary correction, and it may therefore also be safely neglected in nautical practice.

Note.

The following are the reductions referred to in this paper:—

$$\cos z = \cos p \,.\, \cos l + \sin p \,.\, \sin l \cos h$$

$\therefore$ differentiating,

$$0 = -\sin p \,.\, \cos l + \cos p \,.\, \sin l \,.\, \cos h - \sin p \,.\, \sin l \,.\, \sin h \,.\, \frac{dh}{dp}$$

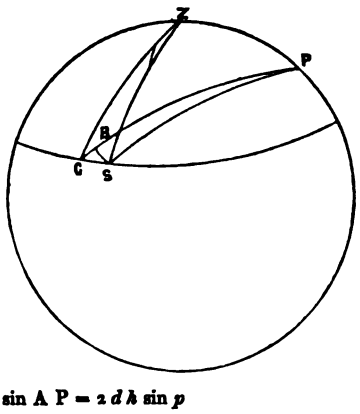
$$\therefore \frac{dh}{dp} = \cot p \,.\, \cot h \,. - \cot l \,.\, \operatorname{cosec} h$$

$$= \frac{\cos p \,.\, \cos h}{\sin p \,.\, \sin h} - \frac{\cot l}{\sin h}$$

$$= \frac{\cos p \,.\, \cos h - \cot l \,.\, \sin p}{\sin p \,.\, \sin h}$$

$$= -\frac{\cot S \,.\, \sin h}{\sin p \,.\, \sin h} = -\frac{\cot S}{\sin p}$$

Or thus, let P C = the polar distance at the A.M. observation, and P A the polar distance at the P.M. observation, and A B a parallel of declination, as the polar distance is diminishing



$$C B = -2C = 2 \,.\, dp,$$

And $\angle$ A P C is the increment of the hour-angle in consequence of this diminution in the polar distance $= 2 \,.\, dh$.

$$A B = A P B \sin A P = 2 \, dh \sin p$$

And $\frac{A B}{B C} = \cot \angle B A C = \cot \angle Z A P = \cot S$

$$\therefore -\frac{dh}{dp} \,.\, \sin p = \cot S$$

$$\therefore \frac{dh}{dp} = -\frac{\cot S}{\sin p}$$

Greenwich Hospital, November 9th, 1858.

Results of the Observations of Small Planets, made with the Transit Circle, and of Donati's Comet, made with the Alt-azimuth, at the Royal Observatory, Greenwich, from July to October 1858.

(*Communicated by the Astronomer Royal.*)

Proserpine. (86)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, July 19	12 39 55·5	20 29 57·91	114 56 24·18
Aug. 7	11 7 39·1	20 12 21·00	115 53 17·49
9	10 58 6·8	20 10 40·20	115 57 5·38
11	10 48 42·8	20 9 7·78	116 0 19·12
16	10 25 17·4	20 5 21·33	116 6 19·76
Sep. 18	8 7 37·0	19 57 24·55	115 38 34·90

Lætitia. (80)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Aug. 13	14 51 28·8	0 20 26·76	90 57 27·08
16	14 39 24·2	0 20 9·87	91 16 17·30
20	14 22 57·8	0 19 26·99	91 44 2·84
27	13 53 17·7	0 17 17·82	92 39 16·81
Sep. 1	13 31 26·4	0 15 5·75	93 23 4·81
11	12 46 24·1	0 9 21·60	94 57 56·04
13	12 37 13·7	0 8 2·78	95 17 29·27
15	12 28 1·4	0 6 42·05	95 36 59·39
16	12 23 24·5	0 6 1·01	95 46 46·06
28	12 14 9·4	0 4 37·48	96 6 5·12
23	11 50 58·5	0 1 5·50	96 53 31·41
24	11 46 20·2	0 0 23·02	97 2 46·16
27	11 32 26·0	23 58 16·23	97 29 45·78
30	11 18 34·7	23 56 12·30	97 55 39·26
Oct. 1	11 13 58·3	23 55 31·68	98 3 59·23
2	11 9 22·4	23 54 51·62	98 12 8·23
8	10 42 1·9	23 51 5·90	98 57 19·96
9	10 37 31·3	23 50 31·08	99 4 12·20
11	10 28 33·3	23 49 24·78	99 17 9·66
13	10 19 39·2	23 48 22·33	99 29 19·97
22	9 40 37·2	23 44 42·91	100 13 29·45
25	9 28 0·8	23 43 54·03	100 22 33·77
26	9 23 51·5	23 43 40·66	100 25 44·82
28	9 15 37·2	23 43 18·08	100 30 33·49
30	9 7 28·9	23 43 1·56	100 34 18·37

Pomona. (89)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Oct. 11	12 4 4·6	1 25 11·78	78 5 7·04
14	11 49 44·4	1 22 38·83	78 25 25·38
15	11 44 57·8	1 21 47·98	78 32 12·06

Ariadne. (43)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Oct. 8	14 42 1·0	3 51 44·41	66 16 18·66
11	14 28 48·9	3 50 19·87	66 20 30·91
15	14 10 45·1	3 47 59·32	66 27 58·44
20	13 47 28·9	3 44 22·04	66 40 25·62

Iris. (7)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, July 14	12 20 31·1	19 50 47·55	105 30 34·96
17	12 5 33·5	19 47 37·20	105 31 49·26
19	11 55 33·8	19 45 28·89	105 32 50·83
29	11 5 43·4	19 34 55·94	105 40 13·56
31	10 55 50·9	19 32 54·94	105 42 1·23
Aug. 6	10 26 36·5	19 27 15·04	105 47 53·40
9	10 12 15·1	19 24 41·00	105 50 54·54
11	10 2 47·9	19 23 5·32	105 52 58·42
13	9 53 26·4	19 21 35·38	105 54 56·57
19	9 26 4·2	19 17 48·04	106 1 1·25
23	9 8 25·3	19 15 52·45	106 4 51·55
24	9 4 5·2	19 15 28·22	106 5 48·33
25	8 59 47·3	19 15 6·12	106 6 43·11
27	8 51 17·0	19 14 27·57	106 8 25·13
28	8 47 4·9	19 14 11·28	106 9 13·99
30	8 38 46·4	19 13 44·57	106 10 53·29
Sep. 13	7 44 21·1	19 14 22·07	106 19 5·75
15	7 37 6·1	19 14 59·03	106 19 41·58
24	7 5 59·8	19 19 16·59	106 19 54·90

Urania. (30)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Sep. 11	13 51 16·3	1 14 24·46	78 40 20·76
13	13 42 25·5	1 13 25·27	78 42 38·64
16	13 28 56·7	1 11 43·91	78 47 39·05
18	13 19 49·7	1 10 28·53	78 51 53·23
25	12 47 10·5	1 5 19·83	79 12 37·95
30	12 23 18·2	1 1 6·39	79 32 20·47
Oct. 2	12 13 39·6	0 59 19·35	79 41 16·24
8	11 44 34·8	0 53 49·11	80 10 29·78
11	11 30 1·9	0 51 3·43	80 26 8·95
13	11 20 21·5	0 49 14·59	80 36 47·46
14	11 15 32·1	0 48 20·89	80 42 8·30
15	11 10 43·1	0 47 27·72	80 47 26·54
22	10 37 24·9	0 41 39·95	81 24 20·69
28	10 9 38·6	0 37 28·38	81 53 29·12
30	10 0 35·4	0 36 16·79	82 2 23·86

Leda. (38)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Aug. 7	11 30 54·8	20 35 40·45	106 8 11·40
16	10 47 51·3	20 27 58·85	106 22 6·52

Parthenope. (12)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, July 8	11 19 37·2	18 26 4·29	109 52 47·55
14	10 50 40·6	18 20 42·28	110 12 34·83
17	10 36 25·9	18 18 14·87	110 22 40·81
19	10 27 2·0	18 16 42·55	110 29 16·17
21	10 17 44·4	18 15 16·54	110 35 54·00
28	9 46 1·8	18 11 4·68	110 58 43·26
30	9 37 14·1	18 10 8·69	111 5 8·11
31	9 32 52·7	18 9 43·06	111 8 17·82
Aug. 9	8 55 9·2	18 7 22·35	111 35 52·81
13	8 39 14·9	18 7 11·75	111 47 31·03

Egeria. (13)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Aug. 25	14 49 12·0	1 5 28·25	102 32 22·44
Sep. 11	13 31 38·1	0 54 43·00	103 36 45·08
13	13 22 4·2	0 53 0·66	103 43 41·29
16	13 7 36·2	0 50 19·99	103 53 22·87
18	12 57 52·4	0 48 27·68	103 59 27·36
23	12 33 19·6	0 43 33·60	104 12 15·90
Oct. 2	11 48 39·4	0 34 15·07	104 25 25·95
5	11 33 44·0	0 31 6·88	104 26 33·85
8	11 18 51·8	0 28 1·83	104 25 53·10
11	11 4 3·7	0 25 0·98	104 23 11·33
13	10 54 14·5	0 23 3·25	104 20 20·99
27	9 47 23·3	0 11 12·86	103 36 19·05

Alexandra. (54)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Oct. 25	7 22 13·1	21 37 45·74	94 41 41·13
30	7 5 45·4	21 40 58·07	94 23 46·49

Nysa. (44)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Sep. 13	10 12 2·7	21 42 27·96	105 59 59·52
16	9 58 19·0	21 40 31·60	106 11 56·71
18	9 49 16·0	21 39 20·32	106 19 4·74
Oct. 2	8 48 5·2	21 33 11·14	106 54 51·23

The observations of *Nysa* in N.P.D. have not been corrected for parallax.

Right Ascensions and North Polar Distances of Donati's Comet, deduced from Observations made with the Altazimuth.

Mean Solar Time of Observation.		Apparent R.A.		Apparent N.P.D.	
	h m s	h m s		° ′ ″	
1858, Sept. 24	7 49 55·7	12 14 54·91		54 48 53·96	
25	8 18 26·8	12 22 59·48		55 20 12·56	
29	16 50 56·6	13 3 28·33		59 16 3·57	
Oct. 1	6 55 40·1	13 20 36·42		61 36 19·55	
2	6 41 31·3	13 31 52·48		63 22 11·81	
5	6 41 54·5	14 8 4·61		70 17 47·17	
9	6 17 26·3	14 57 53·14		82 50 49·10	
11	6 25 17·9	15 22 5·66		89 56 10·50	

Remarks on the Appearance of Comet V., 1858, (*Donati's Comet*), *accompanied by Drawings; as seen at the Royal Observatory, Greenwich, chiefly with the Telescope of the Sheepshanks' Equatoreal.*

1858, Sept. 27, 7ʰ. With a doubly refracting prism applied to the unarmed eye, one image of the tail *nearly* disappeared when the images were placed side by side, and the other when they were placed end to end. I found, upon comparing these effects with those produced on an image reflected from unsilvered glass, that the light is polarised in the plane of the comet's tail.

On trying the light of the head in the same manner, as seen in the Sheepshanks' telescope, there were feeble signs of polarisation.—[G. B. A.]

Sept. 30, 7ʰ. The comet was imperfectly seen. Length of the tail equal to the distance from *α Ursæ Majoris* to a point equidistant from *ζ* and *η Ursæ Majoris*. The tail curved; its convexity to the left.—[G. B. A.]

Oct. 2, 7ʰ. The night favourable. The comet's head much brighter than *Arcturus*. The tail, when it left the head pointed at first not quite to *α Draconis*, but to a place between *α Draconis* and *γ Ursæ Minoris*, but nearer the former; the curvature then made the extremity of the tail point to the right (or to the lower side) of *η Ursæ Majoris*; its length being equal to the distance from *α* to *η Ursæ Majoris*. I could almost fancy that a more brilliant part of the tail proceeded straight from the head in the first-mentioned direction, and a fainter part was then attached to its side. The signs of polarisation were quite distinct, though much less strong than on a previous evening. On viewing the head with the Sheepshanks' telescope it had this appearance:—1st. The parabolic envelope and enclosed illumination. 2d. A brighter flat circular disk (with no special ring of brightness) laid upon the inclosed illu-

mination, just touching the parabola at its vertex. 3d. A still brighter, flat, circular disk, concentric with the last, about one-fourth of its diameter; no bright ring. 4th. The nucleus, concentric with the last, about one-third of its diameter, well defined, and looking very hard. From the nucleus a dark shadow diverged, with an angle at first of 30° or more, diminishing afterwards to 10° or 15°, cutting off the light of the circular disks and everything except the nucleus itself.—[G. B. A.]

The nucleus has a very bright stellar point, and is surrounded by a brightness similar to a planetary disk of 5″ or 6″ in diameter; the circle of the disk is not complete, its lowest part being cut off, through an angle of about 60° or 70°, by a well-marked shadow commencing immediately below the stellar point, continuing downwards, as seen inverted, in the direction of the train, and inclined at an angle of several degrees to the axis of the envelope. Surrounding the planetary disk is an annulus of light, about four times its diameter, considerably fainter, but brighter than the envelope generally, and about two-thirds complete, the lower part being obscured by the same shadow which cuts off a portion of the inner disk; its circumference is exceedingly well defined and of uniform brightness, excepting when it is immediately in contact with the inner annulus, where, perhaps from the effect of contrast, it appears dark.—[R. M.]

Oct. 3, 7^h. The appearance of the comet is generally similar to that of last evening. The inner disk, however, appears larger; and on its surface is a well-defined dark spot, about the same size as the nucleus, situated a little to its left, in the direction of the comet's diurnal motion.—[W. C.]

The centre of curvature of the southern edge of the comet's tail was about 3° below 12 *Canum Venaticorum* (*Cor Caroli*).—[G. B. A.]

Oct. 4, 7^h. The night unfavourable, and the comet was seen for a few minutes only. The outer annulus of light was narrower and less brilliant; the inner disk was evidently broader than on previous evenings; the spot on its surface was distinctly seen in the same position as before.—[W. C.]

Oct. 5, 7^h. The nucleus was much as before; but I am not certain whether the small surrounding circle was visible. I fancied that I saw it, much contracted. The large surrounding circle was very plain, and was distinctly separated from the parabolic envelope; the black shadow more obtuse; the tail about the same length. The general light of the comet, I think, was diminished. Its head now is not so bright as *Arcturus;* whereas, in the last two views I thought it much brighter than *Arcturus.*—[G. B. A.]

The diameter of the inner disk immediately surrounding the nucleus, which has been increasing for the last three days, is now about double its apparent size on Oct. 2. The spot on

its surface appears to be developing itself in the form of a dark concentric ring, as it is considerably elongated, and a little to its right are slight traces of its continuation. The outer circle of light has entirely disappeared. This may, perhaps, be owing to the general faintness of the comet as compared with its brightness on the last few evenings. It has been continually diminishing both in lustre and width. The shape of the envelope appears rather sharper than a parabola. *Arcturus* was in the middle of the tail at $7^h\ 15^m$ M.T.; its distance from the nucleus being as nearly as possible equal to twice the width of the tail measured through the point bisecting that distance.—[W. C.]

Oct. 9, 7^h. The bright point exhibits more of a planetary character than when I last saw it. The inner disk is of a silvery whiteness, and is complete on the apparent upper side of the nucleus; the lower part being cut off by the dark shadow, which is much more diffused, but very dark immediately under the nucleus, spreading out into a parabolic form, leaving two branches of the envelope of considerable brightness on each side. The outer annulus is visible and well defined; but the lower part of it assumes more of a parabolic character, and seems to throw out streams, merging in the two branches of the envelope.—[R. M.]

Oct. 11, 7^h. The nucleus is still of a planetary character, and seems to be cut off on the under side by something like a phase of darkness. The inner disk now extends considerably below the bright point, and has decidedly assumed the parabolic character, throwing two small streams of light to a small distance, where they abruptly terminate. There is a dark curve passing through the extremities of this bright parabola and the central bright point, and going off into a parabolic form into the envelope, leaving a lune of light on each side. The outer bright parabolic annulus is not essentially different from that seen on Oct. 9. It first appeared well defined in the head; but later in the evening its definition was not so good, owing, perhaps, to a thin haze, indicating that its light is not strong. There is apparently a darkish band between the two parabolas of light. A peculiar feature in the comet's appearance this evening is a decided ray that shoots upwards (apparently), or towards the head of the comet, making an angle of 45° with the axis of the envelope towards the apparent left, as seen inverted, and extending about the length of the semidiameter of the nucleus beyond it.—[R. M.]

Oct. 15, $6^h\ 30^m$. The outer parabolic disk much in the same state as on Oct. 11, but a considerable change has taken place with respect to the inner one. The nucleus is seen at the apparent right-hand extremity of the inner disk, which appears to be thrown off from it towards the left in a kind of jet. The shadow beneath the nucleus is not very strong. The envelope is seen to extend beyond the outer disk, excepting immediately at the extremity of the parabola. [R. M.]

Oct. 16, $6^h\ 15^m$. The nucleus is in the same position as last evening, but appears less circular than hitherto. The peak, or vertex of the parabolic shadow seems—as far as the excessive faintness of the comet admits of correct observation—to issue from the centre of the disk in which the nucleus is eccentrically situated. The outer parabola of light is ill defined.—[W. C.]

The following measures of distance from the centre of the bright point to the extreme edge of the inner disk and outer annulus of the head of Donati's Comet, in the direction of the axis of the envelope, were made with a wire micrometer upon the Sheepshanks' equatoreal.

1858, October 9, 7^h.

Distance of bright point to outer edge of the inner disk (4 measures) = 16′
" " " " outer annulus (2 measures) = 38
Inclination of axis of the envelope to the meridian 64° to the East, the train diverging upwards.

Observer, R.M.

October 11, 7^h.

Distance of bright point to outer edge of the inner disk (4 measures) = 26′
" " " " outer annulus (2 measures) = 42
Inclination of axis of the envelope to the meridian 71° to the east.

Observers, R.M. and W.C.

October 15, 7^h.

Distance of bright point to outer edge of the outer annulus (2 measures) = 34′
Inclination of axis of the envelope to the meridian 90° to the east.

Observer, R.M.

October 16, 6^h.

Distance of bright point to outer edge of the inner disk (2 measures) = 16′
Inclination of axis of the envelope to the meridian 93° to the east.

Observer, W.C.

The initials G.B.A., R.M., W.C., are those of the Astronomer Royal, Mr. Main, and Mr. Christy.

In the telescopic examination of the head of the Comet the remarks refer to the appearance of the Comet as seen inverted in the telescope, though in the drawings the representations are as seen in the heavens.

Mr. Carpenter's drawings (which were exhibited at the meeting) were made mainly from the descriptions given above, and are intended to embody all the phenomena therein detailed; while Mr. Christy's are intended to represent the different appearances of the Comet as seen by himself with the Sheepshanks' equatoreal telescope.

Physical Phenomena of Comet V., 1858, *as observed with the Northumberland Telescope at the Cambridge Observatory.* By Professor Challis.

In the following account of the physical appearances of Donati's Comet observed by myself and my Senior Assistant, Mr. Breen, I have first given the phenomena as recorded in rough notes, or represented by sketches, at the times of observation, and have then added some explanatory and supplementary remarks. The observations were all made with a power of 166, the same that was used in taking positions of the comet by reference to stars, and most probably several details which could only be seen with high powers, have on this account escaped observation. At the same time the advantage was gained of judging with a greater degree of certainty of the variations of brightness and form in the larger features of the phenomenon.

Mr. Breen's observations were as follows:—

"1858, Aug. 19. Comet observed through dense cloud; bright and easily seen in the finder, although the field was too cloudy for seeing any stars." The aperture of the object-glass of the finder is $2\frac{1}{4}$ inches, focal length 29 inches, and diameter of the field of view $115'$.

"Aug. 23. Bright, but no trace of a tail; the sky clear, but the moon nearly at full.

"Aug. 27. Condensed nucleus. Aug. 30. Tail to north.

"Sept. 7. Very conspicuous to the naked eye, being nearly of mag. 2. Long and broad tail pointed directly north.

"Sept. 13. The nucleus beautifully sharp and stellar; the tail slightly curved and pointing nearly north.

"Sept. 15. Tail 3° in length; a fan-shaped tail to the nucleus opposite to the tail of the comet." A sketch exhibited the nucleus round, with two projections turned from the tail and equally inclined to its axis.

"Sept. 16. Bright and easily observed; power 166 as usual. The tail curved to the preceding direction and nearly north.

"Sept. 17. The nucleus very distinct and well defined. The tail is now slightly curved towards the east." A sketch exhibited the curvature and an excess of brightness on the convex side.

"Sept. 21. The nucleus like a star of the first mag. The tail about 5° in length, as well as I could judge.

"Sept. 24. Comet very brilliant; nucleus well defined; the luminous sector well seen and brightest at the edges." A sketch represented the nucleus as nearly round, surmounted by a luminous sector, which on the side towards the tail was bounded by two arcs tending to form a cusp at the nucleus,

and on the other side by a semicircular arch, brighter than the space between it and the nucleus. The coma extended beyond this arch, and in the direction of the tail was considerably fainter near the axis than at the sides.

"Sept. 25. Nucleus large and bright; no dark space between it and the arch." A sketch exhibited in other respects the same appearances as on Sept. 24.

"Sept. 27, 7^h. The luminous sector very bright and beautifully seen; the nucleus is now extremely small and bright like a star,— almost dissipated, except a brilliant point,— quite changed since Sept. 25. The tail is getting broader at the end; it could be traced for $7\frac{1}{2}°$ in the finder." According to a sketch, the nucleus was round, and attached to it on the upper side (as seen in the telescope) was a luminous sector, depressed towards the tail on the right side. The coma extended beyond to a distance greater than the radius of the sector, and was terminated by a bright border between which and the sector was a darker space.

"Sept. 30, 8^h. The nucleus again large and planetary; no fantail, but a little rainbow over the nucleus as on Sept. 27. Dark band sharply defined down the tail; the outer boundary of the tail sharp on the right side, and ill-defined on the opposite side." By measurement with the position circle, the axis of the tail was inclined southward from the great circle through the pole by 13°.

"Oct. 5, 7^h. A hole in the fantail adjoining the nucleus." A sketch represented a dark patch on the left side of the nucleus, and within it near the nucleus a bright spot; also a less conspicuous patch above the nucleus.

"Oct. 6. The same appearance as on Oct. 5, but the dark part in the nucleus (?) was not so well or so largely seen, and the left side of the arch was not so well defined."

Oct. 8 and 9. Sketches on these days represented a wing-shaped envelope attached to the nucleus, the arch, as usual, farther off, with coma beyond, and the space dark under the nucleus.

Oct. 11. A sketch indicated two projections from the upper side of the nucleus, turned from the tail, and equally inclined to its axis. The attached envelope and the arch were nearly as before.

Oct. 15 and 16. According to the sketches, the near envelope was broader, and more depressed towards the tail on the left side than on the right side. The arch was marked at about the same distance from the nucleus as before. "The comet had now much faded."

My own observations and sketches were commenced when I perceived that the comet was exhibiting phenomena of a very remarkable character, and unlike any which it had happened to me to witness in any previous instance.

Sept. 27, 8^h. A drawing which I took at this time exhibited a small fan-shaped centre, the broader part turned from the tail, and the brightness increasing towards the narrow part which had a small, circular, and very bright termination [which is properly the nucleus]. This central part was surrounded, except for about 90° on the side towards the tail, with a kind of hood or envelope, less bright and much larger than the central part, but bright enough to be in definite contrast with the coma which extended from it to a breadth about equal to its radius. The hood was more depressed towards the tail on the right side (as seen in the telescope) than on the left side. The right side of the tail was also considerably the brighter; the intermediate part was comparatively dark. A star of mag. 8, seen through this part at the distance of a few minutes from the nucleus, exhibited no unusual phenomena. By measurement with a position-circle the axis of the tail was inclined southward from the great circle through the pole by 8°.

"Sept. 30, 7^h–8^h. In the finder a narrow dark band well defined was observable along the axis of the tail." My drawing represented the central brightness as larger and more oval than on Sept. 27, and having a round blunt termination at the lower or brightest part. Above the upper part was a less illumined space, increasing in brightness towards a luminous semicircular arch pretty definitely bounded on the outside, beyond which the coma was faintly visible to some extent. This arch descended towards the tail a little more on the right side than on the left. The tail appeared to stream both from the arch and from the exterior and interior coma, and the right-hand stream was considerably the brighter. By a drawing of the appearance to the naked eye, the curvature of the tail was now very considerable, the convexity turned southward, the convex side much the brightest, and the extremity of the tail broad and diffused. Its estimated length was 20°.

"Oct. 2, $7\frac{1}{4}^h$. About the very bright and small nucleus on the upper side was a bright fan or hood, and beyond this a larger hood, with a less illumined space between them. The coma was traceable beyond the second hood. Under the cusp of the nucleus the space was remarkably dark, and a dark and rather broad band divided the tail into two parts which, at a considerable distance from the head, overlapped each other." A drawing represented the right-hand branch of the tail considerably the brighter, but the form and appearance of the head was now symmetrical about its axis. To the naked eye the tail was much more sharply defined on the southern than on the northern side, and was widely spread out at the extremity. Its length was 33°, as measured on a celestial globe, by reference to *Arcturus*, which was very close to the nucleus, and to stars of *Ursa Major*.

"Oct. 5, 7^h. There were two dark openings in the space

included by the external arch, the larger on the left side of the nucleus, and the smaller above it. Also between the larger and the nucleus was a bright point, but not so bright as the nucleus." A sketch exhibited these openings as faint patches irregular in form and distribution of light, and the bright point as a small spot of light near the nucleus. The right-hand stream of the tail was the brighter; the nucleus and fan-shaped attachment appeared nearly as on Oct. 2. This evening I looked at the light of the tail with a Nichol's prism and a tourmaline, and found that not far from the nucleus there was some polarisation.

"Oct. 6, 7^h. The cusp of the nucleus not so sharp to-night; the left-hand irregular patch seen, but the one above scarcely discernible. There was much cloud about." A sketch exhibited the bright spot on the right-hand border of the irregular patch, and very near the nucleus.

"Oct. 8, $6\frac{1}{2}^h$. Mottled appearance about the envelope [*i. e.* the space between the exterior arch and the bright central fan]. The left-hand patch seen, but not the bright spot. Apparently a greater divergence of the coma; the right-hand stream still the brighter." The central fan, as shown in a drawing, was spread out farther than on Oct. 6. To the naked eye the tail appeared more diffused than on Oct. 5, especially at its extremity. The greatest apparent curvature was at the distance from the nucleus of one-fourth its length. The length was but little diminished.

"Oct. 9, 7^h. The angular divergence of the streams of coma greater, and the light more diffused; the comet altogether less bright. The bright part of the nucleus and the hood appeared smaller. Clouds about and the sky unfavourable. The axis of the cusp and nucleus not coincident with the axis of the tail." [This appearance was probably owing to the *left* side of the envelope being now extended farther than the right in the direction of the tail.] To the naked eye the tail was fainter and still more diffused than on Oct. 8, but nearly of the same length.

"Oct. 11, $6\frac{1}{2}^h$. Greater angular separation of the streams of the tail; the hood not so definitely bounded, the central brightness rounder, probably owing to the state of the atmosphere, the comet being low." A sketch exhibited a determination of the hood towards the left side, but the two streams of the tail of equal intensity. To the naked eye the tail was much spread out laterally, especially at the extreme part, and the nucleus was less bright. The length of the tail was judged by reference to stars to be 30°.

"Oct. 13, $6\frac{1}{2}^h$. Comet scarcely to be seen for mist. I endeavoured to estimate the radius of the exterior arch in the direction transverse to the axis of the tail. I think it was 6^r of the micrometer, or $52''$. [This estimation is very uncertain.]

Oct. 15, $6\frac{1}{2}^h$. The length of the radius of the exterior arch in a direction inclined from its vertex by about 65° was found by measurement to be 4^r of the micrometer, or 35″. "The coma was distributed very unsymmetrically about the nucleus, being much more apparent on the lower, or northern side, than on the other. It was too faint on the latter side for measurement of the radius of the arch; there was a good deal of daylight. The boundary of the interior envelope was very definite; that of the exterior arch very indistinct and uncertain. The radius of the former on the apparent lower side was about one-third that of the other, or 12″." The nucleus and attached envelope presented in a sketch the form of an inverted comma. The interior boundaries of the two streams of the tail were confused, the coma spreading over the intermediate space. By measurement with a position circle the axis of the tail was inclined northward from the equatoreal direction by 4°.

"Oct. 16, 6^h The comet had nearly the same appearance as on the preceding evening; the comma form of the brightest part was seen in strong daylight; the boundary of the arch was still visible on the apparent lower side. By estimation from the teeth of the comb, its radius transverse to the axis was $4^r{\cdot}7$, or 41″. According to a drawing, the radius of the brighter part was three-sevenths of this measure, or about 18″. The axis of the tail was nearly in the equatoreal direction."

Professor Challis has appended to the foregoing observations a few explanatory notes, but as they will probably be published at full length in another form, we shall merely quote the following remark:

"In the course of the observations my attention was especially directed to the following particulars, respecting which I can speak with confidence. The brightness contiguous to the nucleus preponderated on the right side (as seen in the telescope) till October 2; and on October 9 the excess had passed to the left side. The excess of brightness of the right-hand stream of the tail above that of the other, attained its maximum about October 2; after which there was a gradual diminution, till, on October 11, 15, and 16, the two streams were not sensibly unequal. The dark band separating the two portions of the tail was of uniform width and definite boundary on September 30 and October 2; and in proportion as the boundaries afterwards became indefinite, and the intervening space was gradually filled with luminosity, the angular divergence of the two streams also increased."

The following additional details respecting the comet have been communicated:—

Observed at Bradstones, near Liverpool, by W. Lassell, Esq.

The time of observation was 1858, September 12, from 8 to 9 P.M. The comet was first viewed with the 20-foot equatoreal, with a power of 155, having a field of 19′·1 in diameter.

Nucleus estimated to be 10″ diameter, remarkably well defined. Its appearance reminded me of the disk of the planet *Uranus* in this telescope with a power of 400 when the atmosphere was unfavourable enough to make the edge of the disk soft. A slight bifurcation was suspected at about 10′ from the nucleus, but was not fully ascertained. Breadth of the tail about 12′. Edge of the tail on the side towards which the comet was advancing obviously brighter than the opposite edge.

The comet was also viewed with a refractor of 2·6 inches aperture and power 35, with a field of 76′. The tail was about twice the diameter of the field in length, or $2\frac{1}{2}$ degrees. The tail seemed narrower in proportion than in the 20-foot, but that might arise from the small portion of the tail visible at one time in the latter instrument. The substance of the tail appeared streaky in the direction of its length. It was almost exactly parallel from within a very short distance of the head and had a slight curvature, the convex side being that which, speaking relatively to the comet's motion, was the preceding side. The nucleus in this telescope seemed bright, but rather stellar than planetary.

Owing to the general cloudiness of the sky here I did not see this comet at all until the evening of the 11th September, and then only for a short time between clouds, too short to allow of placing the large telescope upon it.*

Observed at Tretire, Hereford, by the Rev. T. W. Webb.

Having lately received an object-glass from Mr. Alvan Clark, of $5\frac{1}{2}$ inches' clear aperture, of which he speaks highly, but which I have as yet had no opportunity of testing, I was anxious to turn it upon the recent magnificent comet; but a disappointment relative to the tube obliged me to content myself with a very rough temporary mounting, so that my observations were made under altogether unfavourable circumstances as to convenience; their results are, however, I believe, worthy of confidence.

1. *The Nucleus.* This appeared invariably circular, several seconds in diameter, of uniform light throughout; its definition decreasing with the increase of the power employed. As far

* The details of his subsequent observations of the comet may be expected from Mr. Lassell.—Editor.

as I could judge in the use of an excellent little $2\frac{1}{4}$-inch object-glass by Bardon, I think its distinctness on Oct. 11 had increased, as compared with its aspect on Sept. 21. Its colour was a clear yellow, not as deep as that of *Arcturus*.

2. *The Envelope.* This was of considerable extent, much fainter than the nucleus, but brighter than the exterior haze and train. Instead of being a hemispherical cap, serving as a base to the tail, as in the comet of 1811, its outline was distinctly continued through an arc of much more than 180°, round to the edges of the central darkness of the train. Sept. 30, when I perceived it on the first trial of the $5\frac{1}{2}$-inch object-glass, the portion next the sun was least luminous, and seemed to consist chiefly of a narrow arc of light, indicating a hollow structure; this bright ring became more evident at a greater distance from the sun, so that the portions adjoining the central darkness were most conspicuous, especially, perhaps, that on the preceding side, according to the comet's orbital motion. Oct. 4, the whole circumference of the envelope, or photosphere, as it might in this case be termed, appeared entirely filled up with light, excepting where the central darkness cut out a large gap in it. Oct. 5, it was doubtful whether or not its outline was defined by a more luminous arc; but now its principal brightness was confined to about a hemisphere, not however symmetrically situated with respect to the general figure of the comet, but inclining a little backwards as though left behind in its movement, so that the faint sector which completed its form round to the central darkness on the antecedent side was considerably larger than the corresponding one in the subsequent direction: this was not very distinctly made out, but such was the prevailing impression. Oct. 11, when the comet had advanced beyond the range of the great telescope, the smaller one showed, though of course less evidently, a similar appearance; but the more luminous hemisphere now seemed to be inclined the other way, or partially to precede the nucleus in its course, its central radius making an angle of about 30° with the axis of the tail, as though there existed a kind of swinging motion, such as was perceived in the last return of the comet of Halley.

3. *The Tail.* This gave some intimation of its structure even to the naked eye; in the telescope its central darkness was very conspicuous; and as it advanced in its course the sides of the hollow paraboloid became more thin, and the angle included by them increased. Sept. 30, the proportion of the dusky streak to the whole breadth of the tail was estimated at $\frac{1}{6}$: it occupied perhaps $\frac{1}{4}$ on Oct. 11, while it seemed to have become rather more filled up with nebulous light: the clear and broad notch which it cut out of the photosphere, even up to immediate contact with the back of the nucleus, increasing, of course, in breadth in a similar way. Oct. 4, it was traced for a length of 2° or $2\frac{1}{2}$° before it was merged in

the general brightness. The two streams which formed the tail were for a long time unequal in breadth, but were never observed to change sides so as to indicate rotation: the antecedent branch showed greater fulness and density near its origin even with the small object-glass on Sept. 24. With the large one, on Sept. 30, it was estimated the broader in the ratio of about 4 to 3; on Oct. 4 and 5, as 3 to 2. Oct. 11, they appeared with the small telescope of equal breadth. During the whole time the angle at which they came off from the nucleus underwent a steady increase: the antecedent stream was always far better defined than the other at its exterior edge. These two streams were connected round the sunny side of the envelope or photosphere, by a border of the same material, much narrower and fainter than the envelope. This border, which formed the vertex of the tail considered as a paraboloid, was, on Oct. 4 and 5, suspected to be bounded by a delicately brightened up semicircular edging, as though it were merely a thin shell; but this remained questionable. Beyond this border a considerable extent of very faint haze was visible, melting gradually into the clear sky. During the magnificent transit over *Arcturus* it was remarkable how distinctly and fully the light of the large object-glass brought out the tail, even close to the dazzling disk of that vivid star.

To the naked eye there was a remarkable irregularity in the curve of the tail, which gave strongly the impression of the exhaustion, beyond a certain distance of the projectile or repulsive force, and of a consequent diffusion and dispersion of the luminous material. This was first noticed on the evening of the transit, when the train extended in a regular curve as far as σ and ϱ *Boötis*, which were near its centre, but about one-third of the distance between ϱ and γ, where the tail attained its greatest width, the convexity began to be a little deflected backwards or flattened off. Oct. 8, this was still more conspicuous; the curve of the antecedent side, which was carried regularly up to α *Coronæ*, being subsequently deflected from the direction of δ *Draconis*, to which it had previously tended, towards a fresh point between ι and ζ *Draconis*, about one-third of their distance from the latter star, as far as could be ascertained in making an estimate of so very dim an object; sometimes I thought that ζ *Draconis* was the point indicated. The fainter branch could not be well made out higher than ψ *Boötis;* beyond this star it seemed, if anything, to approximate again towards the other branch; and the general impression of this side of the tail was that of spreading out like a feather, as compared with the more definite aspect of the convex edge. The whole length on this evening could not have been less than 45°; the greatest breadth, as measured by α *Coronæ* and ψ *Boötis*, about 7°. Oct. 10, the curvature appeared regular as far as a line joining α *Coronæ* and ζ *Herculis*, or perhaps a little further: thence a fainter ray of considerable

breadth was deflected at a large angle, perhaps 60°, as far as the stars of *Quadrans Muralis*. This portion was very feeble, but certain, and looked quite like a scattered and abandoned vapour. Another observer agreed with me in suspecting that it was less bright at its connexion with the regular tail than a little further off. Oct. 11, the tail has lost all the deflected prolongation, and extends in a simple curve a little above *ζ Herculis*, perhaps to *r*; its concave part is much scattered, and it has probably a breadth of 8° or 10°.

Unfavourable weather precluded any further observations.

The envelope or photosphere, and its interruption by the central darkness, were very clearly distinguished by Mr. With at Hereford, with an excellent object-glass by Bardon, of $3\frac{1}{4}$ inches aperture.

Observed at Rose Hill, Oxford, by J. Slatter, Esq.

Absence from home prevented me from making any observations of the comet before its coming to its perihelion.

The observation on Oct. 11 was made with a portable telescope $2\frac{1}{4}$ inches aperture, and a power of about 30, the comet being unable to be seen with the fixed telescope owing to trees. All the other observations were made with the 7-foot equatoreal, 5 inches aperture, and power 117.

Oct. 1, 7 P.M. *Nucleus* not round, irregular oval; perhaps paraboloidal. Larger diam. 12″, smaller 9″.

Parabolic Envelope distinct and bright, the shape of its posterior boundary not noticed, but in a rough sketch; it descends a little below the nucleus. Measuring through the nucleus, *i. e.* if parabolic, the parameter = 4′·1.

Oct. 5. Unable to find the comet in daylight. As soon as dark it formed a most splendid sight. The tail more curved, like a bright plume, sweeping upwards with *Arcturus* enveloped near the head, enclosing *γ Boötis*, central at the widest part, and *λ Boötis* just at its boundary, the tip reaching to the triangle formed by *ϵ*, *ι*, *κ Boötis*. This was the fullest size it attained. The nucleus to the naked eye not so bright as on the 1st, which was fully explained by the telescope.

7 P.M. *Nucleus* small and bright, and round; diameter 4″·8.

Envelope very large and bright; the posterior edges greatly prolonged behind the nucleus and opening out at an angle slightly differing from 90° by estimation. Parameter 59″.

To the naked eye, *Arcturus*, through the tail, appeared, if anything, to scintillate more than usual; and in the telescope its light appeared white instead of its usual golden colour.

Oct. 8, 7 P.M. Again to the naked eye a perceptible change, the head having become much brighter since the 5th. The tail also apparently more condensed on the brighter side, and much fainter in its upper part above *α Coronæ*.

Nucleus much larger, irregular in shape, but not sensibly differing from a parobola. Longer diameter 22″ (shorter 11″ by estimation).

Envelope very large, the posterior angle much more open; estimated at 150°. Parameter 83″. Inside the nucleus was a yet brighter very small nucleus, estimated at 2″ or 3″ diameter; in shape round.

Oct. 9, 6½ P.M. *Nucleus* large, paraboloidal, with a brighter core. Diameter of outer nucleus 31″; inner ditto 9″.

Envelope had vanished; no trace whatever of any. The tail had greatly diminished in length, not reaching much, if at all, above *α Cor. Bor.*

Oct. 11, 6 P.M. *Nucleus* bright; diameter (estimated) 8″.

Envelope. Another now visible, posterior edges open at about an angle of 150°. Parameter 45″ about, by measure.

The tail had gradually become more and more curved, and its axis at the head less and less inclined to the earth's equator. The axis of the envelope and nucleus made an angle of 7° or 8°, with the axis of the tail at its origin.

Sextant Observations of the Comet made at Kennington, Surrey, by John C. Haile, Esq.

Lat. 51° 28′ 49″ N. Long. 0° 7′ 0″ W.

Height above sea-level 20·25 feet.

Date.	G M.T.	Barometer.	Attached Thermom.	Temperature of Air.	Stars of Observation.	Distances corrected for Index-error.
1858.	h m s	inches.	°	°		° ′ ″
Sept. 24	7 42 43	30·4	60·0	57·5	Dubhe	30 2 35
					Arcturus	29 24 35
25	8 1 30	30·5	64·0	56·5	Dubhe	30 56 45
					Arcturus	27 40 0
27	8 40 28	30·3	63·0	61·0	Dubhe	33 34 0
					Arcturus	24 0 0
29	7 12 43	29·9	65·0	63·5	Dubhe	37 2 30
					Vega	65 49 15
Oct. 1	7 48 15	30·1	62·0	56·5	Dubhe	41 35 0
					Vega	63 43 30
2	7 35 48	30·0	62·0	58·5	Dubhe	44 20 15
					Vega	62 32 30
5	7 26 30	30·0	60·0	52·0	Dubhe	54 8 0
					Vega	59 14 45
8	7 14 3	29·8	56·0	47·5	Dubhe	66 8 30
					Vega	57 25 0
9	6 56 43	29·9	56·0	51·0	Dubhe	70 32 15
					Vega	57 24 37
11	6 42 13	29·8	54·0	47·0	Dubhe	79 27 40
					Vega	58 5 25
15	6 11 37	30·1	60·0	58·0	Arcturus	43 51 15
					Vega	62 6 45

Observed at Highbury, by T. W. Burr, Esq.

The comet was first seen on the 5th Sept. without a telescope. On the 8th the tail was quite half a degree long, of a brushlike form, and the nucleus bright and stellar in appearance. On the 12th the comet had wonderfully increased in brilliancy. The nucleus was more planetary and discoid in character, of a golden tint, and having the coma, or nebulous haze, extending round it on one side, and stretching away to a tail of about 3° of length in the opposite direction. There was a well-marked, comparatively dark, separation between the nucleus and the exterior outline of the head; and the sides of the tail were more brilliant than the central portions, conveying the impression that the bright nucleus was near the extremity of a nebulous tubular envelope with slightly divergent sides, and therefore approximating to a funnel shape. The growth of the tail now became exceedingly rapid; and the brilliancy of the head increased likewise, it being but little inferior to that of the stars composing the tail of *Ursa Major*. By the 21st September the light of the head exceeded that of any star in *Ursa Major*, and closely approached that of *Procyon;* while the tail was 8° long, and curved like a scimetar, a characteristic which it preserved during the remainder of its appearance.

On October 1st the nucleus had become brighter, but smaller, with projecting wings or jets of light sweeping backwards: while on October 2d a fine dark division in the tail was apparent, reaching from the head for about a degree in length, which subsequently became wider and larger. On October 1st, the tail was 21° in length; on October 5th, 28°; and by the end of the week, on October 9th, fully 36° or 38°, as measured by the declination-circle to the point where the eye estimated its appearance to cease.

The following micrometrical measures of the position of the comet with respect to *Arcturus* were made on the evening of the 5th of October:—

G.M.T.	Δ R.A. Comet W. of Star.	Δ Decl. Comet S. of Star.
h m s	′ ″	′ ″
7 1 15·6	13 37·4	
7 9 14·6	11 44·7	
7 9 44·0		19 12·4
7 16 13·5		19 7·2
7 16 53·0	10 41·5	
7 21 42·6	10 20·4	
7 22 10·0		20 13·4
7 53 7·4	2 30·9	
7 53 40·0		20 14·3

The observations were made with a telescope of 3¼ inches aperture, the powers ranging from 20 to 173.

Observed at York, by W. Gray, Esq.

The accompanying sketches of the comet show what particularly impressed me; and though I am unable to draw, they yet may be of use in confirming the observations of better artists.

Sept. 30. The second stratum has its crescent prolonged on the following side.

Oct. 3. The first stratum is a sector of 240°, a dark well-defined space between the branches of the tail, a black spot distinct on the preceding side of the glowing nucleus, and on the second area I was under the impression that a similar spot, very faint, existed. (I did not see the latter spot on subsequent evenings, but the spot on the first area was always visible.)

Observed at Spalding, Lincolnshire, by F. Selby, Esq.

The observations were made with a six-and-a-half-foot refracting equatoreal, of five inches aperture, by Slater, of London. Owing to the obstruction offered by high buildings, the view of the comet was limited to an interval of time, varying from an hour and a half to half-an-hour each evening.

Sept. 13. A sudden and momentary emanation from the nucleus was remarked. At first it was supposed to be due to atmospheric causes; but from its recurring in precisely the same form, the author felt convinced that it was really attributable to a change in the nucleus.

Appearances of a similar nature continued to be observed during the visibility of the comet. The author arrives at the conclusion that the nucleus is a dense body, emitting igneous or gaseous emanations.

The Great Comets of 1811 *and* 1858.

The following extract of a letter from Admiral Smyth to the Editor, in reference to the great comets of 1811 and 1858 will be read with interest. It may be mentioned that the comet of 1811 was observed for several months by the gallant admiral while employed on active service in the Mediterranean:

"*In re* the magnificent comet, I have been closely attending to its fine figure; and am asked on various sides, as I had the advantage of having closely watched both, which I thought the most splendid in appearance, this, or that of 1811? Now, to my memory, which is very distinct, the palm must be given to the latter. As a mere *sight*-object, the branched tail was of greater interest, the nucleus with its 'head-veil' was more distinct, and its circumpolarity was a fortunate incident for gazers.

"But recollect that in these remarks, I mean nothing disrespectful to the *Donati*. On the contrary, with those exceptions, it is one of the most beautiful objects I have ever seen in the heavens. The head is certainly not so fully pro-

nounced as in that of 1811; but greatly its physical interest is increased by segments of light and a dark hollow, giving the aspect a resemblance to the gaslight called a bat's-wing. . .

"This dark line, or space down the centre of the brilliant phenomenon, not only had the direct tendency to strengthen the luminosity of the jets of light, in the manner observable in the burning of a wax taper, but also, on a fuller scrutiny of this singular characteristic, to recall its striking resemblance to the similar feature seen in water-spouts, and in the pillars raised in sand-storms which I have witnessed in North Africa."

There has been forwarded to the Apartments of the Society a drawing by Mr. De La Rue, on a 20-inch celestial globe, of the appearance presented by the comet at the Cranford Observatory on the evening of the 5th of October. The time to which the drawing corresponds is 8^h P.M., when the tail of the comet was passing over *Arcturus*. The drawing gives a good idea of the apparent dimensions and curvature of the tail. It is seen passing over ξ, ς, γ, λ, χ, ι, and θ *Bootis*, extending a little beyond the three last-mentioned stars. At its greatest breadth it extends from λ *Bootis* to a small star, *h* 2, in the same constellation. The tail, roughly speaking, extends from 20° to 56° of north declination, measuring about 36° on the hour-circle passing through *Arcturus*.

This drawing was executed by Mr. De La Rue before proceeding to Russia, whence he intends shortly forwarding the results of his observations.

Observed at Carbost, Isle of Skye (Lat. 57° 16′ 32″; Long. 6° 22′ 40″ W.), by Lieut. Chinmo, R.N.

This paper contains a delineation of the position of the comet, and of the length and curvature of the tail relatively to the principal stars of *Ursa Major*. The time of observation is, September 30, 9^h 10^m P.M. The place of the comet was laid down by means of sextant observations of its distance from α and η *Ursæ Majoris*. The scale employed is two-tenths of an inch to a degree. The tail of the comet is seen extending to ι *Ursæ Majoris*. The curvature is very well shown. The author has also given a delineation of the position of the comet relatively to the constellation of *Ursa Major* for the evening of October 5 (6^h 39^m P.M.), when it approached very near to *Arcturus*. The stars to which the comet was referred were α and η *Ursæ Majoris*.

Note on Comet V. 1858. By W. R. Grove, Esq. F.R.S.

The following extract from a letter addressed by Mr. Grove to Mr. De La Rue refers to the phenomena observed during the transit of the tail of the comet over *Arcturus* on the

evening of the 5th of October. The telescope employed by Mr. Grove was a small instrument of only two inches aperture, and his remarks are offered under an impression that the weather was not generally favourable for observing the comet on the interesting occasion to which they refer:—

"When the comet had entered well within the margin of the tail a dark notch was formed cutting out a portion of the tail round the star; and as the star got further in, this became a dark areola surrounding the star, and in diameter equal to about one-tenth of the line of transit. This continued until the star reached the middle; at this part there is a broad dark line which extends from the nucleus to a distance considerably above the point where the star crossed. When *Arcturus* arrived here, this dark space was perfect up to the star, but on the other side the white light of the tail appeared to come quite up to the star; in short, as the bright part of the tail had been darkened in the vicinity of the star, the dark part was brightened, at least so much of it as was on the side furthest from the nucleus.

"I saw the notch again on the opposite side previous to emersion, and then lost it by clouds.

"The effects I have described are, doubtless, optical, and the notch and areola evidently due to the bright light of this star: the effect on the dark central part is not so easy to explain."

Admiral Smyth has forwarded a series of interesting drawings of the comet by Baron De Forester, representing its appearance as seen to the naked eye at Oporto on various evenings included between September 26 and October 15. The drawings are carefully executed upon a dark ground; and as the stars in the vicinity of the comet are laid down, they serve to give a good idea of the varying *apparent* dimensions, and curvature of the tail.

Thoughts on the Formation of the Tail of a Comet.

By J. J. Waterston, Esq.

Any attempt to account for the formation of the tail of a comet upon exact physical principles would be assisted, if, in the drawings of the telescopic appearances of the nucleus and adjacent coma—which are sometimes provided when circumstances are favourable—we were presented with the projection on the plane of vision of the radius vector, and of the tangent to the orbit. In some cases it might be possible to take such observations as would fix the actual curve of the *exterior line* of the tail, referred to the radius vector, passing through the

centre of the nucleus; *i.e.* supposing the central axis of the tail to be in the plane of the orbit. This line is very marked in the front side of Donati's comet, and might probably be easily fixed by the equatoreal.

If we view the tail as composed of molecules as free from the force of cohesion as the molecules of an uncondensable gas, and raised from the nucleus by the heat of the sun; and that that heat as it strikes upon each molecule is converted into a force centrifugal, that not only effectually counteracts the force centripetal of the sun's gravity, but that greatly exceeds it; such molecules will be quickly removed from the feeble attraction of the nucleus, and assume the motion of bodies entirely free from its influence. This hypothesis is suggested by the mechanical theory as a possibility.

If we inquire as to the centrifugal power of the sun's rays, we may, with the data afforded by modern research, easily compute the accelerative force it is capable of engendering on a single chemical molecule, if their whole heating power were converted into such a force. This is surpassingly great—no less than 800 miles per second! The data are, value of sun's radiation in a solar day equal to 1·8 ft. thickness of ice melted; the mechanical equivalent of which is about 13 lb. raised 1 foot high per second by the heat impinging on a square foot. If this heat impinges on a superficial foot of gold-leaf, one molecule thick (about one 200-millionth of an inch, as deduced from the relation of *capillarity* to *latent heat*, both being the measure of liquid cohesion, the first that of the superficial stratum of molecules, the second that of a cubic mass of molecules), and is converted into an impulsive force in one direction, it would in one second communicate a velocity of 800 miles a second; and in about four minutes a velocity equal to light itself. It is, of course, an extreme case to suppose the whole heat absorbed and converted; but it seems right to have in view the quantitative elements of the question. While the earth's atmosphere absorbs heat from the sun's rays, we have reason to suppose that heat-vibrations are converted into rectilineal velocity of gaseous molecules; but there is no reason to suppose that the impulse is in the specific direction *from* the sun, but equally *from* and *to*, as in vibratory impulses generally. It may be shown that the mechanical equivalent of the solar heat that impinges on the earth amounts to one-hundredth of its gravity towards the sun; so that, if any part of it acted centrifugally, the orbit would be disturbed. The same remark applies to a comet; but it cannot be denied that the phenomena of the tail, more especially as it turns sharp round in the perihelion passage, are such as require for their explanation a very active exhibition of such a centrifugal force as might be engendered by the conversion of heat acting upon single and free molecules.

Oct. 7, 1858.

Report of the Commission sent by the Brazilian Government to Paranagua to observe the Total Eclipse of the Sun of Sept. 7, 1858, Communicated to the Society by order of the Emperor of Brazil.

(*Abstract.*)

This Report, which has been forwarded to the Society through the Hon. William Stuart, Her Majesty's Chargé d'Affaires at the Court of Brazil, is a translation in the French language from the original. It extends to 44 pages of folio MSS. divided into sections, as follows:—

1st section, historical and introductory.

2. Observations of times of contact, exterior and interior.

3. Notes on the passage of the moon over the spots and faculæ on the sun.

4. Notes on the visibility of the moon beyond the edge of the sun.

5. On the colours of the sky, the sea, and of terrestrial objects, during the obscuration.

6. On the state of the limb of the moon, and on Baily's beads.

7. On the intensity of the light of the sun near the limb.

8. On the intensity of the atmospheric light during the totality.

9. On the corona.

10. On the protuberances.

11. Photographic observations.

12. Measures of the distances of the cusps.

13. Meteorological observations.

14. Effect of the eclipse on men and animals.

The commission was appointed by the Emperor of Brazil, at the instance of M. de Mello, director of the observatory at Rio, and was ordered to repair to Paranagua as a central station. The party consisted of M. Oliveira, M. de Mello, director of the observatory, M. Liais, sent out specially by the French Government, and four naval officers on the staff of the observatory, and had a steam-corvette, the Pedro II., placed at their service. This expedition left Rio about seventeen days before the day of the eclipse, and soon after its arrival at its destination, was increased in force by several officers of the corvette who were detached for the special service. Four stations were selected, a chief central one at the country-house of Dr. Reichsteiner, the Swiss Consul, on the sea-shore near Paranagua, a second on board the corvette at sea, and two others inland at the distances of 12 and 8 leagues, situated the one near the south boundary and the other near the north boundary of the shadow.

The weather seems to have been very unpromising, and rain to have been so frequent previously to the 7th of September, that little hope was entertained of success; but in the

event, the excellent arrangements made were not frustrated by this cause.

The position of the shore station was found to be 29° 30′ 33″·2 south latitude, by 48° 26′ 59″ west of Greenwich. At this station there remained M. de Mello with the telescope of the equatoreal of the observatory of Rio mounted parallactically; M. Oliveira provided with an equatoreally mounted comet-seeker; M. Liais, with four parallel telescopes on an equatoreal mounting common to them all, and several other observers with instruments of various kinds. The dimensions of the telescopes employed are, however, not specified with as much fulness and particularity as is desirable.

2. The first exterior contact having been lost by the intervention of cloud at the central station, and the second interior contact having been also lost by the observers being taken by surprise, the remaining contacts noted are passed over here and left for future publication.

3. The solar spots were observed and sketched at Rio on several days preceding the eclipse; but the results are not given in the report. Much change of form and number is alone remarked. In this section the principal point noticed is that by M. Liais, who as the moon advanced over a considerable spot, noticed with a power of 300, a slight flattening of the spot in the direction parallel to the moon's limb, which appears to correspond closely with the effect noticed lately in this country during an occultation of *Jupiter*.

4. In the early stage of the eclipse, both M. de Mello and M. Liais were able to trace the contour of the moon to the distance of from 4′ to 8′ beyond the sun's limb; but the following are far more curious remarks under Section 4. With one of M. Liais's telescopes having an aperture of 3 inches, the image was observed by projection on a plate of ground glass, and the image of the moon was thus seen very distinctly, and remarked to be *whiter* than the neighbouring region of the sky, an appearance which lasted for some time, then became less marked, and at last not distinguishable. Some photographs of this appearance were obtained by M. Liais by the dry process on glass collodionised and albumenised, in which the effect was distinctly traced, while the plates were still wet on coming out of the gallic acid bath. M. Liais adds some remarks in explanation of the curious circumstance that both in the photographic negatives and in the projected positive image, the same effect was witnessed. The reader must, however, be referred to the original accounts for this and other matter of value which here follows.

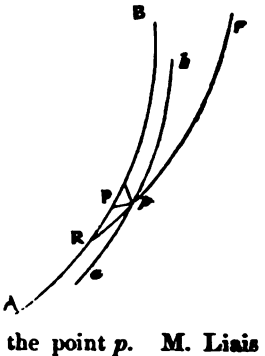
6. M. de Mello and M. Liais were unable to perceive any mountainous irregularities in the last remaining portion of the moon's limb, or on the part which first appeared on the sun after the greatest phase, yet M. de Mello states that in spite of this circumstance, and every care in adjusting for distinct vision

of the sun's limb, the phenomenon of Baily's beads presented itself to him on both the disappearance and reappearance of the sun. His telescope had 4 inches aperture and magnified 72 times. M. Liais observed these occurrences by projection, and detected no beads or irregularities.

8. At Paranagua *Venus*, *Mercury*, *Saturn*, *Sirius*, *Canopus*, and three other stars, supposed to be α and β *Centauri* and α *Crucis*, were seen during the totality; but M. de Mello could not trace *Regulus* on the meridian, although previously provided with its position. Numerical results by Rumford's photometer of the degrees of obscurity are added.

9. The corona appears by the accounts of all the observers to have presented on this occasion an unusually complicated appearance; and improbable as the details of the sketch which was sent with the account appeared at first sight, they appear to be well made out and established by the concurrent evidence of several individuals. More than three independent observers agree in testifying to the appearance of 5 distinct brushes of light reaching to an average distance of about 13′ from the moon's limb, of the form of a cone with convex sides, the base equal to two-thirds the height, resting on the moon. Of these four were in their general directions normal to the moon, two on each side the vertical, and the fifth oblique, bent upwards and having its base nearly coincident with the lower normal cone on the east or apparent right-hand side. In addition to these were two brushes of white light in a strictly radial direction, one on the right, the other on the left of the vertical, and both starting out below the horizontal diameter of the moon, with a thread-like texture; and on the east or apparent right-hand side of the moon an emanation of feebler light of the general outline of a parabola with its apex in contact with the dark limb. Description cannot entirely stand in the place of a figure, and the reader must necessarily be referred to a more detailed account which may probably be published hereafter. The light of the corona forming the ground on which these brighter portions were seen projected was remarked to be exceedingly unequal and patchy, and in many places to present the appearance of coarse rays crossing and intersecting in all directions, and in no part offering any exterior boundary sufficiently defined to have its termination submitted to measurement. An observation was made by M. Liais bearing on the relation of the conical brushes of light to the sun, which must be given nearly in his own words—"7 or 8 seconds after the commencement of the total obscuration, I fixed my attention on the east side on the bundle of rays forming the apparent lower edge of the oblique cone turned upwards, and which set out from the limb of the moon nearly tangentially. My attention was directed during from 15 to 20^s to this bundle of rays, and on a white protuberance having a black border very near to which it passed. One of the rays in particular touched the extremity of

this protuberance, and continued backwards arrived at the edge of the moon at a distance of about 2 degrees. I witnessed this distance diminish little by little, the ray all the while remaining in contact with the extremity of the protuberance and contrasting by its brilliancy with the black border of the latter. I watched the bright (white) portion of the protuberance gradually disappear behind the moon till there remained only a minute black point which vanished about 3 seconds later. The point of intersection of the ray in question with the moon was at that instant exactly coincident with the point at which the apex of the protuberance, which had then the appearance of the summit of a lunar mountain, disappeared." A simple figure may assist this description. A R P R being the dark edge of the moon 7ˢ or 8ˢ after commencement of totality, P *p* the protuberance with a white centre and black outline, R *p r* the well-defined tangential ray of the corona, the moon's edge was seen to intersect the ray at all points between R and *p*, as it advanced from the position A R B to *a p b*, when the protuberance was finally covered up, and the ray was seen to intersect the limb at the point *p*. M. Liais argues that previous observations having connected the protuberances with the sun, and this observation indicating that the corona ray remained consistently stationary as referred to the protuberance, we may conclude that the cone of corona light of which this ray formed a part, also was an appendage to the sun and not to the moon, and by parity of reason may conclude the same for the rest.



10. Several protuberances were also seen during the totality, but they are described as having been of a white colour, and only in one instance tinged with pink. At the commencement of totality three protuberances were found on the sun's east limb, the angles of position of which counted from the lowest point (query nadir) were found by M. Liais *with an equatoreal* having a divided interior position circle, to be 45°, 105°, and 135° respectively. The two first were vividly white, with a black outline. On the west limb measuring, as before, from the lowest point of the disk, there were seen two others, at the angles 110° and 170°, in which a pink tint was noticeable. In a short time the protuberances on the east side were covered over, and a third one emerged on the west side (making six in all), at the angle 60°. The height of the principal protuberance on the west limb (the one situated at 110°) was twice measured by M. Liais by comparison with a fixed scale provided in the field of this telescope and estimated to be 58″ at about 8 seconds after

totality commenced, and 1′ 18″ towards the end, but these values, though probably approximate, are but estimations without the strictly corresponding times. At one of the inland stations, at Campinas, the protuberances at the angles 110° and 60° were seen to be connected by a low dentelated bank of pink light, which was not uncovered to the observers at the central station. These two protuberances with the connecting fringe appear to agree with those in the naval account from Payta on the west coast, with some discrepancy in the angular situation. There appears to be a disagreement between the indications of the letters N E S O on the sketch from Brazil, and the statement in this section that M. Liais's angles were reckoned from the lowest point of the disk. It would appear on examination that though the observers at the central station had three equatoreally mounted telescopes, the only angles read off were referred in reality to the vertical, and that the point marked north on the drawing must be taken for the true nadir point. On applying the correction 24° for the angle between the south pole and the zenith at the sun's centre, we then, from M. Liais's statement, derive the following position-angles for the 6 protuberances seen at Paranagua; 69°, 129°, 159°, 214°, 274°, and 324°, reckoned from N. round by E., the low band of pink light connecting the two last. Similarly correcting the angles read off from the sketch given in M. Le Verrier's bulletin of October 21, by the angle 82°, there result the position-angles 77°, 92°, 257°, and 342°, for the protuberances seen on the west coast, the two latter being connected by a bank of pink matter. Comparing these with the former, the 77° answers to the 69°, and the continuous bank from 257° to 342° with elevated terminations corresponds to M. Liais's bank from 274° to 324° similarly characterised. The angles of M. Liais are probably somewhat more exact than those observed on board the French vessel on the west coast, but they do not encourage one to enter upon any nice comparison with the angles of position of the solar spots and faculæ. Taking them as they here stand, there would appear to be no sort of connexion, which is also M. Liais's conclusion in the Report.

11. The next section is given to an account of the means used for obtaining photographs of the disk, and of the results deduced. The length to which this abstract has already extended precludes the statement of more than that 15 photographs were taken, 9 before the totality and 6 after, of which one failed to develope, and two others were spoiled by the instrument's slipping. The positions of the cusps exhibited in this series of photographs enabled M. Liais to conclude that at the intended central station, the centres of the two bodies passed within 1″·5.

The remainder of the account, consisting principally of meteorological details, may be left for future and more sufficient publication.

Note on the Figure of the Earth. By Captain Clarke, R.E.

The geodetical operations executed in India extend from 8° 10′ north latitude to 29° 30′; those in Europe from 38° 40′ to 70° 40′. The hitherto existing gap of nine degrees is now half filled up (though in the opposite hemisphere) by the valuable results of Mr. Maclear's measurement at the Cape, given in the last number of the *Monthly Notices* of the Royal Astronomical Society. I propose to examine the effect of this new arc in modifying the results obtained for the figure of the earth at page 771 of the *Account of the Principal Triangulation of Great Britain and Ireland*, referring to that work for the method of calculation. If we assume for the radius of curvature of the meridian the expression

$$\rho = A + 2\,B\cos 2\lambda + 2\,C\cos 4\lambda$$

the condition $5\,B^2 - 6\,A\,C = 0$ makes the curve an ellipse; let

$$\frac{1}{A} = \frac{1 + \frac{u}{10000}}{20890000}$$

$$-\frac{2\,B}{A} = \frac{1}{200} + \frac{v}{10000}$$

then if x be the necessary correction to the latitude of the north end, the corrections to the latitudes of the other stations will be as follows:—

Kamies Sector Berg..	$-8{\cdot}213 + 0{\cdot}2224\,u + 0{\cdot}1120\,v + x$
Heerenlogements Berg	$+0{\cdot}325 + 0{\cdot}8032\,u + 0{\cdot}5833\,v + x$
Royal Observatory ..	$-0{\cdot}676 + 1{\cdot}5104\,u + 0{\cdot}6746\,v + x$
Zwart Kop	$+0{\cdot}923 + 1{\cdot}6155\,u + 0{\cdot}7141\,v + x$
Cape Point	$-0{\cdot}229 + 1{\cdot}6608\,u + 0{\cdot}7508\,v + x$

From these numbers are obtained by multiplication the following,—

$(a\,m) = -1{\cdot}4757$	$\frac{1}{2}(a^2)(m) = -7{\cdot}6238$
$(b\,m) = -0{\cdot}7595$	$\frac{1}{2}(b^2)(m) = -3{\cdot}4297$
$(a^2) = +8{\cdot}3440$	$\frac{1}{2}(a^2)^2 = +5{\cdot}6305$
$(a\,b) = +3{\cdot}1190$	$\frac{1}{2}(a^2)(b^2) = +2{\cdot}5330$
$(b^2) = +1{\cdot}6585$	$\frac{1}{2}(b^2)^2 = +1{\cdot}1395$

The values of M A, M B, &c., (the differences of the above) resulting from this arc are as follows—the second line showing the values of these quantities for all the previously measured arcs:—

M A	M B	A²	A B	B²
+ 0·1481	+ 2·6702	2·7135	+ 1·1860	0·5190
143·4540	− 72·4048	2820·0495	− 32·6817	56·3121

For the numbers in the second line see page 768 of the

work quoted above. Adding together the corresponding quantities in the two lines, the following equation results,—

$$0 = + 149{\cdot}6021 + 284{\cdot}7630\, u - 31{\cdot}4957\, v$$
$$0 = - 69{\cdot}7346 - 31{\cdot}4957\, u + 56{\cdot}8311\, v$$

If we write P and Q for the absolute terms of this equation, it becomes by elimination

$$0 = u + {\cdot}0037410\, P + {\cdot}0020733\, Q$$
$$0 = v + {\cdot}0020733\, P + {\cdot}0187450\, Q$$

The numerical values of u and v are,—

$$u = - 0{\cdot}4151\,;\ v = + 0{\cdot}9970$$

from which we obtain the following results,—

$$a = 20926364$$
$$b = 20855340$$
$$a : b = 294{\cdot}63 : 293{\cdot}63$$

These semiaxes differ only by 16 and 107 feet respectively from the values obtained at page 771 of the *Account*, &c. The corrections to the latitudes in the Cape arc are,—

North end	+ 1·280
Kamies Sector Berg	− 6·914
Heerenlogements Berg ..	+ 1·653
Royal Observatory	+ 0·649
Zwart Kop	+ 2·244
Cape Point	+ 1·090

Mr. Maclear states that the position of Kamies Berg station is such as fully to account for this large deflection. Let us therefore suppose it ascertained that the form of the adjoining country produces a deflection of n'' northwards, and trace the consequent effect upon the above values of a and b. The increments of $(a\ m)$ and $(b\ m)$ are,—

$$+ {\cdot}2224\, n, \qquad + {\cdot}1120\, n,$$

and of $\frac{1}{6}(a)\ (m)$ and $\frac{1}{6}(b)\ (m)$ the corresponding increments are,—

$$+ {\cdot}9687\, n, \qquad + {\cdot}4358\, n$$

and the increments of P and Q will be the differences of these, or,

$$- {\cdot}7463\, n, \qquad - {\cdot}3238\, n$$

from which

$$\delta u = + {\cdot}0035\, n : \delta v = + {\cdot}0076\, n$$

and finally $\delta a = -2{\cdot}0\,n$; $\delta b = -12{\cdot}6\,n$. Let $n = 7$ then $\delta a = -14$; $\delta b = -88$. Applying these alterations to the values found for a and b, there result,

$$a = 20926350$$
$$b = 20855252$$
$$a : b = 294{\cdot}33 : 293{\cdot}33$$

These semiaxes exceed those given at p. 771 of the *Account*, &c., by 2 and 19 feet respectively. We have, therefore, this interesting result, that the very slight modification in the elements of the earth's figure and dimensions due to the arc at the Cape is entirely owing to the extraordinary disturbance of the vertical line at Kamies Berg.

On the Development of the Disturbing Function in the Lunar Theory. By A. Cayley, Esq.

The development of the disturbing function for the lunar theory is effected in a very elegant manner in Hansen's *Fundamenta Nova*, but a further step is requisite to exhibit the result in a perfectly explicit form and to compare it with those of other geometers. To do this is the immediate object of the present memoir, the mode of development being a reproduction of that made use of by Hansen. The results are compared with those obtained by Lubbock and Pontécoulant. But the memoir is written with a view to the development and application to the lunar theory of the theory contained in the author's "Memoir on the Problem of disturbed Elliptic Motion," and the notation is consequently that of the memoir just referred to.

The solar eclipse of Sept. 7, 1858, was observed at the Isle of St. Croix, Danish West Indies, by Major Lang. The last contact occurred at $21^h\ 9^m\ 18^s$ local mean time. The sky was free of clouds, but the direct heat of the sun's rays, about 130°, was almost insurmountable. No appearance of Baily's beads nor any other remarkable phenomenon. The atmosphere pure and clear. A very large spot with an extensive cluster on the sun's south-west quadrant, the whole length of which the day before was computed to be about 75,000 English miles.

The following note is by Mr. Williams, the Assistant-Secretary of the Society:—

"This morning, about twenty-five minutes past eleven, chancing to look at the sun, which was at that time greatly obscured by a fog, I distinctly saw without optical assistance

what appeared to be two spots very near each other, a little below the equator of the sun towards the west. Upon applying the telescope my conjecture was verified, two large spots appearing in that place and another above the equator towards the east. This last I was unable to perceive without the telescope. There were also five or six smaller spots visible in the instrument.

"*Nov.* 10, 1858."

Note on the Nomenclature of the Minor Planets.
By J. R. Hind, Esq.

A few months since my attention was directed by Sir John Herschel to the inconvenience and confusion which are being gradually introduced into the nomenclature of the minor planets by the acceptance of names, easily mistaken either in speaking or writing for others belonging to planets previously discovered. I have been fully sensible of the liability to error or misapprehension thereby induced, and am desirous of recording a protest against any further continuance of what must eventually become a positive nuisance to those who are more particularly occupied with the observations and calculations bearing upon this numerous group of planets.

Thus we have already:—

Thetis, Themis; Lutetia, Lætitia; Iris, Isis; Vesta, Hestia; Pallas, Pales.

It will naturally be the wish of every discoverer of a planet that his *enfant trouvé* should be known to posterity by the name which it has borne during his lifetime; but if the practice to which allusion is here made be suffered to continue much longer, there is certainly a probability that a day will arrive when, for the sake of their general convenience, astronomers will consign these troublesome names to oblivion, and substitute others less liable to engender confusion. This consideration alone, we might suppose, would prove sufficiently powerful to induce hesitation on the part of the discoverer before accepting any name likely to be objected to on the score of similarity with that of a planet previously found.

Nov. 11, 1858.

Letter addressed to the Secretary by Sir J. F. W. Herschel. Read and published by his request.

"In the fourth edition of my *Outlines of Astronomy*, p. 337, in which the discovery of *Hyperion* (*Saturn's* eighth satellite) is mentioned as there recent, the discovery is attributed to Mr. Dawes and Professor Bond, without any mention of Mr. Lassell,

in whose observatory and with whose instruments, by the joint observations both of Mr. Lassell and Mr. Dawes, the discovery was made.

"The passage was written very hastily, in the absence of any possibility of reference to books or letters, and under the urgency of very pressing public business, and has, unfortunately, been passed uncorrected in the new edition of that work recently published. My attention having been drawn to the point by a mutual friend of all the parties, I think it due to them, as well as to the Society to which I have the honour to belong, to offer this explanation of what must otherwise appear a singular piece of injustice."

There were exhibited at the Meeting a series of stereoscopic photographs of the moon, which had been executed in the first instance by Mr. De La Rue, and were subsequently enlarged by Mr. R. Howlett to eight inches diameter from the original negatives. The stereoscope in which they were viewed was manufactured by Messrs. Smith, Beck, and Beck.

The instrument was of similar construction to Wheatstone's reflecting telescope; but the objects being transparent, the usual arrangements and adjustments were considerably modified. Prisms with slight curvatures worked on the surfaces were employed instead of mirrors for combining the pictures. The photographs were capable of being resolved; and the illumination was effected by Tyler and Son's new Belmontine lamps, large mirrors being provided for daylight.

The appearance of rotundity over the whole surface of the moon is perfect; and parts which are as plane surfaces in the single photograph in the stereoscope present the most remarkable undulations and irregularities.

By desire of Mr. De La Rue, the instrument is now on view at Smith, Beck, and Beck's, 6 Coleman Street, where it can be seen through by any one interested in the subject.

RECENT PUBLICATIONS.

The *Nautical Almanac* for 1862 has recently been published. The usual Supplement containing the ephemerides of the minor planets for the proximate year, appears in a separate form, the Lords Commissioners of the Admiralty having ordered that the volume itself shall be published more in advance than hitherto. By this arrangement the Supplement is published a little later than the volume, but purchasers of the latter may obtain the former by applying any time before the end of 1859, at the *Nautical Almanac* Office, 3 Verulam Buildings, Gray's Inn, London.

Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the year 1856, under the superintendence of Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XVII. Oxford, 1858.

This volume commences as usual with the observations made with the transit instrument and the meridian circle. Then follows a catalogue of the stars observed in the year 1856, showing the result of every observation. The number of stars is 1368. The astronomical part of the volume closes with a provisional catalogue of 1481 stars, deduced from the observations made at the Radcliffe Observatory during the years 1854–5–6, reduced to 1860·0. This catalogue, to which allusion has already been made in the *Monthly Notices*, is designed to comprehend all objects in the heavens beyond the solar system which are supposed to be affected by some physical peculiarity, or to which other circumstances lend more than ordinary importance. In its present form the Catalogue is not a completed work. It merely represents the progress of the undertaking down to the year 1856. The following are the classes of objects which in its ultimate form it is intended to include:—

1. Stars above the third magnitude.
2. Stars situated within 6° of the north pole, which can be conveniently observed with telescopes of 4-inch aperture.
3. Stars which are, or are supposed to be, variable, or which present unusual appearances of colour.
4. Stars which are, or are supposed to be, affected by proper motion exceeding 0″·1 of the great circle.
5. Double stars known to be affected by orbital motion.
6. Stars of whatever magnitude contained in the standard catalogue of the *Nautical Almanac*, as well as those whose places are used for comparison with the moon.

Mr. Johnson has appended to the present provisional Catalogue a list of stars which differ more than 0ˢ·5 in R.A., and 4″ in N.P.D. from those in the British Association Catalogue. It is plain from the notes which follow this list that the discordancies are in most instances attributable to an erroneous proper motion adopted in the construction of the British Association Catalogue, resulting generally from a comparison of only two determinations.

The Catalogue contains a table of variations and epochs of the variable stars, compiled by Mr. Pogson from his own observations, and from the publications of other observers.

Determination des Orbites des Planètes et des Comètes. Par A. J. Yvon Villarceau.*

The author first gives a general explanation of the method for determining the orbit of a planet or comet founded upon

* "Extrait des Annales de l'Observatoire Impérial de Paris," tome troisième.

employing no more observations than the number which is mathematically necessary for the purpose. He then gives a similar exposition of the method employed by himself, which consists in obtaining expressions in a converging series depending on the time, for the geocentric longitude and latitude of the planet or comet, founded on an indeterminate number of observations made during a short interval of time; and then, by means of those expressions, and the results of their successive differentiation, combined with the differential equations for the motion of a body revolving in a conic section, determining the heliocentric distance, and the velocity and direction of motion of the body at a given instant, these being the fundamental circumstances of motion from which the elements are readily deducible.

The principle of this method, it will be seen, is the same as that used by Laplace in his solution of the same problem, but M. Villarceau has pointed out and skilfully discussed the various circumstances which render a modification of the form of solution indispensable, and has given convenient formulæ applicable to each particular case. He has also shown how the elements may be rectified by a process of approximation founded on applying successive corrections to the coefficients of the expressions for the geocentric longitude and latitude, the values of these corrections being obtained by the solution of a system of linear equations.

The author, at the close of his investigation, gives numerical applications illustrative of several cases of the problem considered by him.

Descrizione del Nuovo Osservatorio del Collegio Romano D.C.D.G., E Memoria sui lavori eseguiti dal 1852 *a tutto Aprile* 1856, *del P. Angelo Secchi, D.M.C., Roma,* 1856.

This volume commences with an historical account of the Observatory of the Collegio Romano. The author then gives a description of the New Observatory, and the principal instruments with which it is furnished. These consist of a meridian circle by Ertel, two feet three inches in diameter, a small transit-circle by Reichenbach, a refractor equatoreally mounted, by Merz, having a clear aperture of nine French inches, and a sidereal clock by Dent. Professor Secchi contemplates devoting the resources of the observatory to the advancement of stellar astronomy. For this purpose the great equatoreal chiefly claims his attention. This fine instrument was first erected in the new observatory in the month of October 1854, and has already done good service in the hands of the distinguished Director of the establishment.

The second part of the volume is devoted to an account of the observations made during the period referred to in the title

of the work. The first section contains a series of micrometrical measures of double stars with the great refractor, and a comparison of the results with those of other observers.

Then follow, in the second and third sections, observations of nebulæ and clusters of stars, with remarks upon the zodiacal light. The fourth section is devoted to micrometrical measures of *Saturn* and his rings, and the fifth to physical observations and micrometrical measures of *Jupiter* and his satellites. In the sixth section the author determines the latitude of the observatory, which he finds to be 41° 53′ 53″·72. The seventh section contains miscellaneous observations on the spots and temperature of the sun, on selenography and on the colours and scintillation of the stars. The eighth and ninth sections are devoted to meteorological and magnetical observations. In an appendix the author has inserted some cometary and planetary observations; also additional measures of *Saturn* and observations of the planet *Mars*.

Zweijahrige Beobachtungen der Meisten jetzt Bekannten Veränderlichen Sterne. Von J. A. C. Oudemans.*

In this paper Dr. Oudemans gives the details of observations of most of the known variable stars, made by him at the Observatory of Leyden during the years 1854 and 1855, and part of the years 1853 and 1856. The method employed by him, which is due to Argelander, is founded on a series of comparisons of the light of the variable in each instance with that of certain stars in its vicinity. The paper contains a discussion of the observations of *β Lyræ* and *δ Cephei*, and the deduction of the definitive results with respect to the epoch and period of each star. The author has also given a graphical representation of the variations of the light of each star, and has instituted a comparison between his results and the corresponding light curves of Argelander and Heis.

* Verhandelingen der Koninklijke Akademie van Wetenschappen, Derde Deel, 4to., Amsterdam, 1856.

ERRATUM.

Vol. XVIII. No. 5 for March, 1858, page 140, in the Apparent N.P.D. of *Irene*, Jan. 28, *for* 77° 20′ 36″·84, *read* 77° 20′ 28″·83.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *December* 10, 1858. No. 2.

Dr. LEE, V.P., in the Chair.

The Rev. Adam Storey Farrer, Oxford;
Wm. Pearce Poole, Esq., Trinity College, Dublin; and
Thomas Slater, Esq., 136 Euston Road,
were balloted for and duly elected Fellows of the Society.

The Astronomische Nachrichten.

It may be well to mention for the information of such of the Fellows of the Society as may contemplate subscribing to this valuable periodical, that a new volume (the fiftieth) will shortly commence. The price per volume is fifteen shillings, payable in advance. Two volumes are generally published in the course of a year. Gentlemen desirous of subscribing are requested to send an intimation to that effect to Mr. Williams, Assistant-Secretary of the Society.

Letter from Mr. Maclear to the Astronomer Royal.

"I herewith send the observations made on D'Arrest's comet. By the same post I forward a copy of them to M. Villarceau of the Paris Observatory, who wrote to me respecting them.

"It was a very faint object, partly owing, perhaps, to the line of sight being through the Kloop that separates the Lion's Head from the Table Mountain. When I detected it all light had been excluded from the equatoreal room for some fifteen minutes, otherwise perhaps it might have escaped; and when occasionally I looked during the series of observations afterwards, the image was never brighter than at first. Making due allowance for the low altitude, and the direction along the north edge of the Table Mountain cap, my impression is, that in the search for it hereafter the object-glass should not be less than six or seven inches."

Underneath are the observations referred to in the foregoing letter:—

Right Ascensions and North Polar Distances of D'Arrest's Comet observed at the Royal Observatory, Cape of Good Hope. By T. Maclear, Esq.

Date.	No. of Star.	Cape Mean Time.	Difference in R.A.	R.A. of Comet.	Log $\frac{p}{P}$	No. of Obs.
1857.		h m s	m s	h m s		
Dec. 5	1	8 59 18·7	−0 25·97	19 51 35·34	+8·7743	1
7	2	8 42 9·3	+0 5·77	20 0 19·60	·7736	13
8	3	8 51 43·6	+0 47·66	20 4 44·96	·7741	12
9	4	9 7 46·0	+0 9·00	20 9 10·62	·7732	3
10	5	8 37 9·0	+0 15·68	20 13 30·22	·7724	10
		8 57 43·3	+0 19·11	20 13 33·65	·7737	10
14	6	8 38 51·3	+0 39·49	20 31 4·04	·7715	5
		8 51 55·8	+0 41·81	20 31 6·36	·7727	5
15	7	8 38 50·3	+0 32·93	20 35 26·23	·7711	8
		9 3 39·2	+0 37·55	20 35 30·85	·7724	8
18	8	8 40 0·7	−0 9·41	20 48 32·04	·7701	10
	9	8 58 3·5	−0 19·38	20 48 35·34	·7716	10
20	10	8 42 4·8	−0 30·27	20 57 12·46	·7695	6
		9 14 23·5	−0 23·98	20 57 18·75	·7701	2
21	11	8 40 34·4	+0 17·09	21 1 31·67	·7690	10
		9 4 58·16	+0 21·71	21 1 36·29	·7704	8
22	12	8 37 39·5	+0 4·01	21 5 49·57	·7679	3
24	13	8 51 29·6	+0 41·46	21 14 26·07	·7689	8
1858.						
Jan. 4	14	8 43 19·5	−0 6·81	22 0 15·49	·7622	10
		9 4 55·3	−0 3·21	22 0 19·09	·7640	9
5	15	8 45 40·0	−1 5·09	22 4 17·76	·7619	6
		9 7 38·6	−1 1·21	22 4 21·64	·7634	3
7	16	8 43 13·6	−0 2·77	22 12 18·93	·7606	10
10	17	8 45 22·7	+0 38·95	22 24 10·22	·7595	8
		9 6 17·3	+0 42·34	22 24 13·61	·7608	6
12	18	8 44 14·1	+0 47·75	22 31 57·13	·7583	4
		8 53 52·8	+0 49·31	22 31 58·69	·7594	5
13	19	8 45 17·4	−0 54·68	22 35 48·43	·7576	4
		8 53 38·2	−0 53·73	22 35 49·38	·7587	4
15	20	8 41 37·1	+0 34·54	22 43 26·40	·7563	3
		8 48 45·2	+0 34·62	22 43 26·48	·7573	3
16	21	8 46 28·0	−0 39·67	22 47 12·89	·7564	4
		8 54 33·5	−0 38·13	22 47 14·43	·7573	4
17	22	8 45 23·1	−0 14·31	22 50 58·14	·7558	8
18	23	8 43 44·1	+0 30·84	22 54 42·13	·7552	4
		8 51 46·3	+0 32·31	22 54 43·60	+8·7562	4

Date.	No. of Star.	Cape Mean Time.	Diff. in N.P.D.	N.P.D. of Comet.	Log $\frac{q}{P}$	No. of Measures.
1857.		h m s	′ ″	° ′ ″		
Dec. 5	1	9 2 10·7	+ 1 58·04	111 16 34·8	+9·7233	1
7	2	8 29 45·2	— 1 52·48	111 12 40·3	·6866	10
		9 1 23·9	— 1 57·93	111 12 34·8	·7223	2
8	3	8 51 53·6	+ 0 53·78	111 9 58·8	·7122	12
9	4	9 4 53·5	— 9 23·51	111 6 52·2	·7253	2
10	5	8 48 13·5	— 7 37·49	111 3 25·4	·7070	10
		9 7 3·5	— 7 43·01	111 3 19·9	·7284	6
14	6	8 44 49·9	— 4 51·39	110 44 44·8	·7027	5
		9 3 31·7	— 4 58·04	110 44 38·1	·7230	6
15	7	8 50 57·9	— 5 3·27	110 38 55·6	·7092	10
18	8	8 49 5·3	— 8 3·23	110 19 0·8	·7061	10
	9	9 6 43·6	+ 7 4·76	110 18 54·1	·7247	6
20	10	9 5 30·8	+ 0 47·76	110 3 35·7	·7229	7
21	11	8 53 19·6	+ 0 52·01	109 55 19·4	·7111	10
22	12	8 47 52·4	+ 4 23·15	109 46 35·4	·7051	4
24	13	9 2 53·6	+ 7 40·44	109 27 57·0	·7222	5
1858.						
Jan. 4	14	8 55 9·6	— 1 44·81	107 21 18·6	·7168	10
5	15	8 57 52·1	—11 30·44	107 7 56·3	·7189	5
7	16	9 5 28·1	+ 4 14·65	106 40 22·7	·7274	3
10	17	8 56 23·3	— 0 11·91	105 57 30·1	·7220	6
12	18	8 44 24·3	— 5 17·70	105 27 49·6	·7138	4
		8 54 3·0	— 5 23·91	105 27 43·4	·7216	5
13	19	8 45 28·0	— 8 45·09	105 12 41·6	·7142	4
		8 53 48·6	— 8 52·67	105 12 34·0	·7208	4
15	20	8 41 47·6	— 6 52·18	104 41 48·4	·7143	3
		8 48 55·5	— 7 1·05	104 41 39·6	·7197	3
16	21	8 46 38·2	— 3 51·99	104 26 1·5	·7179	4
		8 54 43·9	— 3 54·65	104 25 58·8	·7240	4
17	22	8 52 32·2	— 3 33·45	104 10 5·6	·7235	4
18	23	8 43 54·4	+ 4 13·70	103 54 5·1	·7186	4
		8 51 56·6	+ 4 12·88	103 54 4·3	+9·7244	4

The comet's places are corrected for refraction, but not for parallax. p and q are the parallax corrections in seconds of time and arc respectively, and P is the horizontal parallax.

The foregoing observations were made by Mr. Mann with the 8½-feet equatoreal, to which was attached a position micrometer with stout silver wires. Power 90.

The comet appeared to be a very faint nebulous body subtending a diameter of about 1′¼; and was observed generally under very unfavourable circumstances, owing to its low altitude (from 8° to 16°), and the continued interruption caused by drifting clouds from the Table Mountain.

1857.
Dec. 5. The two observations made this evening are very rough.
7. Much annoyed by cloud; the observations uncertain.

Dec. 9. A few glimpses only obtainable through cloud; the observations very doubtful.
14. Much interrupted by cloud.
15. Cirrous haze in the west; the comet barely visible when taking the second set of transits.
20. Clear and calm, but the moonlight nearly obliterates the comet; it is detected with great difficulty.
21. Clear, but the comet barely visible, owing to moonlight.
22. Clouded; the few observations obtained this evening are very indifferent.
24. The faintest trace of the comet visible; after the last observation it was no longer seen.

1858.
Jan. 4. The comet is now remarkably faint, but the atmospheric circumstances are favourable, and the observations on the whole satisfactory.
5. Violent S. wind, the chronometer beat heard with difficulty; the second set of transits doubtful; further observations impracticable.
7. Much annoyed by cloud; the comet barely visible when the N.P.D. measures were taken.
10. The comet almost too faint for work; the second set of differences in R.A. very doubtful.
12 to 18. The comet of the last degree of faintness, and the observations in consequence very difficult.

Royal Observatory, Cape of Good Hope,
Oct. 6, 1858."

Mean R.A. and N.P.D. of the Stars compared with D'Arrest's Comet, 1857, 1858, and Apparent Places for the Day of Observation. By T. Maclear, Esq.

No. of Star.	Mag.	Mean R.A. 1858·0.	Annual Precession.	No. of Obs.	Mean N.P.D. 1858·0.	Annual Precession.	No. of Obs.
		h m s	s		° ′ ″	″	
1	9	19 52 2·42	+3·529	4	111 14 25·88	−9·42	5
2	10½	20 0 14·90	·520	4	111 14 21·78	10·05	4
3	10	20 3 58·36	·514	3	111 8 54·06	10·33	3
4	11	20 9 2·65	·511	3	111 16 4·72	10·71	3
5	9	20 13 15·56	·504	3	111 10 51·87	11·02	3
6	9½	20 30 25·49	·473	4	110 49 25·19	12·24	3
7	8½	20 34 54·22	·465	3	110 43 47·94	12·55	3
8	11½	20 48 42·31	·440	3	110 26 53·23	13·46	4
9	9½	20 48 55·58	·435	4	110 11 38·58	13·48	4
10	8½	20 57 43·55	·419	3	110 2 37·26	14·04	4
11	10	21 1 15·39	·411	3	109 54 16·78	14·26	3
12	11½	21 5 54·37	·400	3	109 42 1·69	14·54	4
13	7	21 13 45·36	·382	3	109 20 6·11	15·01	3
14	9	22 0 22·87	·279	3	107 22 54·10	17·38	3
15	8	22 5 23·40	+3·271	3	107 19 17·52	−17·60	3

No. of Star.	Mag.	Mean R.A. 1858·0.	Annual Precession.	No. of Obs.	Mean N.P.D. 1858·0.	Annual Precession.	No. of Obs.
		h m s	s		° ′ ″	″	
16	10	22 12 22·23	+3·251	4	106 35 59·17	−17·88	4
17	10	22 23 31·77	·227	4	105 57 33·47	18·30	4
18	9	22 31 9·85	·221	3	105 32 59·02	18·57	4
19	7	22 36 43·56	·201	4	105 21 18·58	18·75	4
20	7	22 42 52·29	·187	4	104 48 32·78	18·93	4
21	7½	22 47 52·98	·178	3	104 29 45·86	19·07	3
22	11	22 51 12·86	·171	4	104 13 31·53	19·16	3
23	9½	22 54 11·69	+3·164	3	103 49 44·08	−19·23	3

No. of Star.	Apparent Places. R.A.	N.P.D.	No. of Star.	Apparent Places. R.A.	N.P.D.
	h m s	° ′ ″		h m s	° ′ ″
1	19 52 1·31	111 14 36·77	13	21 13 44·61	109 20 16·52
2	20 0 13·83	111 14 32·74	14	22 0 22·30	107 23 3·43
3	20 3 57·30	111 9 5·02	15	22 5 22·85	107 19 26·74
4	20 9 1·62	111 16 15·74	16	22 12 21·70	106 36 8·09
5	20 13 14·54	111 11 2·89	17	22 23 31·27	105 57 42·01
6	20 30 24·55	110 49 36·15	18	22 31 9·38	105 33 7·30
7	20 34 53·30	110 43 58·88	19	22 36 43·11	105 21 26·69
8	20 48 41·45	110 27 4·06	20	22 42 51·86	104 48 40·61
9	20 48 54·72	110 11 49·35	21	22 47 52·56	104 29 53·50
10	20 57 42·73	110 2 47·96	22	22 51 12·45	104 13 39·03
11	21 1 14·58	109 54 27·43	23	22 54 11·29	103 49 51·41
12	21 5 53·58	109 42 12·25			

The above places are deduced from observations made with the transit circle.

Results of Observations of Small Planets, made with the Transit Circle, at the Royal Observatory, Greenwich, during the month of November 1858.

(*Communicated by the Astronomer Royal.*)

Egeria ⑱.

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1858, Nov. 11	8 40 45·6	0 3 32·59	102 6 54·76
12	8 36 (25)	...	101 59 36·82
18	8 11 44·3	0 2 2·41	101 12 49·48
22	7 59 40·1	0 1 45·88	100 39 11·69

Urania (30).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Nov. 11	9 8 45·3	0 31 36·83	82 43 19·26
12	9 4 39·0	0 31 26·42	82 45 34·14
18	8 40 41·7	0 31 4·52	82 55 18·44
22	8 25 22·9	0 31 29 47	82 57 50·11
23	8 21 37·8	0 31 40·30	82 57 59·61
26	8 10 35·0	0 32 25·33	82 57 18·67

Pomona (32).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Nov. 22	8 51 36·6	0 57 47·50	82 18 29·44

Lætitia (39).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Nov. 2	8 55 27·8	23 42 48·15	100 38 58·08
9	8 28 18·0	23 43 9·79	100 41 9·81
11	8 20 45·7	23 43 29·39	100 39 49·14
12	8 17 2·3	23 43 41·93	100 38 49·30
23	7 37 35·7	23 47 30·97	100 14 49·50
26	7 27 18·9	23 49 2·12	100 4 28·85

Ariadne (43).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Nov. 2	12 47 57·5	3 31 59·49	67 28 46·64
11	11 58 35·5	3 21 54·97	68 14 8·37
12	11 53 31·5	3 20 46·71	68 19 33·45
18	11 23 11·4	3 14 0·97	68 53 32·56
22	11 3 8·8	3 9 41·29	69 16 49·14
26	10 43 20·0	3 5 35·45	69 39 57·90

Searle's Planet (44).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1858, Nov. 18	8 15 50·1	0 6 8·90	87 7 20·07
22	8 0 38·0	0 6 40·52	86 54 10·83
26	7 45 52·2	0 7 38·53	86 39 1·34

Physical Observations of Jupiter. By Sir W. Keith Murray.

"I annex a sketch of *Jupiter*, showing the position and appearance of remarkable streaks visible below the southern principal belt since the 27th October last. They were first observed by me at $16^h\ 30^m$ G.M.T., on Wednesday 27th October; I have seen them several times since, but not well, till last night, at $9^h\ 48^m$ G.M.T., when they appeared to occupy the same relative position and proportions as when first observed. I have observed another detached spot, and also indications of several white spots, all on the southern hemisphere, but the atmosphere has generally been too unfavourable for delicate observation. The instrument used is an $8\frac{1}{2}$-feet Munich telescope.*

"*Ochtertyre, Crieff, Nov.* 13, 1858."

Physical Observations of Jupiter. By W. Lassell, Esq.

"As a favourable state of atmosphere for the use of the 20-foot equatoreal is in this locality almost as rare as the visit of a comet, I cannot deny myself the pleasure of sending you a drawing of the planet *Jupiter*, as seen last night with this instrument—or rather early this morning.

"I had previously been in the smaller dome, showing the planet with the 9-foot telescope to some visitors, and after their departure to some members of my family, the atmosphere being unusually fine; and, moreover, on turning to γ^2 *Andromedæ*, and finding that the picture was very fine, the third star being well and cleanly separated from the second, with a power of 752, so as to show the sky fairly between them; I opened the larger dome and began a series of measures of the equatoreal diameter of *Jupiter*, with Airy's double-image micrometer.

"When they were nearly completed I was struck with the unprecedented sharpness of the images; and even while they were separated and in process of passing over each other, some details caught my attention which I had not observed with the 9-foot, and induced me forthwith to prepare to observe the planet more attentively and with higher powers. The enclosed drawing I have made as carefully as in my power, from sketches made during observation in my journal, and it represents gene-

* It is interesting to find that the two streaks here referred to by the author exhibit on the accompanying diagram precisely the same relative magnitude, and the same position on the disk (subject to a slight difference in longitude, caused by the rotation of the planet), as similar streaks observed by Mr. Lassell (see next paper).—Ed.

rally the aspect of *Jupiter* at about $13^h\ 45^m$ G.M.T. I say generally, because to give every minute spot and shading in its true form, position, and intensity, is far beyond my powers. There is, however, I believe no point of detail here marked down, which had not its counterpart in the original, to which it bears at least a strong resemblance. One of the most novel features I noticed was the existence of a numerous group of white spots in the bright equatoreal region, far more delicate and difficult to see than those in the southern hemisphere, which I first noticed on the 27th March, 1850, and of which there is a sketch in the *Monthly Notices*, Vol. X., page 134. For several years I failed to see any such spots upon the face of *Jupiter* at all, but last year they appeared again in the same quarter of the planet, and were attentively and ably observed by Mr. Dawes, who has furnished the Society with a series of elaborate and very beautiful drawings of them. As they are always very delicate phenomena, it may be possible that the southern declination of *Jupiter*, during a great part of the interval, may account for their not having been observed.

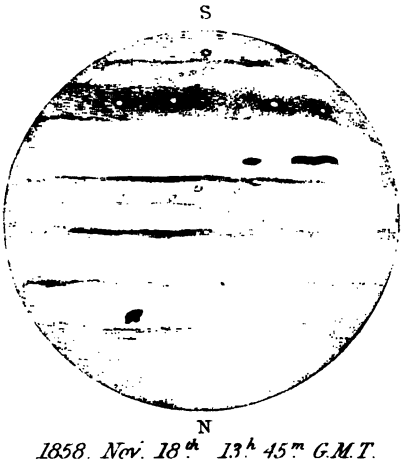
"But the appearance of similar spots in the very brilliant central portion of the planet is to me new. They could only be seen in the most tranquil moments of the atmosphere, while the formerly noticed spots in the southern region remained constantly and steadily visible.

"The two remarkable oblong spots have existed some time on the planet, but I have never seen them so distinctly before: in point of intensity they are not at all exaggerated in the drawing. The dark belts of *Jupiter* this year are far narrower, more numerous and far lighter in tint than usual—this accounts perhaps for the great contrast, and suggests the idea that these spots (which have also others in their train) may be an incipient dark belt, such as has formerly occupied this region of the disk.

"This observation has been very interesting to me in another point of view, as establishing the correctness of the opinion I have held for some time, that the last surfaces given to the mirrors of the 20-foot telescope are as perfect both in point of curve and regularity as those of the 9-foot telescope, and only require an atmosphere *proportionately fine* to establish their proportionate efficiency. No impression was received of the existence of the equatoreal spots with the 9-foot, though defining admirably with 270, while with 565 on the 20-foot they were seen nearly as plainly at the moments of best vision, as the southern spots were seen with the former instrument. The chief difference in the quality of vision was that in the smaller equatoreal, it was almost continuously good, while in the larger equatoreal the fits of finest vision had to be waited for.

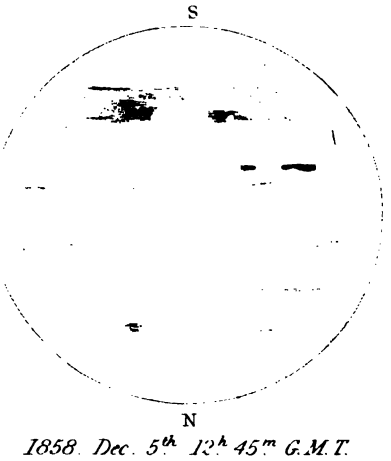
"*Bradstones, Sandfield Park, near Liverpool,*
Nov. 19, 1858."

Fig. 1.



1858. Nov. 18th 13h 45m G.M.T.

Fig. 2.



1858. Dec. 5th 12h 45m G.M.T.

Views of Jupiter.

"Referring to my former communication of the 19th Nov., I send another drawing of the planet *Jupiter* as seen on the 5th Dec., at $12^h\ 45^m$ with a power of 430 on the 20-foot equatoreal. On comparing this diagram with the former one, it will be seen that the two conspicuous dark spots, or lines, are nearly similarly situated in both, but the largest and plainest bright spot is now evidently in a different position with respect to the dark ones, as if it had gained somewhat in rotation upon them. I observed both the bright and dark spots on the 21st Nov., but they then appeared to me to have the same relative positions as on the 18th. The dark spots have not sensibly changed their relative position either to each other or to any specific locality on the face of the planet since I first noticed them 7 or 8 weeks ago; *i.e.*, if their regular return to conjunction in accordance with the known period of the planet's rotation may be taken as a test. The night of the 5th inst. was not nearly so favourable as that of the 18th ult., and not so many spots in the southern belt could be seen, nor more than indications of bright spots in the bright equatoreal band; yet the two bright spots here delineated were so exactly similar in appearance and in relative position, that I think there is ground for presuming that they are the same as the largest and brightest two seen on the 18th ult., which must lead to the inference of a rate of motion different from that of the dark spots. Further observations, if the sky permit, may possibly settle the question.

"The outline of this diagram is rather carefully drawn from a series of measures made with the double-image micrometer with a view to determine the exact form of the planet, whether truly elliptical or not. The measures were made on the 5th inst., and were as follows—the numbers being simply the readings of the micrometer—magnifying power 467:—

Equatoreal diameter .	11·115
At 30° latitude diameter .	10·955
,, 60° ,, ,, .	10·627
Polar diameter . .	10·494

A comparison of these measures of the polar and equatoreal diameters gives the equatoreal to the polar as 1 : 0·9441; a former series with power 310 on the 21st Nov., gave the proportion as 1 : 0·9422, another with power 467 on the same evening gave 1 : 9458, mean of the whole 1 : 0·9440, or the polar diameter less than the equatoreal by $\frac{1}{17\cdot857}$th part of the latter. The result shows a less ellipticity than I had expected, but the measures were carefully taken, and the circumstances pretty favourable.

"*Liverpool, 16th December*, 1858."

Note on the Variable Star numbered 83 *in the Greenwich Catalogue of* 1576 *Stars for* 1850, *whose Variability was discovered by Mr. Hind.* By the Astronomer Royal.

This star was discovered to be variable by Mr. Hind on the 4th of November, 1850, as announced in Vol. XI., No. 2, of the *Monthly Notices* (page 46). He there calls it a fiery-looking star of the seventh magnitude, and states that it was not noticed by Lalande or Bessel, and does not occur in the excellent map of that region recently published by the Berlin Academy. It was observed first at Greenwich on Nov. 8 of the same year, and noted as of the 6.7th magnitude; on Nov. 14, 28, and 29, it was estimated of the 8th magnitude; and on Dec. 5, 7, and 12, of the 8.9th magnitude. Its colour on Nov. 14 was noted as pale white, and its appearance disk-like; on Nov. 28 the colour was again noted to be white; but on Nov. 29 and Dec. 12 it was slightly red. It was not observed again at Greenwich until the year 1854, and was then estimated to be of the 11th magnitude; since which time it has been repeatedly looked for without being observed. Till the present year this may have arisen from other small stars near it having been observed instead; but this year it has been particularly looked for with an exact knowledge of its place, particularly by Mr. Dunkin on Dec. 2, when the night was remarkably clear, who thus became assured of its total invisibility.

Observations of Comet V., 1858 (*Donati's*), *taken with the Equatoreal of the Liverpool Observatory.* By J. Hartnup, Esq.

1858.	G.M.T.	Comet's R.A.	Log $\frac{p}{P}$	Comet's N.P.D.	Log $\frac{q}{P}$
	h m s	h m s		° ′ ″	
Sept. 12	7 18 29·8	11 10 24·04	+8·665	53 54 50·7	−9·8859
	7 33 30·0	26·65	8·653	48·0	9·8978
	7 48 31·0	28·97	8·638	42·3	9·9089
15	7 26 36·5	11 22 22·16	8·660	53 37 12·6	9·8915
	7 46 37·9	26·16	8·643	8·4	9·9066
	8 6 39·3	30·06	8·621	3·9	9·9210
18	7 31 5·0	11 36 39·89	8·659	53 33 10·6	9·8929
	7 43 6·4	42·53	8·649	14·3	9·9021
	7 55 7·5	45·29	8·637	18·7	9·9110
21	7 7 28·1	11 53 48·49	8·677	53 52 9·9	9·8698
	7 30 30·4	54·69	8·662	19·0	9·8890
	7 53 33·6	11 54 0·89	8·642	28·1	9·9066

1858.	G.M.T.	Comet's R.A.	Log $\frac{p}{P}$	Comet's N.P.D.	Log $\frac{q}{P}$
	h m s	h m s		° ′ ″	
Sept. 24	6 33 23·9	12 14 30·93	8·686	54 47 39·3	9·8344
	6 48 26·4	35·59	8·683	57·4	9·8484
	7 3 29·2	40·35	8·678	54 48 15·2	9·8618
25	6 42 22·1	12 22 25·54	8·683	55 18 7·4	9·8413
	6 52 23·9	28·68	8·680	20·4	9·8505
	7 2 26·2	32·20	8·677	35·2	9·8594
27	6 52 48·4	12 39 46·50	8·675	56 42 17·4	9·8482
30	6 9 35·2	13 9 19·04	8·658	60 1 17·9	9·8108
	6 24 39·9	25·73	8·660	2 11·2	9·8236
	6 39 44·2	32·22	8·662	3 6·5	9·8363
Oct. 4	5 52 49·7	13 55 19·09	8·605	67 38 18·2	9·8186
	6 12 58·4	29·42	8·618	40 22·5	9·8316
	6 33 4·6	39·43	8·627	42 24·1	9·8439
8	6 15 7·2	14 45 26·07	8·570	79 25 17·2	9·8703
	6 55 24·4	47·54	8·595	30 52·6	9·8806
13	6 30 53·8	15 44 53·96	8·543	96 54 16·5	9·9184
	6 46 0·2	45 0·97	8·558	56 24·9	9·9163
15	5 59 52·1	16 5 40·80	+8·493	103 16 6·5	−9·9379

1858.	Star of Comparison.	1858.	Star of Comparison.
Sept. 12	B.A.C. 3811	Sept. 27	12 Can. Ven.
15	3965	30	B.A.C. 4390
18	3998	Oct. 4	Arcturus
21	4128	8	B.A.C. 4853
24	4128	13	5306
25	4233	15	5720

The observations are corrected for refraction. The corrections to be applied for parallax in time and arc, are represented by p and q, P is the equatoreal horizontal parallax. The following are the assumed mean places of the stars of comparison for

1858, Jan. 0.

	R.A.	N.P.D.	Authority.
	h m s	° ′ ″	
B.A.C. 3811	11 1 30·37	52 55 14·00	British Association Catalogue
3965	11 33 33·92	54 59 51·26	....
3998	11 42 18·63	54 16 45·57	....
4128	12 9 21·81	56 8 44·56	....
4233	12 26 37·93	55 58 4·20	....
12 Can. Ven.	12 49 22·68	50 54 50·04	Nautical Almanac 1858
B.A.C. 4390	13 0 21·84	61 36 42·55	British Association Catalogue
Arcturus	14 9 11·09	70 4 35·57	Nautical Almanac 1858
B.A.C. 4853	14 34 54·62	77 43 27·64	British Association Catalogue
5306	15 53 8·17	98 0 20·49	....
5720	16 53 9·76	103 20 22·78	....

The observations were for the most part taken before the close of daylight, at which time, the tail and coma not being visible, the nucleus was well defined and suitable for accurate determination of its position. Later in the evening I was frequently disturbed by visitors, but through the kind assistance of my friend Mr. J. T. Towson, the following measures of the nucleus, coma and tail were obtained at about 20^h, Liverpool sidereal time, each evening.

Date.	Diameter of Nucleus.	Diameter of First Envelope.	Diameter of Second Envelope.	Distance from Centre of Nucleus to Front of Coma.	Diameter of Coma at Right Angles to Tail Measured through Centre of Nucleus.	Length of Tail Measured along the Curve on the Convex Side.	Distance from Nucleus to End of Tail in a Straight Line.	Greatest Diameter of the Tail.
1868.	″	′ ″	′ ″	′ ″	′ ″	° ′	° ′	° ′
Sept. 30	17	2 1		1 26	5 46	26 35	26 0	3 10
Oct. 4	20	1 40		1 32	4 28	34 20	31 30	6 0
8	22	1 52	5 37	3 36	8 46	39 30	35 0	7 30
11	23	1 54	5 43	4 19	10 39	..	..	..

Decided dark spots were seen in the coma near the nucleus; one on the 8th and two on the 11th of October. The tail was curved towards the north, and broadest at or near the end. It was more luminous and better defined on the convex than on the concave side, and a dark band passed from the nucleus through the centre to the extreme end of the tail. On the 30th September a well-defined conical shadow was visible, the length of which, measured from the nucleus or the base of the cone to the apex, was 18′. The length of this shadow on the 4th October was 21′, but the contrast between it and the dark band in the centre of the tail was much less striking than it was on the 30th September. On the 8th October it was rendered invisible by the increased darkness of the band which passed through the centre of the tail. The tail of the comet was more symmetrical and the envelope was brighter and better defined on the 30th September than on any other occasion.

On the Advantages to be derived from the Use of Silver Mirrors for Reflecting Telescopes, and on a Novel Method of Mounting such Instruments. By Dr. Steinheil.

(*Translation of a Letter from Dr. C. A. Steinheil of Munich, to Professor C. A. F. Peters of Altona, published in No.* 1138 *of the Astronomische Nachrichten*).

"What mainly induces me to address this communication to you is a wish to make you acquainted with the result of my

latest experiments,—a result that appears likely to lead to a total modification in the construction of astronomical instruments. Do not be alarmed at the sanguine hope expressed in the above proposition, but allow me to detail the observations on which it is founded.

"As long as two years ago (see the supplement to the *Augsburger Allgemeinen Zeitung* of the 24th March, 1856, first article) I constructed a telescope with silvered glass mirrors. The silvering was effected by Liebig's process, and the extremely thin coat of silver adhered so firmly to the glass that its metal-side could be polished with soft leather without injury. The mirror afforded a surprisingly good image, and I even at that time entertained the hope of arriving by this course at a durable construction of catoptrical instruments, inasmuch as the base of glass maintains the form of the mirror. Quite recently I have taken the matter up again, which in the meanwhile M. Léon Foucault, see the *Comptes Rendus*, vol. xliv., No. 7, Feb. 1857, p. 339, without being acquainted with my experiments, has laid before the Institute of France as something novel.

"Very recently Liebig has materially improved his method of silvering, so that one can now obtain faultless mirrors of any size that are coated so thinly with silver that the sun when looked at through the silvering appears like a sharply defined blue disk of light. These mirrors, too, bear polishing on the metal-side quite readily. One has simply to rub the surface with a little cotton pad covered with the finest glove-leather. In a few minutes a high polish is obtained, quite without scratches. I have determined the reflecting powers of these mirrors by carefully measuring their brightness with my eye-piece photometer; and at the same time I observed also therewith other mirrors as well as prisms, and likewise object-glasses, by way of comparison.

"The following amount of brightness was obtained for an angle of reflexion of 45°, that due to direct light being considered = 100:—

	Brilliancy.	Loss of light per cent.
Direct light	100·00	0·0
Silver mirror	91·00	8·9
Quicksilver mirror with glass	76·50	23·5
Metal mirror, Lord Rosse's alloy	67·18	32·8
Object-glass, by Fraunhofer, of 34 French lines aperture, and 42 inches focal length	76·00	24·0
One of my object-glasses of 21 French lines aperture and 21 inches focal length	86·67	13·3
Crown-glass prism of 50 French lines aperture	77·00	23·0

"It thence follows that silver mirrors reflect more light than rectangular glass prisms, and that after a second reflexion

they still have more light than Fraunhofer's object-glasses, and, finally, that after three reflexions they yet have more light than the present mirrors of telescopes after a single reflexion. We are enabled, therefore, by the employment of silver mirrors to construct reflecting instruments that with equal aperture are *superior* to our achromatics in their amount of light.

"I have examined to what degree the accuracy of the form extends. For although the coat of silver is very thin there, nevertheless in giving the polish it might be more rubbed off at one part than another, and the form be thereby injured. My spherometer, which rests on mirrors of contact without axes, enables me to produce a glass surface of any radius of curvature upon which there is not anywhere a deviation from the sphere amounting to a hundred thousandth of a French line. I see indeed up to two millionths of a line, but I am only certain in my measurement up to about seven millionths. When an object-glass (one of about 6·5-inches aperture, English measure) is finished with this degree of precision, it unites the rays with sufficient accuracy. This cannot, however, be recognised in the image, but only on changing the position of the eye-piece towards the focal point of the object-glass. If, namely, the image of the sun upon a highly-polished steel ball about half-an-inch in diameter (the inclosed space being maintained at a uniform temperature) is observed, at the distance of about 60 feet, with a refractor of 4-inches aperture, using a power of about 250, it will have the appearance of a star of the first magnitude when the air is quite steady, and look like a small defined disk of light encircled by fine coloured rings (Schwerd has explained the theory of this in his 'Beugungserscheinungen,' or 'Phenomena of Inflexion'). But on drawing out the eye-piece about half-an-inch, there is formed a disk of light consisting of concentric spectrum-rings. These are due to the interference of the rays proceeding from one point, and each ring corresponds to a determinate zone of the object-glass wherein the rays interfere by one length of wave. These rings, therefore, are the best test of the accuracy of the form, since they at the same time indicate whether the individual portions of a line of wave are situated symmetrically. I have tested my silver mirror by this means; it showed the interference-rings precisely as a good object-glass does. Upon this the mirror was again submitted to another polishing, and that without any further precaution. No alteration of any kind in the effect on the rings was however observable.

"Whether a plane mirror is worked sufficiently true is best seen at small angles of reflexion. If a star at an angle of reflexion of 5° still appears round and sharp, and if the interference-rings remain rigorously circular the mirror acts satisfactorily. This test is even more sensitive than altering the position of the eye-piece with a direct and a reflected image. I possess silver mirrors that stand these tests perfectly well.

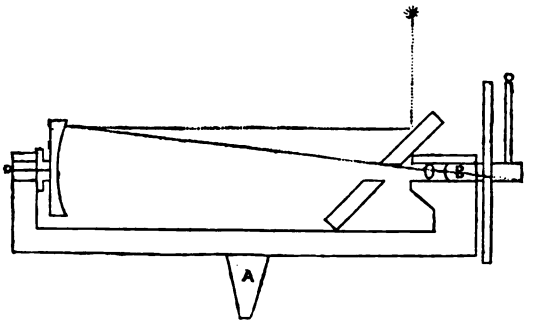
"Now, I infer from the above, firstly, that the forms of silver mirrors can be produced with sufficient accuracy *to afford the same precision of the image* that is attained with refractors, and I thereby refute practically the opinion that reflectors never give as distinct images as refractors do, and, secondly, that a subsequent polishing of the mirror, which may become necessary, should it tarnish with time, has no appreciable influence on the form of the mirror. When one considers that these mirrors may be protected from tarnishing when they are not in use by a cover of india-rubber, so that a fresh polish will not have to be given to them above once a-year — object-glasses, too, must be cleaned at least once during that time—and that if after the lapse of many years the silver surface of the mirror should have suffered, it is a light matter to have it silvered again, the objection hitherto brought against catoptrical instruments on the score of their want of durability appears also to be overcome.

"Hence we now possess reflectors which are in every respect free from the faults of the former instruments. What advantages in the mechanical construction of astronomical instruments may not be attained if one may presuppose two reflexions! I will here only touch shortly upon two arrangements, treating this matter in detail in a monography, that the Bavarian Academy of Sciences will publish in the ensuing autumn on the occasion of its centenary anniversary.

"*Catoptrical meridian circle, or transit.* The axis of the concave mirror of 6-inches aperture and 36-inches focal length, lies east and west, the mirror is fixed on a stone pillar. It receives light from a plane mirror, permanently fixed at an angle of 45°, and which rotates round the prolongation of the optical axis of the concave mirror. This axis of rotation, as also the plane mirror, is traversed by an aperture, so that the image of a point in the meridian falls in the axis of rotation. The cross wires are placed here, and the eye-piece sees them distinctly, as well also as the image of the point in the meridian. An altitude circle fixed firmly on the perforated axis, and lying in the plane of the meridian, measures the nadir distances from a quicksilver horizon. The spherical aberration of the marginal rays of this mirror amounts to 0″·374, and this is strictly corrected by means of a small object-glass, having a long negative focus, which is inserted in the tube 3 inches in front of the cross wires. If both the mirrors are attached to the same stand, pretty much in the manner shown in the figure, we have a reflecting telescope mounted horizontally, since a rotation about A alters the azimuth, while a rotation about B alters the altitude.

"If the axis A be made to coincide with the axis of rotation of the heavens, the telescope is equatoreally mounted, for rotation about A measures the hour-angle, about B the declination. Of course the eye-piece tube is provided with a finder,

for the sake of more readily meeting with the objects. This telescope possesses, moreover, another advantageous quality, the cost of an instrument of 6·4 inches aperture English measure



is only 300 Bavarian florins (about 25*l.* 10*s.*), and in its action it must be superior to a refractor of equal aperture, because it is free from chromatic aberration. For this reason, especially, catoptrical heliometers will possess peculiar recommendations. I am convinced that by the employment of silver mirrors upon glass we shall arrive at obtaining reflecting telescopes of colossal dimensions. Only in that case it will be necessary to adopt a method of mounting them different to that now in use. It appears to be indispensable to give the telescope, or rather the two mirrors respectively, for the tube may be dispensed with, a constant position.

"If the axis of the concave mirror is brought to coincide with that of the rotation of the heavens, and if the plane mirror, the centre of which is perforated, is so disposed that it transmits to the concave mirror situated parallel to its axis, the parallel rays proceeding from a star, then can the image, formed by the concave mirror be observed through the aperture in the plane mirror, and the elimination of the diurnal rotation of the earth is reduced simply to a rotation of the plane mirror about the axis of the heavens. For other declinations one has only to give the plane mirror another inclination. When thus arranged no special observatory is needed for the erection of a telescope of 2-feet aperture. The plane mirror might be adapted in front of a window of a dwelling-house, the concave mirror being situated below on the ground in the direction of the axis of rotation of the heavens. A tube between the two mirrors would be quite superfluous.

"By means of this application instruments of great optical power, and simple and convenient to manage, are rendered accessible to the admirers of astronomy, and that, too, at a moderate outlay, which is a point that should not be by any means overlooked.

"*Munich, April* 5, 1858."

Catalogue of 317 *Stars selected from the B.A. Catalogue (being such as were supposed to have large proper motions), deduced from Observations made at the Honourable E.I. Company's Observatory at Madras in the Years* 1853–7. By Capt. W. S. Jacob, Director of the Madras Observatory.

"The instruments employed on the observations from which this Catalogue is derived are a 5-foot transit and 4-foot mural circle, both by Dollond, which have been frequently described in the Madras volumes. Nearly the whole work of the Catalogue, both as regards observation and reduction, was executed by the native assistants in the observatory, and will be found creditable to them. In case of any doubt or discrepancy, I have occasionally repeated an observation, and have, of course, maintained a strict watch over the errors and adjustments of the instruments; the whole of the reductions have also been carefully checked by myself.

"During a portion of the time embraced by these observations, the charge of the observatory devolved on Major W. K. Worster, of the Madras Artillery, in consequence of my absence in England for the recovery of my health.

"The large proper motions which had been assigned to many of the southern stars, are for the most part contradicted, and seem to have their origin from instrumental or other errors in the Paramatta observations, they having been dependent chiefly on the Brisbane Catalogue."

Proper Motions of the Stars of the Greenwich Catalogue of 1576 *Stars for* 1850 *not included in the Greenwich Twelve-Year Catalogue, deduced by Comparison with the Results of Bradley's Observations as given in the "Fundamenta Astronomiæ."* By the Rev. R. Main, M.A.

This paper is a continuation of a former paper by the author, published in vol. xix. of the *Memoirs of the Society*, containing a determination of the proper motions of all the stars which are common to the *Fundamenta* and the Greenwich Twelve-Year Catalogue of 2156 Stars. The last-mentioned Catalogue, and the one referred to in the title of this paper, contain the results of all the star-observations made at Greenwich from the year 1836 to 1853 inclusive, comprising a period of eighteen years. The whole number of stars of which the proper motions are assigned in the two papers amounts to 1437. It was explained in the introduction to the former memoir that all the stars observed at Greenwich have been selected for special purposes, and that the accurate determination of their places forms one of the most important objects of modern sidereal astronomy. In this respect the stars contained in the

Catalogue of 1576 Stars are particularly valuable, several new classes having been introduced. Amongst these may be mentioned the results of the observations necessary to complete the list of all stars visible at Greenwich between the first and fourth magnitudes (with very rare and accidental exceptions); of a list of about 140 stars, to be used in addition to those given in the *Nautical Almanac*, for the determination of clock-error; of stars in the constellation *Canis Major* compared with *Sirius* for the purpose of more accurately determining the variability of the proper motion of the latter star; of about thirty stars near the north pole arranged in pairs differing by nearly 12 hours in right ascension, useful for determining azimuthal errors; and, finally, of such stars as have conspicuous proper motions.

Occultations of Stars by the Moon. Observed by Capt. Noble.

Friday, November 19th.

μ Arietis. The star disappeared instantaneously at $4^h\ 22^m\ 18^s{\cdot}8$ L.S.T.

The reappearance was not seen.

Monday, November 22d.

136 Tauri. The star disappeared at $2^h\ 8^m\ 37^s{\cdot}5$ L.S.T.

And reappeared at $3\ 13\ 38{\cdot}8$ L.S.T.

The star entered into contact somewhat sluggishly, and the brightness of the moon was almost overpowering; notwithstanding which the disappearance was well seen. The emersion was perfectly instantaneous.

The telescope employed in each case was my Ross equatoreal of 4·2 inches' aperture, with a power of 115, adjusted on the respective stars.

Observatory, Forest Lodge, Maresfield, Sussex,
9th December, 1858.

Physical Observations of Comet V., 1858. By E. B. Powell, Esq.

Donati's Comet was first noticed at Madras on September 30th, when it was seen through breaks in the clouds shortly after sunset. On succeeding evenings it gained rapidly in brilliancy, becoming a truly magnificent object. My equatoreal not having arrived from England, I resolved to try how closely I could determine the comet's path by means of sextant observations, and accordingly took the following measures with

a good 8-inch instrument by Troughton and Simms, graduated on platinum to 10″; and I am in hopes that the care I expended upon the work will have prevented the error on any distance from rising above 1′.

Greenwich Mean Time.	Comet to * A	Comet to * B	Comet's R.A.	Comet's N.P.D.
	o ′ ″	o ′ ″	h m s	o ′ ″
Oct. 3·0583	8 16 17	13 7 35		
4·05139	4 46 12	11 24 35	13 52 54	67 10 6
8·05	11 48 37	19 12 42	14 42 47	78 43 24
12·06875	9 42 52	8 8 54	15 31 13	92 43 42
16·05624	10 35 9	5 25 4	16 13 39	105 39 23
27·05903	2 6 0	2 37 25	17 33 59	126 19 55
Nov. 4·05556	8 30 50	16 55 34		

Greenwich Mean Time.	Comet's Geoc. Long.	Comet's Geoc. Lat.	* A	* B
	o ′ ″	o ′ ″		
Oct. 3·0583	...	...	Arcturus	ι Bootis
4·05139	196 48 45	31 58 14 (+)	Arcturus	ι Bootis
8·05	214 22 50	25 43 8 (+)	Arcturus	α Cor. Bor.
12·06875	231 7 2	15 51 9 (+)	α Serpentis	β Libræ
16·05624	244 20 48	5 29 24 (+)	Antares	β Scorpii
27·05903	264 37 35	12 59 25 (−)	35 Scorpii	κ Scorpii
Nov. 4·05556			ι Sagittarii	Venus

The difference of longitude between Madras and Greenwich was taken at $5^h\ 21^m$.

On all the dates except November 4th, the observations for the star A were taken in two sets; one before the measures for the star B, the other after, in order that the mean distances might be reduced to the same instant. To clear the resulting right ascension and north polar distance of the comet from refraction, I calculated* the errors in R.A. and N.P.D. on the catalogue places of the stars for the moment of observation, added them to the true R.A. and N.P.D., then worked out the apparent R.A. and N.P.D. of the comet, and finally applied to these last the appropriate corrections. Pontécoulant's modification of Laplace's method of approximating to the perihelion distance, and the epoch of perihelion passage, were next applied to the observations of the 4th, 8th, and 12th October, and gave approximate values of the fundamental elements. Owing, however, to an error in the reduction of the distances of the comet from the stars of reference on the 12th Oct., which was not detected for three or four days, the values were much

* In this a table of refractions in hour-angle and north polar distance for Madras, drawn up by Capt. Jacob, proved of service.

inferior in closeness to the truth to what they would have been if the mistake had not occurred; and consequently a great deal of unnecessary labour was entailed in correcting the fundamental elements. After no fewer than five repetitions of the process of correction, I arrived at the following values, using in this part of the problem the places of the comet for the 4th, 12th, and 27th October:—

Epoch of perihelion passage, Sept. 29·9908 G.M.T.	
Perihelion distance	·577974
Inclination of orbit to ecliptic ..	63° 5′ 58″
Long. of ascending node ..	165 17 52
Long. of perihelion on ecliptic ..	14 26 23

Motion retrograde.

The errors on the parabolic arcs from the 4th to the 12th, and from the 4th to the 27th, are respectively—

$$u - \iota = -7'' \quad \text{and} \quad u' - \iota' = -3''$$

Hence, taking into account the length of the arc described between the 4th and the 27th, which was 53° 42′ 41″, and the telling portion of the orbit to which it belonged, I am induced to hope that the foregoing elements represent the comet's path with considerable accuracy. It would be waste of time to apply corrections for parallax and aberration to sextant observations; the preceding may, therefore, be regarded as the final results of my measures.

I add a few remarks respecting the changes of appearance presented by the comet from day to day.

Oct. 2d. Nucleus large and bright, but ragged; not so bright as *Arcturus*, but considerably brighter than η *Ursæ Majoris*. The eastern or convex edge of the envelope sharper and brighter than the other; the curvature, too, on the following side much greater than on the preceding. The tail extending from two-thirds to three-fourths of the distance to η *Ursæ Majoris*, or about sixteen degrees.

Oct. 3d. Nucleus brighter than ι *Boötis* and α *Coronæ Borealis*, but not so bright as *Arcturus*. The tail brighter and longer than last night, extending very nearly to η *Ursæ Majoris*. About the nucleus a cuspidated body of light visible, with a semicircular edge towards the sun. The following edge of the envelope clear and marked; the preceding faint and indistinct.

Oct. 4th. Nucleus nearly as bright as *Arcturus*, but very ragged; the cuspidated appearance evident; the outline of envelope indistinct even on the upper or following side, showing in this respect a decided change since last night. A dark axial space visible down the envelope for the first time; η *Boötis* lying before the head, and almost in the direction of the tail, which is about 24° long.

Oct. 8th. Outline of envelope ragged, the tail reaching a little beyond *α Coronæ Borealis*. On the lower side the light shades off almost imperceptibly; on the upper, though ragged, it terminates comparatively abruptly. The darkness down the envelope scarcely so clear as before. Nucleus about as bright as *Mars* or *α Lyræ*. The shape of the envelope bears a resemblance to a pen, being narrow at the head, and after a short space suddenly spreading on the lower side like the feather of a quill.

Oct. 12th. Moonlight. Nucleus nearly but not quite as bright as on the 8th. The tail much longer but fainter; the axial darkness down the envelope not visible.

Oct. 16. Nucleus large and diffused; the convex or (now) southern side of envelope brighter than the other, the light in it appearing to diminish more abruptly, and so to form a greater contrast with the ground of the sky.

Oct. 26th. Envelope evident, but very faint; nucleus large, ragged, and indistinct.

Oct. 27th. Comet very faint; nucleus large and hazy; tail roughly estimated at four or five degrees in length.

In examining the appearance of the comet, use was made of a $3\frac{1}{2}$-foot refractor, with an aperture of $2\frac{3}{4}$ inches.

Madras, Nov. 13, 1858.

Observations of Comet V., 1858. By R. L. J. Ellery, Esq.

(*Letter to the Astronomer Royal.*)

A large comet made its appearance in these latitudes about the 10th of this month, and continues to increase in brilliancy —its nucleus is about equal to a star of the $1\frac{1}{2}$ magnitude—its tail now extends over about 6°; at its first appearance it was not visible for more than 3°. I inclose herewith a few observations made with an 18-inch altazimuth on the 145th meridian. The observations are merely corrected for index errors and microscopes, each observation is made up of sets with the instrument in reversed positions. We are getting a series of photometric measurements and drawings, which I hope to be able to forward by next mail.

Date.	Object.	Observed Azimuths.	Observed Altitudes.	Barometer.	Temp. Air.	Local Sidereal Time.
		° ′ ″	° ′ ″	Inches.	Fahr.	h m s
Oct. 12	Comet	79 59 37·35	16 5 50·99	29·964	53·0	20 14 30·00
		81 53 49·57	14 2 24·72			20 25 3·00
		84 55 50·30	9 42 20·32			20 47 37·75
		86 31 0·82	8 11 32·60			20 55 30·25
	Antares	97 40 40·20	35 24 7·11			20 37 25·00
		101 35 9·83	29 35 12·66			21 7 17·05

Date.	Object.	Observed Azimuths.	Observed Altitudes.	Barometer cor. to 32.	Temp. Air.	Local Sidereal Time.
		° ′ ″	° ′ ″	Inches.	Fahr.	h m s
Oct. 13	Comet	85 15 53·50	15 12 59·39	29·920	59·8	20 41 30·00
		86 52 17·67	13 19 56·44			20 51 42·50
		89 41 59·97	9 48 37·28			21 9 25·35
	Antares	100 42 17·60	30 52 25·63			21 0 37·50
		103 33 46·42	26 39 5·19			21 22 31·00
14	Comet	86 10 58·40	19 39 36·70	29·952	57·6	20 39 59·90
	Antares	89 30 4·52	32 39 57·00			21 51 25·40
	Comet	92 3 43·60	12 19 54·05			21 17 57·00
	Antares	108 2 21·40	20 37 52·66			21 56 53·60

Observatory longitude 145° E., latitude 37° 46′ 50″.

"*Astronomical Observatory, Williamstown,*
"*Victoria, Oct.* 16, 1858."

Sextant Observations of Donati's Comet taken on board Her Majesty's Ship Siren.

(*Communicated to the Admiralty by Capt. G. M. Balfour, Commander.*)

Date. 1858.	Lat. S.	Long. W.	Compass Bearings.	Ship's M.T. nearly.	Remarks.
	° ′	° ′		h m s P.M.	
Oct. 7	15 7	33 44	N.W. by W.	7 35 0	Apparent Altitude 2°45′. Was first seen at an altitude about 10° 1′ at 7h 15m P.M. About the same size as a star of the 2d magnitude, with a tail which subtended an angle of about 5°.
8	16 28	34 31	W.N.W.	6 44 0	App. Dist. from Moon's N. L. 29° 45′.
	...	...	...	7 5 33	App. Dist. from Vega 57° 31′.
	...	...	...	7 5 33	App. Altitude 5° 4′ 35″. Distance from Vega 57° 44′.
9	...	...	...	...	Heavy rain. Stars obscured.
10	19 6	35 31	W. by N. ½ N.	7 to 8	Apparently about the same size as a star of 1st magnitude. Length of tail about 10°.
11	19 32	37 0	W. by N.	7 11 0	App. Alt. 13°.
	...	..	...	7 16 0	App. Dist. of Moon's N. L. 33° 10′.
	...	...	...	...	App. Dist. of Vega 58° 18′.
	...	...	...	7 21 0	App. Dist. of Altair 65° 32′.
	...	...	...	7 23 0	App. Alt. 10° 15′. Set W. ½ N. at about 8h 9m P.M.

Royal Observatory, Greenwich, 1858, *Dec.* 9.

There was exhibited at the Meeting a drawing of Donati's Comet, as it appeared early in October when viewed from the Dockyard in Simon's Bay, at the Cape of Good Hope. It was executed by Miss Caroline Drew, daughter of Captain Drew, R.N., and was communicated to the Society by Admiral Smyth.

Captain Washington, Her Majesty's Hydrographer, writing to Admiral Smyth, states that Donati's Comet was first seen at

Rio de Janeiro	..	Oct. 7
Buenos Ayres	..	7
Loando (W. Africa)		7
Cape of Good Hope		11

Errata in the Logarithms of Taylor, Shortrede, Babbage, Hülsse, and Filipowski.

M. Houel, in a letter dated Thaon, near Caen, in Normandy, thus writes to Admiral Manners, the Foreign Secretary:—

"I am engaged at present in revising the part of Callet's Tables which contains the logarithmic sines and tangents to every second of arc for the 5th degree. Besides a very great number of errata, which a simple collation with Taylor's Tables has brought to light, I have met with some which are due to Taylor himself, and have been subsequently reproduced by Shortrede.

"By the aid of the *Trigonometria Britannica*, I have obtained the following corrections:—

Log sin	4° 18′ 53″	8·8764197
	4 4 49	8·8522000
	4 22 32	8·8824884

"Taylor and Shortrede give these logarithms with a unit less in the last figure.

"The following are also some errata which I have remarked in Babbage's Tables and the Antilogarithmic Table of Filipowski:—

Babbage.

Last page (*Constants*).

	For	Read
Rad. reduced to minutes, Compl. of Log	7·463	6·463
Sidereal Day	·99726967	·99726957

(This last erratum is also to be found in the *Sammlung Mathematischer Tafeln* of Dr. Hülsse, Leipsic, 1849.)

Filipowski.

	For	Read
Page 10, Number to Log ·04207	1617	1717

***Micrometrical Measures of* 120 *Double or Multiple Stars, taken at the Honourable East India Company's Observatory at Madras in the years* 1856–58.** By Captain W. S. Jacob, Director of the Madras Observatory.

The measures contained in this paper may be considered as a continuation of those given in the last volume of Madras results for 1848–52, having been made with the same instrument, viz., the equatoreal of 6·3 inches aperture, constructed for the author by Lerebours and Secretan of Paris.

The stars selected have been chiefly such as were either suspected or known to be binaries, or such as seemed likely to give indications of parallax, or as for some other reason seemed to require watching, no attempt having been made to detect new ones. Three such were, however, picked up accidentally; one (B.A.C. 2170) in observing an occultation by the moon, when the star disappeared at two distinct instants, the others are τ^4 *Eridani*, which has a close companion as well as the distant one mentioned in Smyth's *Cycle*, and a small very close pair in $9^h\ 22^m$, 118° 11′.

The following are a few extracts from a series of notes which the author has appended to his Catalogue:—

"539, ζ *Cancri*. A, B seems to have an orbit nearly circular with a period of about 110 years. The movement of A, C appears to be about −0°·5 per annum, but is somewhat irregular; perhaps the proximity of B to A may give rise to a bias in measuring; there must also be perturbations arising from the mutual action of the three bodies, even should there be no other disturbing force.

"ξ *Ursæ Majoris*. The large errors exhibited by the observations of this star, when compared with any elliptical hypothesis, seem to suggest perturbation. Sir John Herschel remarks (*Mem. Ast. Soc.* Vol. V.), that the two halves of the orbit seem to belong to different ellipses, which implies the same suspicion.

"563, α *Centauri*. Although this pair has gone through more than a whole revolution since its discovery, and has been carefully watched by myself through nearly 100° of its course, the elements of the true orbit are still in some uncertainty. There is evidently some disturbing force causing a sensible deviation from elliptic motion, as has been already pointed out by me in the *Monthly Notices* for March 1856, and the apparent path still continues to differ little from a straight line. The area annually described by the radius vector, which had fallen from nearly 2″·00 in 1848 to 1″·62 in 1855, is now again on the increase, but until the extremity of the ellipse has been passed we can scarcely hope for a good approximation to the true elements.

"573, α *Herculis*. In the Appendix to the Madras volume of Results for 1848–52 is given a discussion of the parallax of

this star as deduced from a series of observations taken in 1851–4. Another pretty extensive series has now been completed with a result as near to the former as could be expected. The observations are not quite so closely accordant as in the first series, the weather having been somewhat less favourable, and the observer from the state of his health not being in quite so good practice, yet the course of the parallactic curve at different seasons of the year is traced out nearly as well as before.

"A discussion of the angles of position gives for the value of the constant of parallax, $x = + 0''{\cdot}0692 \pm 0{\cdot}0080$. A similar discussion of the distances gives $x = + 0''{\cdot}031 \pm 0{\cdot}0136$. Combining the two values of x, according to their weights the definitive value is $x = + 0''{\cdot}0616 \pm 0{\cdot}0075$, a result almost identical with that obtained in 1853."

RECENT PUBLICATIONS.

Zeitschrift für populäre Mittheilungen aus dem Gebiete der Astronomie und verwandter Wissenschaften, Herausgegeben von Professor C. A. F. Peters, Director der Sternwarte in Altona. Band i. Heft i. Altona, 1858.

The Editor states, in a short preface, that this periodical, which may be considered as a continuation of Schumacher's *Jahrbuch*, is to be devoted to essays on astronomical and physical subjects. The present number contains a paper by Professor Quetelet on the Periodical Appearances of Planets, and a paper by Professor Hansteen on Terrestrial Magnetism.

Annales de l'Observatoire Impérial de Paris. Par U. J. Le Verrier, Directeur de l'Observatoire. Tome troisième. Paris, 1857.

The first two volumes of this important publication have been already noticed in the Annual Report of the Council for 1857. The present volume commences with M. Villarceau's valuable researches on the problem for determining the orbit of a planet or comet, a brief account of which was given in the *Monthly Notices* for last November. The remaining part of the volume is devoted to a continuation of M. Le Verrier's astronomical researches, as given in the first and second volumes. We find in it a thorough investigation of the theory of the periodic comet of 1770, an exposition of the principles relative to the construction of tables of the movements of the planets, the formation of ephemerides, and the comparison of theory with

observation. There are two appendices to the volume, containing the transits of fundamental stars observed at Greenwich between 1750 and 1830, and at Königsberg between 1814 and 1830, with the resulting clock-errors.

Verhandelingen der Koninklijke Akademie van Wetenschappen. Tweede Deel. Amsterdam, 1855.

This volume contains a paper by Dr. Oudemans, in which he determines the orbit of D'Arrest's Comet, by a discussion of the totality of the observations made on the occasion of its passage of the perihelion in 1851.

Uber die Existenz eines widerstehenden Mittels im Weltraume. Von J. F. Encke, Director der Berliner Sternwarte. Berlin, 1858.

This is a separate publication of a paper which appears in the Berlin *Jahrbuch* for 1861. In a short introduction, Professor Encke gives a general explanation of its nature and of the circumstances which gave rise to his researches on the existence of a resisting medium. Upon his discovery, in 1819, of the periodicity of the comet which bears his name, he expressed his opinion that the time of revolution was gradually becoming shorter. The twelve perihelion passages of the comet which have since elapsed have served to confirm this conjecture. In every instance the return of the comet has been calculated beforehand upon the supposition of the existence of a resisting medium, without which its rediscovery, except perhaps on three occasions, would not have been effected. The author, accordingly, does not entertain the slightest doubt respecting the legitimacy of his hypothesis. His researches on the subject are mainly contained in the volumes of the Berlin Academy of Sciences for 1829, 1831, 1833, 1836, 1842, 1844, 1851, and 1854. Abstracts of these papers are to be found in various numbers of the *Astronomische Nachrichten*. In Nos. 210–11 (Vol. IX.), of that periodical there appears an original paper on the subject. The volumes of the Berlin *Jahrbuch* for 1822 and 1823 also contain some results of Professor Encke's earlier labours on the motion of the comet considered with reference to a resisting medium.

The paper now before us is divided into seven sections, in which the existence of a gradual diminution of the time of revolution of the comet is proved,—

1st. From the close agreement of the observations of the comet with the results of calculations executed beforehand on the supposition of a resisting medium.

2d. From researches on the observations of 1819–38.

3rd. From researches on the observations of 1819–48, and the agreement of the results with those of 1819–38.

4th. From the impossibility of representing the motion of the comet during the period 1819–48 by the existing theory of gravitation.

5th. From the close agreement of the observations previous to 1819, and subsequent to 1848, with the results derived from 1819–48.

6th. From an examination of the observed perihelion passages and the corresponding results freed from planetary perturbation.

In the seventh and last section Professor Encke briefly explains the reasons by which he has been led to account for the anomaly in the comet's motion by the hypothesis of a resisting medium.

The author finally remarks in the introduction to his paper that no new calculations have been considered necessary on the present occasion. All the numerical values employed have been already given in the memoirs above referred to. Only a few results relating to the two last perihelion passages of 1855 and 1858 have been added to them.

§ 1. It has been stated that, soon after his discovery of the periodicity of the comet in 1819, Professor Encke, upon comparing the perihelion passage of that year with the earlier perihelion passages of 1805, 1795, and 1786, discovered that the time of revolution, when freed from the effects of planetary perturbation, was gradually becoming shorter.—(Berlin *Jahrbuch*, 1822, p. 200; 1823, p. 215). After the rediscovery of the comet in 1822, the perihelion passages for 1825 and 1828 were calculated on the supposition of a resisting medium; and in Nos. 210–11 of the *Astronomische Nachrichten*, the author has given a complete investigation of the subject, and has supplied the arguments and formulæ to which he has since invariably adhered. In that paper Professor Encke supposes the resistance offered by the medium to be proportional to its density and to the square of the velocity of the moving body: he moreover supposes the density of the medium to vary inversely as the square of the distance from the sun. He accordingly expresses the action of the resisting medium upon the comet by the following tangential force:—

$$U' = U \cdot k^2 \left(\frac{2}{r} - \frac{1}{a}\right) \cdot \frac{1}{r^2}$$

U being a constant depending on the resistance, k the Gaussian constant of the *Theoria Motus*, a the mean distance of the comet, and r the radius vector.

Professor Encke collects together all the results relative to the times of perihelion passage which have been observed during the 72 years comprised between 1786 and 1858, and he

proposes the problem to account for the motion of the comet during that period. Previous to 1819 the effects of planetary perturbation are only approximately calculated. During the period 1819–48 these effects are rigorously taken into account. In regard to the remaining period 1848–58, the minor planets occupied so much of the calculating force of the observatory that Professor Encke could only take into consideration the influence of *Jupiter*, which exercises by far the most powerful disturbance on the motion of the comet.

In consequence of the calculations which have been made beforehand on the occasion of each successive return of the comet to the perihelion, subsequently to the year 1819, no return of the comet has since been allowed to pass unobserved. In every one of such instances the hypothesis of a resisting medium has been introduced into the calculations. The following are the differences between the observed and calculated geocentric places of the comet at the time of its rediscovery on the occasion of each return to the perihelion:

	′		′		′		′
1822	2·0	1832	2·2	1842	0·9	1852	0·5
1825	2·3	1835	1·3	1845	0·8	1855	8·2
1828	3·0	1838	2·0	1848	3·7	1858	0·5

The large error for 1855 occurs in a year for which the perturbations are only approximately given. There occurs also a slight error in the calculation for that year.

The author here remarks that since the time of the comet's perihelion passage, setting out from the year 1819, occurs earlier on each successive occasion than it ought to do according to the existing theory of planetary motion, and that to the extent of

0·5 days in 1838
2·0 ,, 1848
4·5 ,, 1858

So there lies in the agreement between the observed places of the comet and the corresponding places calculated beforehand on the hypothesis of a resisting medium, the strongest indirect proof which could be desired in favour of such an hypothesis. In the year 1858 the error of 4·5 days in the time of the perihelion passage would have caused an error of 3°·4 in the geocentric place of the comet. "It has not been possible," says the author, "to institute a similar comparison with respect to any other periodic comet. Astronomers have satisfied themselves with merely assigning the place in general where the comet was to be sought for, and have contented themselves with a rough approximation. A similar investigation undertaken with respect to the other periodic comets is much to be desired. Each of such investigations would have its peculiar difficulties, arising from the circumstance of the time of revolution being longer than in the present case, and consequently there being fewer observed passages of the perihelion."

In the second section the author investigates the motion of the comet for the period 1819–1838, comprising seven revolutions. The perturbations are computed in both directions from the perihelion passage of 1822, (Jan. 9·72). Normal places are obtained by means of the observations, and 52 equations of condition are formed, involving eight unknown quantities, viz. the corrections to the six elliptic elements, a correction to the mass of *Mercury*, and a correction to the quantity U (approximately assumed in the first instance) depending on the action of the resisting medium. The treatment of these equations by the method of least squares gives new values of the mass of *Mercury* and of U, with which the perturbations of the mean motion and mean anomaly are computed afresh for each perihelion passage. With the resulting values of these elements and the values of the other elements previously deduced, the places of the comet are calculated for the instants corresponding to the normal places furnished by the observations; and in this manner a comparison is instituted between the calculated and observed places. The mean value of the residual errors is 19″·92. Upon this point the author makes the following remarks:—

"This mean error is certainly greater than what we should be justified in expecting from the excellence of the greater number of the observations. Its magnitude, however, mainly depends on the uncertainty affecting the observations after the perihelion passages of 1822 and 1832. If we reflect further that the motion of the comet is represented during a period of twenty years, in the course of which it is difficult to ascertain, in consequence of the imperfectly defined outline of the body, whether the same point had been assumed for the centre of gravity on the occasion of each of the seven returns to the perihelion, the result of the comparison between theory and observation must be regarded as very satisfactory. Moreover, if we take into consideration the circumstance that in order to obtain this degree of accordance it has been found necessary to apply to the mean anomaly a correction depending upon U, proportional to the square of the time, and amounting to 540″ for the period 1819-38, a correction which is altogether foreign to the theory of elliptic motion, and in some instances produces a displacement of $2\frac{1}{2}°$ in the geocentric place of the comet, we hereby obtain a direct proof that the hypothesis in its general form is necessary throughout."

In the third section Professor Encke exhibits the results of a similar investigation of the motion of the comet, founded on the observations made during the period 1819–48, comprising ten perihelion passages. The definitive values of the elements agree closely with those derived from the previous investigation. The mean value of the residual errors amounts to 26″·02. If the observation of 1842 May 18·63 be excluded the mean error amounts to 22″·73.

In the fourth section the author shows that if the expression for U be excluded from the investigation, nearly the same results will be obtained for the period 1819–48, if a system of elements be used in which the mean motion varies in the direct proportion of the time, and the mean anomaly in the proportion of the square of the time. He proves that it is utterly impossible to represent the motion of the comet by a system of elements in which the purely elliptic values of the latter elements are employed. In the fifth section the observations previous to 1819 and subsequent to 1848, are compared with the theoretical results founded on the researches for the period 1819–48 and satisfactory results are obtained.

In the sixth section the author institutes some interesting comparisons illustrative of the gradual shortening of the time of revolution of the comet, and the adequacy of his theory to account for the result. We extract the following table:—

	$\tau_n - \Delta\tau_n$	Diff.	$a f^2$	Regular Periods.
1786	−15747·66		+ 9·46	−15738·20
{1789}	−14534·87	1212·79	8·06	−14526·81
{1792}	−13322·20	1212·67	6·77	−13315·43
1795	−12109·65	1212·55	5·59	−12104·06
{1799}	−10897·21	1212·44	4·53	−10892·68
{1802}	− 9684·88	1212·33	3·58	− 9681·30
1805	− 8472·66	1212·22	2·74	− 8469·92
{1809}	− 7260·56	1212·10	2·01	− 7258·55
{1812}	− 6048·56	1212·00	1·40	− 6047·16
{1815}	− 4836·67	1211·89	0·89	− 4835·78
1819	− 3624·89	1211·78	0·50	− 3624·39
1822	− 2413·23	1211·66	0·22	− 2413·01
1825	− 1201·68	1211·55	0·06	− 1201·62
1829	+ 9·76	1211·44	0·00	+ 9·76
1832	+ 1221·08	1211·32	0·06	+ 1221·14
1835	+ 2432·30	1211·22	0·22	+ 2432·52
1838	+ 3643·41	1211·11	0·50	+ 3643·91
1842	+ 4854·39	1210·98	0·89	+ 4855·28
1845	+ 6065·27	1210·88	1·40	+ 6066·67
1848	+ 7276·04	1210·77	2·01	+ 7278·05
1852	+ 8486·69	1210·65	2·74	+ 8489·43
1855	+ 9697·24	1210·55	3·57	+ 9700·81
1858	+10907·68	1210·44	+4·52	+10912·20

The second column of the foregoing table exhibits the number of days, less the effects of planetary perturbation, included between the various observed passages of the perihelion extending from 1786 to 1858, and the perihelion passage of 1829. The third column exhibits the differences of those

numbers, which would be constantly of the same value and equal to the time of revolution of the comet if its motion was solely dependent on the force of gravitation. The fourth column contains the numbers, with reversed signs, depending on the square of the time, which the author has deduced from the hypothesis of a resisting medium. These numbers, when applied to those in the second column, give the numbers in the fourth column, which exhibit a series of intervals regularly increasing in each direction from the perihelion passage of 1829, conformably to the principles of elliptic motion. It is plain, therefore, that in the present state of the observations of the comet, the term αt^2 suffices to account for the anomaly in its motion. It ought to be remarked that slight corrections have been applied to the perihelion passages previous to 1819 and subsequent to 1848, in consequence of the imperfect computation of the perturbations for those periods. The perihelion passages included between the intervals 1786–95, 1795–1805, and 1805–19, were not observed.

In the seventh and last section of his investigation Professor Encke states the reasons by which he was led to account for the anomaly in the comet's motion by the hypothesis of a resisting medium. The probability of the existence of such a medium was supported by the authority of Newton, who merely assumed its non-existence on the ground of its not exercising any sensible influence on the motions of the planets. It is not surprising, however, that such should be the observed result when we take into consideration the condensed masses of the planets on the one hand, and on the other the loose structure of the comet, which exhibits no trace of a determinate nucleus. But what appeared to Professor Encke to constitute the most powerful argument in favour of a resisting medium was this,—the anomaly appeared to consist solely in a gradual increase of the mean motion of the comet, which was explicable by a force continually acting in the direction of a tangent to the orbit; and this again was in exact accordance with the effect which would be produced by a resisting medium. As regards other explanations which had been offered, the effects were of a periodical nature, and, therefore, could not account for the phenomena in the present instance.

ERRATA.

Vol. XIX., No. 1, page 1, line 1, *for* Jackson, *read* Graham.

— — — 2, *for* Nicholl, *read* Nichol.

— — 18, line 7 from the bottom. The sentence beginning with "To the naked eye," &c., and ending with "*Ursa Major*," should have been attached to the observations of Oct. 5 instead of those of Oct. 2.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *January* 14, 1859. No. 3.

ARTHUR CAYLEY, Esq., in the Chair.

William Radcliff Birt, Esq., 11 Wellington Street, Victoria Park,

was balloted for and duly elected a Fellow of the Society.

On the Variable Star U *Geminorum.* By J. Baxendell, Esq.

After remaining invisible for about twelve months, this extraordinary and highly interesting variable suddenly reappeared in the middle of November last, and passed through a maximum of unusually short duration; the whole time of visibility not exceeding *nine* days. My observations of its magnitude on different nights during this short apparition were as follows:—

	d	h	m	
1858, Nov.	12	12	0	14·0 magnitude
	15	11	30	9·3 ,,
	16	11	0	9·5 ,,
	17	11	30	9·8 ,,
	18	13	0	10·6 ,,
	21	12	30	below 13·3 ,,

These magnitudes were derived from careful comparisons of the variable with several of Mr. Pogson's comparison stars (*Monthly Notices*, vol. xvii. p. 202), and also with three faint

stars not included in Mr. Pogson's list, the magnitudes of which I have determined photometrically with the 13-inch reflector of Mr. Worthington's observatory. As showing the degree of reliance to be placed on these magnitudes, I may remark that the greatest difference between my photometrical results and Mr. Pogson's for the stars included in his list does not exceed *two-tenths* of a magnitude.

From the above observations it appears that between the 12th and the 15th of November the brightness of U *Geminorum* increased through 4·7 magnitudes; or at the extraordinary rate of fully *one magnitude and a half* per day! The rate of decrease, though less striking, was still remarkable, averaging between the 16th and 21st of November not less than *three-fourths* of a magnitude per day.

The liability of the star to these rapid changes of brightness at its maximum may, probably, be the chief reason why it escaped observation at the expected times of its reappearance in January and May last; as a very short, and possibly also very low, maximum might occur during a few days of unfavourable weather, or when hazy skies were prevalent, and the moon at the same time inconveniently near the place of the star.

When the variable was at its maximum brightness on the 15th of November, I remarked that it was much less sharply defined than Mr. Pogson's comparison stars *b* and *c*, to which it was then equal in brightness; and that, compared with them, it had a somewhat hazy or nebulous appearance. This hazy appearance was also remarked on the 17th while viewing the star with Mr. Worthington's large reflector. During the whole time of the star's visibility, its colour, which was *white*, remained unchanged.

The next maximum of U *Geminorum* may be expected to occur between the 18th and 26th of February; and it is very desirable that it should then be more generally looked for than it appears to have been on past occasions, in order that it may not wholly escape observation in consequence of the prevalence of unfavourable weather in particular localities.

Manchester, Jan. 17, 1859.

Observations of Donati's Comet. By William Lassell, Esq.

(*Abstract.*)

I send some drawings of Donati's Comet with some observations in continuation of those which appeared in page 21 of the current volume of the *Monthly Notices*.

No other favourable opportunity of observation occurred

until the 30th September. *Arcturus* and the comet were at about the same altitude, and the tail could be traced nearly far as the nearest bright star (η) of the *Great Bear*. The sky remained clear for only a short interval, and did not permit any view with either of the equatoreals.

On the 3d Oct. the comet was surveyed in the 20-foot equatoreal from about $6\frac{1}{4}$ to $7\frac{1}{4}$ hours. The following measures were taken along an imaginary line drawn through the nucleus at right angles to the direction or axis of the tail,

Diameter of nucleus .	6·74
,, ,, 1st envelope .	30·11
,, ,, 2d envelope .	102·2

On the following evening, the 4th, the change of aspect was marvellous. The black spot is an unexpected and surprising phenomenon. Unfortunately the sky shortly clouded, only affording time for the following measures to be rather hastily taken:

Diameter of nucleus .	6·68
,, ,, 1st envelope .	36·07
,, ,, 2d envelope .	81·37

The comet was generally not so well defined as yesterday.

On the 5th October at about $6^h\ 50^m$ P.M., the comet was surveyed briefly and uninterruptedly in the large equatoreal during intervals of storms of wind and rain.

On the 8th October there remained only a trace of the dark spot in the 2d envelope seen on the 4th and 5th insts., but the whole of this envelope had assumed a mottled appearance.

After the 8th I had no favourable view of the comet, and the last glimpse of it I saw was on the 17th Oct., about $6\frac{1}{4}$ P.M., when it presented a greatly faded aspect, at about 4 or 5° above the horizon.

P.S. Having been requested to scrutinize the immediate neighbourhood of α *Lyræ* for a minute star, or stars, which have been seen or suspected within the distance of the recognised companion, I send you the results of my examination on two favourable nights.

12th Sept. $10^h\ 40^m$ P.M. Viewed α *Lyræ* with the 20-foot equatoreal under very good circumstances, with various powers from 155 to 650, without being able to detect any star nearer than the well-known two, which with α form a nearly right-angled triangle with very unequal sides. The air steadier and the sky brighter than usual. The above two companions were very bright, and the search for others careful.

9th Dec. $5^h\ 40^m$ P.M. Looked again at α *Lyræ* for a small star accompanying it within the distance of the well-known

small one. Various powers were used up to 650, but I could see no new star, although the space between α and the companion for at least nine-tenths of their distance exhibited as black a sky as any part of the field. I am, therefore, unable to furnish any corroborative evidence of the existence of the suspected star.

Bradstones, Sandfield Park, near Liverpool,
5th January, 1859.

Magnitudes of Forty-eight of the Minor Planets for the first Day of each Month of the Year 1859. By Norman Pogson, Esq.

(*Communicated by Dr. Lee.*)

The following table, intended to facilitate the identification of the Minor Planets, has been furnished by Mr. Pogson, of Dr. Lee's Observatory, Hartwell House. It is computed upon the light-ratio 2·512 (*Monthly Notices* of the Royal Astronomical Society, Vol. xviii. p. 48).

The log. distances have been taken from the *Supplement to the Nautical Almanac* for 1862.

No.	Planet.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Mean Opp. Mag.
(1)	Ceres	9·5	9·6	9·5	9·4	9·2	9·0	8·6	8·3	8·1	8·2	8·5	8·8	9·1	7·7
(2)	Pallas.. ..	10·2	10·2	10·2	10·0	9·8	9·6	9·3	9·1	9·1	9·3	9·6	9·8	10·0	7·9
(3)	Juno	10·7	10·4	10·2	9·9	9·9	10·0	10·3	10·7	11·0	11·1	11·2	11·2	11·2	8·7
(4)	Vesta	8·3	8·3	8·3	8·3	8·1	7·9	7·7	7·3	7·0	6·9	7·1	7·5	7·9	6·6
(5)	Astræa ..	9·7	9·3	9·0	9·1	9·6	10·1	10·6	11·0	11·3	11·6	11·8	11·9	11·9	10·0
(6)	Hebe	10·5	10·3	10·1	9·9	9·5	9·2	8·7	8·2	7·7	7·4	7·5	7·9	8·4	8·6
(7)	Iris	9·5	9·6	9·6	9·6	9·6	9·6	9·6	9·5	9·4	9·3	9·2	8·9	8·7	8·6
(8)	Flora	11·1	10·9	10·5	10·1	9·7	9·7	9·9	10·3	10·6	10·8	10·9	11·0	11·0	8·9
(9)	Metis	10·4	10·2	9·8	9·5	9·4	9·7	10·0	10·4	10·7	11·0	11·1	11·2	11·2	8·9
(10)	Hygeia ..	10·2	10·3	10·6	10·8	11·0	11·2	11·3	11·3	11·2	11·1	10·9	10·6	10·2	9·8
(11)	Parthenope	10·8	10·9	11·0	11·1	11·1	11·0	10·9	10·7	10·5	10·1	9·8	9·7	9·9	9·5
(12)	Victoria ..	10·7	10·8	11·2	11·6	11·9	12·1	12·2	12·2	12·2	12·1	11·9	11·6	11·2	9·6
(13)	Egeria ..	10·9	11·2	11·4	11·5	11·5	11·5	11·4	11·2	11·0	10·8	10·4	10·1	9·7	9·9
(14)	Irene	9·6	9·2	8·8	8·7	8·9	9·4	9·8	10·2	10·6	10·9	11·1	11·2	11·3	9·7
(15)	Eunomia ..	11·2	10·9	10·6	10·2	10·0	10·0	10·2	10·5	10·7	10·9	11·0	11·1	11·0	9·1
(16)	Psyche ..	11·7	11·6	11·4	11·0	10·7	10·3	9·9	9·7	9·9	10·1	10·4	10·7	10·9	10·1
(17)	Thetis ..	10·6	10·7	11·0	11·3	11·6	11·8	11·9	11·9	11·9	11·8	11·6	11·3	11·0	9·9
(18)	Melpomene	11·9	11·6	11·3	10·8	10·3	9·7	9·4	9·3	9·5	9·8	10·0	10·2	10·4	9·2
(19)	Fortuna ..	11·9	11·7	11·3	10·9	10·5	10·3	10·3	10·5	10·8	11·1	11·3	11·4	11·4	9·3
(20)	Massilia ..	11·7	11·6	11·5	11·2	10·9	10·5	10·1	9·9	10·0	10·3	10·6	10·9	11·1	9·3
(21)	Lutetia ..	12·0	11·7	11·3	10·9	10·4	9·8	9·4	9·4	9·7	10·1	10·5	10·9	11·2	10·3

Planet.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Mean Opp. Mag.
Calliope ..	11·4	11·1	10·9	10·9	11·2	11·5	11·8	12·1	12·3	12·4	12·5	12·5	12·4	10·6
Thalia ..	13·1	13·1	13·0	12·8	12·5	12·2	12·1	12·2	12·4	12·7	13·0	13·3	13·4	11·0
Themis ..	12·1	11·8	11·5	11·3	11·3	11·6	12·0	12·3	12·6	12·9	13·0	13·1	13·1	11·6
Phocea ..	11·7	11·9	12·3	12·6	12·9	13·1	13·2	13·2	13·2	13·1	12·9	12·6	12·2	10·5
Proserpine..	12·2	12·4	12·5	12·6	12·5	12·4	12·2	12·0	11·7	11·5	11·2	11·3	11·6	10·7
Euterpe ..	12·1	12·0	11·7	11·4	11·1	10·8	10·8	11·1	11·5	11·8	12·0	12·2	12·3	9·9
Bellona ..	10·6	10·3	10·1	9·8	9·8	10·0	10·4	10·8	11·2	11·5	11·7	11·8	11·8	9·8
Amphitrite	11·1	10·9	10·7	10·4	10·0	9·6	9·4	9·4	9·6	10·0	10·3	10·5	10·7	9·1
Urania ..	10·3	10·7	11·1	11·4	11·6	11·8	11·9	11·9	11·9	11·9	11·7	11·5	11·2	10·1
Euphrosyne	13·6	13·6	13·5	13·4	13·1	12·8	12·5	12·3	12·1	12·1	12·3	12·5	12·7	11·6
Pomona ..	12·1	12·4	12·7	12·8	12·9	12·9	12·9	12·7	12·5	12·3	11·9	11·5	11·1	11·0
Polyhymnia	11·7	11·6	11·5	11·4	11·3	11·2	11·1	11·0	10·8	10·6	10·4	10·5	10·9	11·4
Circe	11·3	10·9	10·9	11·2	11·6	12·0	12·3	12·6	12·8	12·9	13·0	13·0	13·0	11·5
Atalanta ..	15·3	15·1	14·8	14·5	14·1	13·7	13·4	13·3	13·5	13·6	13·8	13·9	13·9	12·9
Fides.. ..	12·7	12·6	12·4	12·2	11·9	11·5	11·0	10·6	10·4	10·6	10·8	11·1	11·3	10·5
Leda	12·6	12·7	12·7	12·6	12·5	12·3	12·0	11·6	11·2	10·7	10·4	10·3	10·6	10·9
Lætitia ..	9·8	10·1	10·3	10·5	10·6	10·7	10·7	10·6	10·5	10·3	10·0	9·7	9·5	9·3
Harmonia ..	9·6	9·4	9·4	9·8	10·2	10·6	10·9	11·1	11·2	11·3	11·3	11·2	11·1	9·1
Isis	12·2	11·8	11·7	11·8	12·0	12·3	12·5	12·7	12·7	12·7	12·6	12·4	12·2	10·6
Ariadne ..	11·1	11·6	12·0	12·3	12·5	12·7	12·7	12·7	12·6	12·4	12·1	11·7	11·2	10·0
Nysa	12·0	12·2	12·2	12·2	12·1	12·0	11·8	11·6	11·2	10·8	10·4	9·9	9·5	10·4
Eugenia ..	12·3	12·6	12·8	12·9	13·0	12·9	12·8	12·7	12·4	12·1	11·8	11·5	11·4	11·0
Hestia ..	12·6	12·8	13·2	13·7	14·1	14·3	14·6	14·7	14·7	14·7	14·6	14·4	14·2	12·4
Aglaia ..	12·1	12·3	12·7	13·0	13·3	13·4	13·5	13·6	13·5	13·4	13·2	12·9	12·6	11·5
Doris	11·1	11·5	11·8	12·1	12·3	12·5	12·6	12·6	12·6	12·5	12·3	12·1	11·8	11·4
Pales	10·5	10·6	10·9	11·4	11·8	12·1	12·4	12·6	12·7	12·7	12·7	12·5	12·3	10·9
Virginia ..	13·4	13·4	13·3	13·6	13·9	14·3	14·6	14·8	14·9	15·0	14·9	14·8	14·6	12·3

On Certain Phenomena in the Motions of Solar Spots.

By R. C. Carrington, Esq.

The present short paper is in continuation of a preceding one inserted in the number for November, my object being to illustrate graphically and otherwise the divergence of individual spots in groups of small extent, and to establish clearly the circumstance that instances occur of change of position of detached spots, not explicable by change of form, but due to drift on the photosphere. It will then be seen that in any investigation of the elements of rotation of the sun, the movements observed in groups must be excluded as liable to special perturbations; that the discordances of the periods of rotation of the photosphere found by many previous investigators are

in great measure due to the omission of the consideration of the latitude of the spots discussed, and that we have arrived at some grounds for the recognition of currents on the sun. I am compelled thus to treat the subject by portions from want of leisure at the present time to enter upon it more completely.

In the first of the two lithographed sheets which accompany this paper, I have given three instances of the divergence of the components of groups, and the action thus illustrated by diagrams drawn to scale is common to all groups of small extent. The fourth example is of two neighbouring spots which have broken out at a sufficient distance apart for this action to be nearly imperceptible. The fifth set of figures is an example of a normal circular spot separating completely into two, which continue to diverge after separation. I have two other similar cases recorded, though not of so striking a character, nor so favourably observed; and the last plate but one (his Taf. IX.) of Mr. Schmidt's *Resultate* supplies another. In the lithographed sheet the observations of successive days are placed vertically under one another, commencing from the top when the spot came first into view, the middle diagram being the observation on the day when the group was nearest to the centre of the disk in its passage across from *right to left*, the figures and the apparent movements being reversed as in all my representations. The squares are of 10 degrees in longitude and latitude, and the longitude increases from right to left.

I have acknowledged before that Dr. C. H. F. Peters, now Professor of Astronomy at Hamilton College in the United States, has the right to claim priority of publication of this action upon one another of the individuals of a group of spots, though, as the examples now given will show, it could not have escaped me in the examination of my own records. There are two ways in which this action may be mechanically explained; firstly, by the centre of eruption beneath the photosphere being situated between the openings in the photosphere which we see as dark spots, and its emissions of gaseous or other expansive matter being thrown out obliquely, an explanation which is strongly supported by instances occasionally occurring in which an elliptical group of numerous small nuclei have all their penumbræ turned (or thrown) outwards; secondly, by the pressure of the necessarily superincumbent matter of the photosphere thrown out of the cavity of the spots, and which, as the emissive force dies out, must, in its efforts to resume its general level, exert a lateral pressure. The mechanics of the subject may, however, wait awhile till observation has further advanced; my object at present being chiefly to show that groups present peculiarities of internal commotion which unfit them for discussion in the correction of the elements of rotation.

In the second lithographed sheet are given some series of

positions of neat single spots, favourably observed, in order to show in the briefest way, and at the same time conclusively, that no single value of the time of rotation of the photosphere will satisfy the successive observed longitudes of normal spots, and that the outstanding movements are such as are only referrible to proper motion of the nature of drift of the entire spot. The period of sidereal rotation with which the reductions have been effected is 25·380 mean solar days; and it will be seen that while spots (6) and (7) move to the left, or increase their longitude from the adopted fixed meridian, spots (8), (9), and (10), move to the right, their longitudes decreasing, and that these motions are of an amount which necessitates the supposition of bodily drift. It were easy to add the numerical positions of these five series, but I do not see that figures would here add anything to the force of the ocular demonstration.

I have proceeded to select out of my stock of results all series of observed positions of single spots recorded on more than two days, to which no *a priori* objection existed, and to deduce for each series the mean daily drift of the spot in longitude and latitude, expressed in minutes of arc on the surface of the sun, and arranged the results in order of the latitude of the spots from north to south. As I do not consider the subject fully ripe, I will only now give the mean results of contiguous values, remarking that the groups have been made as usual with reference to the tendency exhibited to change of sign, and that the signs within the limits of the several groups presented a remarkable consistency. The resulting means are as follow:—

Latitude.	Daily Drift.		
°	′	′	
30 N	− 25 in longitude	+ 5 in latitude, by	4 series
18 N	− 14 ..	+ 1 ..	4 ..
8 N	+ 8 ..	− 5 ..	14 ..
11 S	+ 10 ..	− 3 ..	6 ..
19 S	− 10 ..	+ 1 ..	5 ..
29 S	− 21 ..	+ 4 ..	8 ..

There is a mean excess of retrogradation in longitude of 9′ a day with the period 25·380, which was employed provisionally in reduction. If we correct this period for the 9′, we have 14° 2′ for the daily sidereal rotation, and $25^{d}\cdot652$ for the corresponding period; but though this is the mean period which results, and happens to agree exactly with the mean period of Dr. C. H. F. Peters, it necessarily is affected by the mean drift, and may be not a whit nearer the truth than the one provisionally used, from the convenience of its ready subdivision. The numbers exhibited in the foregoing little table are however very significant, when the method of their derivation is duly borne in mind. They show that with a mean period of rotation there is an equatoreal current causing spots to move in the direction of

the solar rotation, and a reverse current in the higher north and south, between 15° and 40°, causing the those latitudes to apparently regress. It now appears when the disclosure of my former map, in the *Monthly Notice*, is remembered, how former observers have disagreed in their concluded periods, from having at different epochs when the limiting parallels were d each case, and when the considerations of latitude were omitted. It further results from the above between the parallels of 15° N. and S., the mean latitude is towards the equator, and beyond 15° of l thereabouts towards the poles in each hemisphere, an differences of drift in longitude are far larger than latitude. The effect of the mean drift in longitude is had I inferred a mean period of sidereal rotation observations of 1854 and 1855, there would have $25^{d}{\cdot}11 \pm$, and from the subsequent period after the distribution exhibited in my map, a period of 25^{d}· question may hence arise whether at the epoch of the change of distribution in latitude, a simultaneous currents did not set in, but our records are not yet s supply materials for the discussion of such speculatio

In Dr. Peters's Tables, A and B, there are mate similar investigation. We are at variance in at leas namely, that he finds the movement in latitude towards the equator; and I am uncertain whether l result for longitude is in accordance with my own o cannot make sure from his words in which direction his meridians. I may have occasion to return to t when I have had time to correspond with him on thi other points; and in the meantime may be permitted reader's attention to a certain similarity of this dist currents with the diagram of the winds of our glob Plate III. of Lieutenant Maury's Sailing Directions.

1858, *December* 27.

Results of the Observations of Small Planets, m Royal Observatory, Greenwich, in the month of 1858.

(*Communicated by the Astronomer Royal.*)

Thetis.

Mean Solar Time of Observation.	h m s	Apparent R.A.	h m s	App
1858, Dec. 29	12 3 3·3	6 35 38·24		7

Urania.

Mean Solar Time of Observation.	h m s	Apparent R.A.	h m s	App
1858, Dec. 20	6 51 32·2	0 47 46·99		8

Leucothea.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1858, Dec. 2	11 25 7·4	4 11 9·10	57 38 14·09

Lætitia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1858, Dec. 2	7 7 19·3	23 52 38·58	99 39 22·16

Ariadne.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1858, Dec. 2	10 14 13·2	3 0 3·23	70 13 39·21

Note.—The above observations of *Thetis* and *Leucothea* are published on account of their near agreement with the Ephemerides of Schönfield and Schubert respectively, but from the continued unfavourable state of the sky, no other observations could be secured at Greenwich to compare with them.

Extract of a Letter from Prof. R. Wolf, of Zurich, to Mr. Carrington, dated Jan. 12, 1859.

(*Translation.*)

" In my eighth communication on the subject of the solar spots now ready for the press, I intend partly to give in detail my observations during the year 1858, and partly to continue the researches commenced in the seventh number. I shall accordingly show, by employing, on the one hand, my own observations in the year 1849 to 1858; and on the other, extracts from the observations of Schwabe in the years 1826 to 1848, that the formula

$$M = 50{\cdot}31 + 3{\cdot}73 \left\{ \begin{array}{l} 1{\cdot}68 \sin 585^\circ{\cdot}26\, t + 1{\cdot}00 \sin 360^\circ\ \ t + \\ 12{\cdot}53 \sin 30^\circ{\cdot}35\, t + 1{\cdot}12 \sin 12^\circ{\cdot}22\, t \end{array} \right\}$$

in which t denotes the number of years elapsed since a period of mean spot-frequency, gives a curve very similar to the sun-spot-curve; and therefore is very fit to be taken as the foundation of the more detailed research which I have now in hand. Now, as the coefficients of the four sines are the values which the fraction $\frac{m}{r^2}$ assumes, when for m and r are successively substituted the masses and mean distances of *Venus, Earth, Jupiter, and Saturn;* and the angles of the four sines are the values of $\frac{360^\circ}{t}$, when for t are substituted the periodic times of the same planets, the conclusion seems to be inevitable, that my

conjecture that the variations of spot-frequency depend on the influences of *Venus, Earth, Jupiter, and Saturn*, will not prove to be wholly unfounded. The preponderating planet *Jupiter* will in such case mainly determine the length and height of the wave of the spot-period; *Saturn* will cause small variations in the length and height; and, finally, the earth and *Venus* will change the smooth wave-line into a rippled one. Further investigation may decide this important question either affirmatively or negatively; but sketched out merely as it now is, I believe it will be found to have considerable interest.

"You will oblige me by bringing this contribution of mine to the knowledge of the Royal Astronomical Society. . . ."

On the Visibility of Donati's Comet. By R. Hodgson, Esq.

Aug. 30th. The comet was barely visible with the naked eye, though easily found with an opera-glass.

Oct. 5th. Daylight, 11 A.M. *Arcturus* bright as a point; but the comet invisible, though the clock carried the telescope, (6¼ inches aperture; 7 feet 6 inches in focal length). Power used, low comet eye-piece, magnifying 25 times.

Oct. 6th. Repeated the observation at 11 P.M. Covered my head with hood, and excluded all extraneous light from the eye, but could not see the comet. *Arcturus* brilliant; and *Jupiter* to be seen near the western horizon.

Oct. 9 and 10. Same time, telescope, and powers; but the comet was not to be seen.

Oct 17, 6 P.M. dark twilight. The comet visible in the finder, but invisible to the naked eye.

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*Note on a Method recently proposed by Lieut. Raper for Clearing the Lunar Distance from the Effects of Parallax and Refraction.** By J. Riddle, Esq.

If C represent the centre of the earth, O the place of the observer on its surface, S and M the places of the sun and moon (the figure is so easily conceived that it is hardly necessary to draw it). Then adopting the notation of the paper referred to, with the addition of d for the right line S M; the plane triangles S C M, S O M, C S O, and C M O, give the following symmetrical equations:—

* *Monthly Notices*, Vol. XVIII. p. 303.

$$2\,s\,m\cos\delta = s^2 + m^2 - d^2$$
$$2\,s_,m\cos\delta_, = s_,^2 + m_,^2 - d^2$$
$$2\,r\,s\sin\alpha = r^2 + s^2 - s_,^2$$
$$2\,r\,m\sin\beta = r^2 + m^2 - m_,^2$$

If the last three of these be added and the first be subtracted from their sum, we obtain,

$$\cos\delta = \frac{s_,m_,}{s\,.\,m}\cos\delta_, + \frac{r}{m}\cos\alpha + \frac{r}{s}\cos\beta - \frac{r^2}{s\,.\,m} \qquad (1)$$

the leading equation of Lieut. Raper's paper.

The third and fourth of these equations give at once the values of $\frac{s_,}{s}$ and $\frac{m_,}{m}$ or Π and Π' as they are given in that paper.

Nor is it more difficult to obtain equation (1) by spherical trigonometry, which is indeed a very convenient substitute for co-ordinate geometry in problems relating to the sphere (see solution in the note appended to this).

In a rigorous solution of the problem under consideration, it is necessary to consider the effects of parallax and refraction separately; and I suppose, that this special consideration of the effects of each is intended to be facilitated in the method proposed. In nautical practice the reasons for this separation are overruled on the ground of the simplicity gained in the solution of the problem, and the comparative insignificance of the error due to the neglect of them.

If they be neglected here also, then $\frac{s_,m_,}{s\,.\,m}$, for the determination of which special tables are suggested, is already tabulated in a sufficiently convenient form,

$$\text{for } \frac{s_,m_,}{s\,.\,m} = 2\cos N$$

when N is the "auxiliary arc" of Table 25 of the first five editions of Riddle's *Navigation*.

A slight modification of the other terms only would be required.

With a careful estimation of the moon's parallax in altitude and a combination of the corrections, both the parallax and refraction may be allowed for, in the manner suggested for the refractions alone (known as Lyons' method). The first order of differences is not however sufficient for the larger correction (for the moon's parallax); a very concise formula for the computation of this little supplementary term, which I suggested, was printed in the *Monthly Notices* some years since.

I am under the impression that this mode of proceeding is as accurate and much more concise than the method proposed;

but open still to the objection of involving too many subordinate computations.

Equation (1) *by Spherical Trigonometry.*

Let δ = true distance and δ' = apparent distance of sun and moon.
Let a, b, be the true zenith distances.
and $a'\ b'$ be the apparent zenith distances.

$$\text{Then } \cos\delta = \cos a \cos b + \sin a \sin b \cos \angle Z$$
$$\text{and } \cos\delta_{,} = \cos a' \cos b' + \sin a' \sin b' \cos \angle Z$$

And eliminating $\cos \angle Z$

$$\sin a' \sin b' \cos\delta - \sin a \sin b \cos\delta' = \sin a' \cos a \sin b' \cos b - \sin a \cos a' \sin b \cos b'$$
$$= \sin(a' - a) \sin b' \text{ as } b + \sin(b' - b) \sin a \cos a'$$
$$\therefore \cos\delta = \frac{\sin a \sin b}{\sin a' \sin b'} \cos\delta_{,} + \sin\varpi \cos\beta + \sin\varpi \cos\alpha \text{ nearly.}$$

Observations of Donati's Comet made at Haddenham, Bucks.
By the Rev. W. R. Dawes.

The following are a few extracts from this paper, which the Council will, in all probability, order to be printed *in extenso* in the forthcoming volume of *Memoirs*, along with a few others on the same subject. The author states that his observations of the comet were made with an excellent object-glass of $7\frac{1}{4}$ inches aperture, by Mr. Alvan Clark, applied to his Munich equatoreal mounting.

"1858, Sept. 12. Sky remarkably clear. Comet found with the equatoreal at $6^h\ 43^m$ G.M.T., and was at that time well and easily seen. At $8^h\ 32^m$ the visible length of the tail was $3°\ 30'$. A fine object to the naked eye. In the telescope the nucleus appears quite planetary. Long after the head had set, the tail was visible, rising obliquely from the horizon, and might have been seen all night but for the intervention of trees in the northern horizon. The comet was a beautiful object in the north-east at 15^h."

"Sept. 24. Visible in the finder of the equatoreal at $6^h\ 10^m$. The arc or sector round the southern, or *sun-ward*, side of the nucleus is finely seen to-night, though the sky is not perfectly free from haze. I cannot trace the arc round on the side opposite to the sun; it seems to come to a rather abrupt termination at about 40° beyond a semicircle, being visible to about that extent further into the tail on the eastern side than on the western. It looks like a *hood* set on awry, the tail resembling a fine gauze veil dependent from it. The narrow dark channel extending from the nucleus up the axis of the tail is very remarkable: its edges are surprisingly well defined, especially *very* near the nucleus. The comparatively sharp definition of

the eastern edge of the tail contrasts strikingly with the softness of outline on the western side."

"There is a second arc very close to the nucleus, and rather bright, and I think nearly uniformly so throughout its extent. The larger sector, on the contrary, is decidedly brighter towards its outer edge, and terminates rather sharply for such an object. A soft nebulosity or coma surrounds the larger arc, and appears to be concentric with it. Its outline is certainly not continuous with that of the tail, the apex of which falls *within* the arc of the sector.

"By measurement of a careful sketch, the middle radius of the sector (drawn from the nucleus to the middle of the arc) makes an angle with the direction of the dark channel in the tail of about 156°, reckoned round by the *east* side."

"Oct. 5. At $6^h\ 30^m$ G.M.T. *Arcturus* is just within the eastern boundary of the comet's tail, and about 20′ north of the nucleus.

"The edge of the outer sector is much more indistinct to-night than it was some days ago, and so is also the edge of the eastern side of the tail. The inner sector is astonishingly enlarged since Oct. 2. There is in it an irregular *dark* spot on the western side of the nucleus; and a little to the north of that dark spot, and nearer to the nucleus, there is a *bright* spot softly defined and less bright than the nucleus.

"At $7^h\ 11^m$ *Arcturus* is judged to be in the middle of the tail.

"At $7^h\ 15^m$ *Arcturus* is in the dark channel.

"At $7^h\ 56^m$ *Arcturus* is just out of the tail on the western side.

"The following are measurements of angles of position and distances of the nucleus of the comet with respect to *Arcturus*, the comet *preceding* the star to the *south*:—

	h m s	°	′ °
At	7 34 30	P = 208·3	D = 21 35·6
	7 39 35	205·5	
	7 40 47	...	21 53·3
	7 50 15	200·7	22 26·6

"Mr. Carrington having suggested to me to observe the angles of position of a series of tangents to the curve formed by the eastern edge of the tail, I endeavoured to do so; though my apparatus is not well suited to such an observation, for which a very low power and large field would be more appropriate than those ordinarily belonging to the parallel wire-micrometer. It is essential that the points at which the wire is made, a tangent should be pretty accurately determined; but this was not very easy to do. The mode which struck me as the most practicable was, to fix on certain differences of north polar distance from the nucleus. Having, therefore, brought

the nucleus to the cross-wires in the field, I set the declination circle half a degree northward, and observed a tangent; and so on, at intervals of half a degree. But I found that, after the fourth setting, the edge of the tail became so diffused, that the observations could not be carried further with any certainty. The results were as follows; power 96:—

At 0 30	north of the nucleus, pos. of tangent =	218·2	G.M.T. =	8 23
1 0	,,	,,	,,	216·2	,,	8 31
1 30	,,	,,	,,	213·1	,,	8 33
2 0	,,	,,	,,	212·3	,,	8 36

"The tail reaches to-night to the triangle formed by θ, ι, and κ *Bootis*, and is therefore from 30° to 35° in length. The middle radius of the *interior* sector makes an angle of about 176° with the direction of the dark channel. The *exterior* sector is not sufficiently well defined at its extremities to admit of accurate estimation of the middle point of its arc; but its direction does not differ much from that of the interior sector.

"Oct. 8. During broad daylight I set the equatoreal to the place of the comet indicated by Farley's ephemeris. On applying my eye to the telescope at $5^h\ 20^m$ G.M.T., while the sun was shining brightly into the observatory, the comet was instantly seen in the centre of the field. This was the only occasion on which I was able to detect it while the sun was above the horizon; though at the time noted it was so plainly seen that I think it probable it might have been perceived at least ten minutes sooner. On two or three days I think it might have been perceived if the sky had been free from haze; but the effect of a very slight film of haze when acted upon by the sun is fatal to the visibility of such an object.

"On examining the form of the nucleus as thus seen during sunshine, I was immediately struck with its *elongated* and even *crescent* shape, the convexity being towards the sun. This was the whole of the comet which was visible; and it certainly gave me a strong impression of its *solidity* and *opacity*, as presenting a true *phase;* and thus satisfactorily accounting for the dark line extending from it in the direction of the axis of the tail. As the daylight diminished, the distinct outline of the nucleus was rapidly lost through the brightness of the coma immediately in contact with it.

"$7^h\ 30^m$, The comet is finely seen. The bright nucleus is much smaller than it was. A small bright semicircular arch surrounds it on the sunward side, and a much larger arch, not so bright, outside of it, which becomes decidedly brighter towards its outer edge. And exterior to that, on the *western* side, and only a few seconds from it, there is a portion of a *third* arch, extending *sunwards* for a few seconds, and it is then gradually lost in the faint nebulosity which surrounds the sunward side of the larger arch.

"There is a *bright spot* of rather irregular shape (rather *pear-shaped*, with the smaller end towards the nucleus) on the western side of the nucleus; and a streak of light extending almost directly sunward from the smaller arch to the edge of the larger arch. The middle radius of the exterior sector makes an angle of about 188° with the direction of the dark channel as deduced from my sketch of its appearance.

"Oct. 11. The *interior sector* has greatly increased since the 8th. It is much *mottled*, the luminosity in different parts being far from uniform. To the south-west of the nucleus it has a very *dark* spot, rather pear-shaped, the smaller end being toward the centre; and also a *bright* spot nearly to the west of the nucleus, and only a few seconds from it, looking rather like a second, smaller, and fainter nucleus. There is also a bright irregular streak, extending sunward from the nucleus to the arc of the inner sector. The middle radius of the sector is now inclined to the axis of the tail (reckoned round the *east* side as before) at an angle of about 205°, as deduced from my sketch.

"Oct. 17, $6^h\ 18^m$. The comet is dim and hazy, but a marked peculiarity was immediately observed in the head. The nucleus is *very eccentric* (towards the *orbital* preceding side) with respect to the inner arch; and also, though not so strikingly, with respect to the out erarch. The inner arch is much smaller than that seen on the 11th. Scarcely any tail is visible in the strong twilight and so near the horizon. A bank of clouds soon covered that part of the sky, and the weather did not subsequently permit the comet to be seen at all.

"*Haddenham, Thame,*
"*January*, 1859."

Note on Saturn and his Rings, with a Sketch of the Planet. By Capt. Noble.

The accompanying sketch* is intended to indicate a feature in the detail of the planet *Saturn*, which is not delineated in any of the engravings to which I have access, and of which I can find no mention in any of the published accounts of Saturnian topography in my possession. I allude to the curvature of the shadow of the ring (or rings), on the ball of the planet. In Captain Jacob's admirable and most truthful drawing, this shadow, which, it seems to me, *should* (at the epoch of its execution) have exhibited this curvature, is represented as bounded by an edge strictly parallel to that of the exterior edge of ring A; whereas, on the night on which my sketch was made (and I have had one corroborative glimpse since), the curvature was so marked as at once to strike the eye on its

* This drawing was exhibited at the Meeting.

application to the telescope, bending downwards most noticeably towards the preceding limb of the planet. The nebulous ring was wonderfully bright, in fact (especially in the preceding ansa) was of a brilliant slate colour; and, despite the bright moonlight, five satellites were steadily seen. I may say that my drawing represents with great accuracy the appearance which the ring C presented, and always does present to me, viz. that of dipping under ring B; as I never could yet satisfactorily trace the junction of those portions of it in the ansæ with the part crossing the ball. The division in the outer ring was not seen. This may seem an unnecessary addition when the aperture of my telescope (4·2 inches) is considered; but I have on two occasions this year (Wednesday, Jan. 13th, and Thursday, Jan. 14th) glimpsed this division very fairly for some seconds together. I could not, either, see the peculiar streaking on the 19th ult. on the southern part of the ball of *Saturn*, which was visible early in 1858; nothing but the one great rust-coloured equatoreal belt being visible. The powers employed were 255 and 394: the latter only for confirmation of detail, the drawing having been made with the former.

Observatory, Forest Lodge, Maresfield, December 9, 1858.

Extract of a Letter from Professor Wolfers, of Berlin, to Mr. Johnson.

"Allow me to call your attention to an interesting phenomenon in the heavens which will take place on the 20th of July next, viz., the near approach to each other of the planets *Venus* and *Jupiter*. I believe I am the only astronomer who has noticed this rare approaching phenomenon. The approximate elements which I have computed are to be found in the *Astronomische Jahrbuch* for 1859, and in the *Astronomische Nachrichten*, No. 1161. I hope both these works are accessible to you. Greenwich is better situated with respect to this phenomenon than Berlin. The time of the nearest approach of the planets is 1859, July 20, $15^h\ 44^m$, mean Greenwich time; it will, therefore, take place before sunrise. You will oblige me by directing the attention of the Royal Astronomical Society to this phenomenon."

A letter has been received by the Astronomer Royal from Mr. Maclear, dated Royal Observatory, Cape of Good Hope, 1858, December 15, giving a good account of his observations

of Donati's Comet. The observations extend from October 11 to December 14, and are fifty-three in number. The instrumental places in R.A. and N.P.D. are given, corrected for refraction, but the results will not be available for the correction of the orbit till the stars of comparison have been observed with the transit circle. Notwithstanding the moonlight the comet was still an observable object.

Mr. Maclear remembers the comet of 1811, and considers that neither it nor Donati's comet is comparable in splendour to the great comet of 1843.

New Planet.

It appears that the planet discovered by Dr. Goldschmidt on the 9th of September, 1857, which was supposed at the time to be a rediscovery of *Daphne*,* was in reality a new planet. The discovery of this curious fact is due to M. Ernest Schubert, who having been appointed to compute an ephemeris of *Daphne* for publication in the American Nautical Almanac, found it impossible to reconcile the results of an orbit which he had calculated from the observations of 1857 with the observations made on the occasion of the original discovery of the planet in the previous year. The most recent researches on the orbit of the new planet are due to Dr. R. Luther of the Bilk Observatory. From the Bilk observation of Sept. 15, and two normal places of dates, Sept. 23 and 30, founded on the Berlin and Leyden observations, he has derived the following elements:

Elements of Planet ⓜ.

Epoch 1857, Sept. $13^h\ 0^m$ Berlin M.T.

M	...	35	55	12·3	
ϖ	...	294	57	50·5	Mean Equinox 1857·0
Ω	...	194	54	56·2	
i	...	7	56	2·3	
φ	...	13	7	17·6	
μ	...	854″·4862			
Log a	...	0·4122010			

These elements give for June 1, 1856—

Calculation.	Observation.
$\Delta\alpha = +12^\circ\ 20'$,	$\Delta\delta = -7^\circ\ 23'$

whence it is plain that the planet discovered on the 9th of September, 1857, is a distinct body from *Daphne*. This will appear still further evident from the following comparison between the eighteen observations of 1857 and the corresponding results of Dr. Luther's elements:

* First discovered on the 22d of May, 1856.

		Calculation—Observation. $\Delta\alpha$	$\Delta\delta$
		′	′
Paris (Goldschmidt)	Sept. 9	−18·6	− 7·0
Bilk	15	+ 0·3	+ 0·1
Leyden	16	− 3·7	+ 0·5
Bonn	16	+ 9·8	+ 3·2
Berlin	16	− 0·6	+ 0·6
Leyden	17	− 0·9	+ 1·8
Berlin	17	+ 6·5	+ 2·1
Cambridge (England)	18	− 1·7	+ 2·4
Leyden	20	+ 4·1	+ 6·7
Bilk	20	− 1·4	− 1·2
Leyden	22	− 0·6	+ 0·8
Leyden	23	+ 0·3	+ 1·1
Berlin	23	+ 0·9	− 1·2
Leyden	24	− 3·6	+ 4·5
Leyden	26	+ 4·5	+ 0·9
Leyden	27	+ 2·9	+ 0·7
Leyden	29	+ 12·2	+ 1·9
Berlin	30	+ 0·2	+ 10·2

Dr. Luther has calculated an ephemeris of the planet from the foregoing elements extending from Jan. 0, 1859, to Feb. 17, which will be found in No. 1175 of the *Astronomische Nachrichten.*

The fifty-fifth of the minor-planet group, discovered by Mr. Searle (*Monthly Notices*, vol. xviii. page 302) at the Albany Observatory has been named *Pandora.* The following elements are by Dr. Möller:*—

Epoch 1858, Nov. 4·3

		°	′	″
L	.....	16	7	48·7
M	.....	5	58	25·9
π	.....	10	9	22·8
Ω	.....	10	55	29·9
i	.....	7	20	44·0
φ	.....	7	59	14·2
μ	.....	769″·9607		
Log a	.....	0·4423587		

These elements refer to the equinox of the epoch.

It may be interesting to the Members of the Society to know that the living of Stone, near Aylesbury, has become vacant in consequence of the resignation of the Rev. J. B. Reade.

* Berlin *Jahrbuch* 1861.

RECENT PUBLICATIONS.

Tabulæ Reductionum Observationum Astronomicarum annis 1860 *usque ad* 1880 *respondentes, auctore J. Ph. Wolfers: additæ sunt Tabulæ Regiomontanæ annis* 1850 *usque ad* 1860 *respondentes, ab Ill. Zech continuatæ.* Berolini, 1858.

In a preface extending to 72 pages the author explains the construction of the tables contained in the first part of this work, commencing with a statement of the circumstance to which they gave rise. The tables contained in the *Tabulæ Regiomontanæ*, published by Bessel in 1830, serve for the reduction of astronomical observations included between 1750 and 1850; consequently for the computation of ephemerides they ceased to be applicable after the year 1846. Professor Zech accordingly re-calculated the tables down to 1860, using Bessel's elements; and the results, hitherto unpublished, have been employed in computing the apparent places of the stars for the Berlin *Jahrbuch*. These in their turn being exhausted, in so far as the last-mentioned object is concerned, Professor Wolfers has continued the tables from 1860 to 1880. In the execution of this task the author has generally employed new elements of reduction, but he has in all cases strictly adhered to the processes described in the preface to the *Tabulæ Regiomontanæ*, generally using Bessel's own words.

The constant of precession adopted by the author is that given by Bessel in the *Tabulæ Regiomontanæ*, but for the constants of nutation and aberration used in that work he has substituted those of Struve and Peters respectively. Besides Maskelyne's 36 stars, to which the *Tabulæ Regiomontanæ* are confined, the author has included in his tables the additional nine northern stars whose places are given in the Berlin *Jahrbuch*. New values of the mean places of the 45 stars are obtained by a comparison of the results contained in the most esteemed modern catalogues, and those derived from the researches of MM. Peters, Argelander, and Le Verrier.

Professor Wolfers' tables are followed by those of Professor Zech, extending from 1850 to 1860. The elements of these tables are the same as those employed by Bessel. The explanations contained in the introduction to the *Tabulæ Regiomontanæ* are therefore in all respects applicable to them.

Ueber die Reductionstafeln zu den Bessel'schen Zonen, die im XVII^{ten} *Bande der Königsberger Beobachtungen enthalten sind.** By Dr. Winnecke.

In this paper Dr. Winnecke shows that the reduction-tables to Nos. 497–536 of Bessel's Zones are considerably erroneous.

* *Ast. Nach.* No. 1168.

In so far as the right ascensions are concerned, the errors seem to have arisen partly from the influence of personal equation not having been taken into account, partly from mistakes committed in the computation of the hourly rate of the clock, but more especially from the effects of instrumental error having been somehow overlooked. The author has computed the following corrections, which must be applied to the right ascensions, computed by means of the reduction-tables contained in Vol. XVII. of the *Konigsberg Observations:*—

Zone.	Date.	D.	Correction.
		°	s
499	1831, April 1	+36	−0·212
501	11	32	−0·179
502	12	20	−0·092
503	22	26	−0·134
505	1832, Jan. 5	40	+0·220
506	Feb. 1	18	+0·087
507	—	28	+0·141
508	2	36	+0·191
509	—	40	+0·220
510	3	42	+0·236
511	4	44	+0·253
512	9	40	+0·220
513	—	30	+0·153
514	11	44	+0·253
515	12	38	+0·205
516	13	42	+0·236
517	14	32	+0·165
518	16	34	+0·178
519	17	40	+0·220
521	21	24	+0·118
522	22	42	+0·236
523	23	26	+0·129
524	28	22	+0·027
525	29	32	+0·055
526	April 11	28	−0·053
527	Nov. 8	38	−0·462
528	21	40	−0·496
529	27	42	−0·533
531	Dec. 24	44	−0·418
532	31	30	−0·238
534	1833, Jan. 7	32	−0·261
535	17	28	−0·199
536	21	+30	−0·219

The author gives the following new values of k for the remaining zones:—

	Zone 497.		
	h m	s	
1831, March 26	9 0	−31·681	+618
	9 30	51·063	714
	10 0	30·349	800
	10 30	29·549	868
	11 0	28·681	+922
	11 30	−27·758	
	Zone 498.		
	h m	s	
1831, March 27	9 30	−31·623	+604
	10 0	31·019	678
	10 30	30·341	743
	11 0	29·598	+790
	11 30	−28·808	
	Zone 500.		
	h m	s	
1831, April 8	9 30	−44·098	+281
	10 0	43·817	327
	10 30	43·490	366
	11 0	43·124	+401
	11 30	−42·723	
	Zone 504.		
	h m	s	
1831, May 17	12 30	−70·840	+824
	13 0	70·016	801
	13 30	69·215	+768
	14 0	−68·447	
	Zone 520.		
	h m	s	
1832, Feb. 19	5 30	−87·242	−227
	6 0	87·469	128
	6 30	87·597	−211
	7 0	−87·626	
	Zone 530.		
	h m	s	
1832, Dec. 15	2 0	+17·010	−635
	2 30	16·375	570
	3 0	15·805	−494
	3 30	+15·311	
	Zone 583.		
	h m	s	
1833, Jan. 6	2 0	+8·852	−946
	2 30	7·906	863
	3 0	7·043	−761
	3 30	+6·282	

The author obtains a confirmation of the results at which he has arrived by instituting a comparison between Bessel's observations and the corresponding star-positions in the catalogues of Johnson, Groombridge, Struve (*Posit. Med.*), and Argelander. Only in the case of zones 528 and 529 does there appear to be any difficulty in accounting for the discordance between the two sets of results.

In regard to the declinations Dr. Winnecke has recomputed the values of *d* for the above-mentioned zones, and obtained the following results:—

Zone 528.

h m	*d*	
	″	″
1 0	−174·58	
		+ 8·56
1 30	166·02	
		10·99
2 0	155·03	
		13·27
2 30	141·76	
		+15·49
3 0	−126·27	

Zone 529.

h m	*d*	
	″	″
1 30	−163·56	
		+ 9·85
2 0	153·71	
		12·21
2 30	141·50	
		+14·32
3 0	−127·18	

The author remarks in conclusion that in order to obtain the results legitimately deducible from Bessel's zones a careful revision of all the reduction-tables is absolutely necessary.

Sur les Comètes et sur l'Hypothèse d'un milieu Résistant; par M. Faye (*Comptes Rendus, Nov.* 29, 1858).

Remarques présentées par M. le Verrier au sujet de la lecture fait par M. Faye, dans la séance du 29 Novembre sur les Comètes et sur l'Hypothèse d'un milieu Résistant (*Comptes Rendus, Decembre* 6, 1858).

In the first of the above-mentioned papers M. Faye adduces various objections to Professor Encke's theory of a resisting medium, and proposes a theory of his own to account for the acceleration of Encke's comet, the existence of which he admits to have been established beyond all doubt by the researches of the distinguished Director of the Berlin Observatory. One of the points upon which he chiefly insists is this: if such a medium exists it is highly improbable that it should wholly escape observation notwithstanding its extreme rarity. In support of this assertion M. Faye computes the quantity of

matter in a comet upon the basis of a theory due to M. Roche, of Montpelier, in which the dimensions of a comet's atmosphere are deduced from its mass and its distance from the sun. M. Roche takes into account the gravitation of each particle of the cometic atmosphere towards the sun and towards the centre of the nucleus, but neglects the mutual action of the particles, and all consideration of orbital or rotatory motion. It is not difficult from such principles to obtain the radius of the exterior *surface de niveau* of the comet's atmosphere in terms of its mass. Conversely from the observed dimensions of the atmosphere, the same theory enables us to arrive at an approximate value of the mass. Applying these considerations to Donati's Comet, he finds the mean value of its mass to be 0·000000043, the earth's mass being represented by unity, and combining this result with the observed dimensions of the nucleus he finds the mean density to be 0·009, that of air being supposed equal to unity. "Thus," he remarks, "in by far the most compact and brilliant part of the comet's mass, the mean density is hardly nine times greater than that of the vacuum produced in our best air-pumps, although the aggregate mass of the nucleus was not inconsiderable, as we have just seen. However, notwithstanding this extreme rarity, the nucleus shone by a light comparable to that of a star of the second magnitude." M. Faye arrives at still more striking results by applying the same considerations to the tail of the comet. The density of the tail is found by him to be inconceivably less than that of the vacuum produced in our best air-pumps, and yet this part of the comet's structure was distinctly visible, even at its remotest distance from the nucleus. M. Faye hence concludes that if a resisting medium really existed in the planetary regions, it could hardly fail to be visible, notwithstanding its rarity, but no trace of such a medium had hitherto offered itself to the observations of astronomers.

M. Faye further remarks that if the existing medium was endued with a rotatory motion around the sun, it would obviously exercise a different effect on the motion of the comet according as it was direct or retrograde. It would also produce considerable perturbations in the plane of the comet's orbit, unless the latter coincided with the plane of rotation of the medium.

M. Faye accordingly proposes a different explanation of the observed acceleration founded on certain researches of Laplace contained in the *Mécanique Céleste*. In tome IV. page 355, *et seq.*, of that work, the author investigates the effect which would be produced on the motion of a body revolving around the sun, if the solar light be supposed to consist of a series of material particles incessantly emitted from the luminous body. The question had originally suggested itself to the mind of Laplace towards the close of the last century, in the course of his endeavours to account for the secular acceler-

ation of the moon's mean motion. He thus obtained two disturbing forces, one radial and the other tangential. M. Faye makes use of the tangential force to explain the observed acceleration of Encke's comet, and endeavours by means of the radial force, to give an account of the tails of comets in general.

M. Le Verrier, in his reply to M. Faye, remarks that the objection advanced by the latter to the existence of a resisting medium, on the ground of its producing a displacement in the plane of the comet's orbit such as could not fail to be detected by observation, is untenable. With the exception of the mean motion, all the other elements would be affected only in a very inconsiderable degree by the resistance of the medium. Again it was equally unjustifiable to argue against the existence of the medium on the ground of its not being visible. The comet of Encke could be seen only in telescopes of considerable power. It is contrary to all reason therefore to expect that the medium in which the comet revolves, and which must necessarily be much rarer than the comet, could be seen with equal facility. M. Le Verrier finally remarked that in fact M. Faye's hypothesis involved the idea of a *resistance* as well as that proposed by Prof. Encke. In considering the question of the acceleration of a planet by the agency of some cause besides gravitation, Laplace first investigated the effect which would be produced by the *resistance* of a medium pervading the planetary regions; and he remarks that if we adopt the undulatory theory of light, his analyisis would be applicable to the resistance afforded by the luminiferous ether. This is exactly the theory of M. Encke. Laplace next considers the *resistance* which would be produced in the particles of light upon the hypothesis of the corpuscular theory. At a subsequent meeting of the Academy of Sciences, M. Faye has endeavoured to support his speculations by attributing the observed phenomena of comets to the agency of a force directed *from* the sun, and referable to some unknown cause; but we have no space for further details.

Astronomische Beobachtungen auf der Königlichen Universitäts-Sternwarte Königsberg (Drei und dreissigste Abtheilung). Königsberg, 1858.

Besides the usual astronomical observations with the meridian instruments and the heliometer, this volume contains three important disquisitions connected with the operations executed at the Königsberg Observatory. The first is entitled, *Declinationes Stellarum fundamentalium ex ultimis Ill. Bessel Observationibus derivatæ, auctore Eduardo Luther.* The twenty-seventh volume of the Königsberg

Observations contains the zenith distances of the stars referred to (Maskelyne's 36 stars), derived from observations made by Bessel with Repsold's meridian circle, and reduced to the beginning of 1843 with Peters' Constant of Nutation and Struve's Constant of Aberration. In the present paper Professor Luthor has investigated the effects of the flexure of the instrument, and has calculated the coefficients of the equations of condition for obtaining a correction to the mean zenith distance of each star.

The second paper is entitled, *Beobachtungen zur Bestimmung der Längen differenz zwischen den Sternwarten von Berlin und Königsberg in den Jahren* 1856 *und* 1857. This paper contains a systematic treatment, by Dr. Wichmann, of a series of observations made for determining the difference of longitude between Berlin and Königsberg by means of the electric telegraph. The final result indicates the difference of longitude between the centre of the dome of the Berlin Observatory and Reichenbach's Meridian Circle to be $28^m\ 24^s{\cdot}07$.

The third paper is also by Dr. Wichmann. It contains the reduction of observations with the heliometer made between April 23, 1841, and April 30, 1842, for ascertaining the physical libration of the moon. The volume closes with a statement of the meteorological observations made at the Observatory between 1855, October 1, and 1856, April 1.

Annales de l'Observatoire Impérial de Paris, publiées par U. J. Le Verrier, Directeur de l'Observatoire; Observations, tome I. 4to. Paris, 1858.

This forms the commencement of a series of volumes, distinct from those devoted to the investigation of the theories of astronomy, in which M. Le Verrier proposes to include the various observations made at the Imperial Observatory, with their reduction and discussion. In a short preface he gives a brief account of the operations executed at the Paris Observatory during the present century. A regular course of meridional observations was commenced in 1800, and has been continued down to the present time, with the exception of an interruption during the years 1828–34, arising from the unsuitableness of the observing apartments. The observations were published in the *Connaissance des Temps* down to 1810, and subsequently in separate folio volumes, the last of which includes the operations executed at the observatory down to the year 1846. In all these cases the observations were inserted without any accompanying reduction or discussion. M. Le Verrier proposes

in consequence to supply these deficiencies. The present volume contains the reduction of the meridional observations extending from 1800 to 1829, which were executed under the directorship of M. Bouvard. The introduction contains an explanation, by M. Le Verrier, of the observations and instruments, and of the mode of reduction employed. It also includes an elaborate investigation of the errors of the division of Fortin's Circle by M. Villarceau.

Annales de l'Observatoire Impérial de Paris, publiées par U. J. Le Verrier, Directeur de l'Observatoire, tome quatrième. 4to. Paris, 1858.

This volume commences with an investigation of the theory of the earth's motion. The author then proceeds to consider the geocentric motion of the sun and the measurement of time. In the third section he discusses an immense mass of right ascensions of the sun determined at the observatories of Greenwich, Paris, and Königsberg, between 1750 and 1850. The remaining part of the work is devoted to the construction of new solar tables from these materials, some account of which will in all probability appear in the forthcoming Annual Report of the Council.

Berliner Astronomisches Jahrbuch für 1861, *herausgegeben von J. F. Encke, Director der Berliner Sternwarte, unter Mitwirkung des Herrn Prof. Wolfers.* Berlin, 1858.

In this volume the mean and apparent places of the forty-five standard stars which are usually inserted in the *Jahrbuch*, are for the first time derived from Professor Wolfers' *Tabulæ Reductionum*, &c. At pages 310–11 we find an interesting comparison between the apparent places of the stars thus obtained and the corresponding places computed from Bessel's elements. The Appendix contains a paper by Professor Encke on the existence of a resisting medium, an account of which was given in the *Monthly Notices* for December. We find also in this part of the work the apparent places of the forty-five standard stars for the year 1860, computed from the *Tabulæ Reductionum*, &c., the first of the series of years to which those tables are applicable. The volume concludes with an ephemeris for 1859 of each of the minor planets which come to opposition in this year, and a statement of the elements of all the minor planets.

Ueber die Verbesserung der Planeten-Elemente aus beobachteten Oppositionen, augewandt auf eine neue Bestimmung der Pallas-Bahn. Von Dr. J. G. Galle. Breslau, 1858.

This paper commences with a brief account of the researches for determining the orbit of *Pallas* since the epoch of its original discovery in 1802. In 1834 the discordance between the observed and calculated places of the planet amounted to as much as 5′ of space, but in 1836 it was found to have diminished. From the nature of the irregularities it was plain that they could not be attributable exclusively to errors in the elements of the orbit, but that their origin must be rather sought for in the effects of planetary perturbation. In correcting the elements of the orbit, the disturbing action of *Jupiter* alone had been taken into account, but the discordances were of too great a magnitude to be explicable by the perturbations of the other planets. It appeared much more probable that they were mainly due to Laplace's value of *Jupiter's* mass, which had been hitherto used in the calculations. The author accordingly corrected the elements anew, by employing in his computation Airy's determination of *Jupiter's* mass $\left(\frac{1}{1048{\cdot}69}\right)$. The groundwork of his investigation consisted of six oppositions of the planet, extending from 1816 to 1836.

Although the error in the computed place of the planet has seldom amounted to as much as 1′ during the 20 years which have elapsed since the period of Dr. Galle's researches just referred to, still it appeared to him desirable to undertake a fresh investigation of the orbit, in order to ascertain to what extent the residual errors in the place of the planet might be diminished by slight changes of the elements, and by substituting for Airy's value of *Jupiter's* mass the slightly different value deduced by Bessel $\left(\frac{1}{1047{\cdot}879}\right)$. This investigation forms the object of the present paper. It is based upon 12 normal places corresponding to an equal number of oppositions of the planet included between 1816 and 1845. Formulæ specially applicable to such observations are given, and the equations of condition are solved by the method of least squares. The agreement between the normal places of the planet and the places calculated from the corrected elements is very satisfactory.

The author has instituted a comparison between his own elements and those assigned by Mr. Farley in the *Nautical Almanac* for 1860, which have been also found to represent the observations of the planet very closely.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *February* 11, 1859. No. 4.

Rev. R. MAIN, M.A., in the Chair.

Francis Thos. Selby, Esq., Spalding;
Hamilton Field, Esq., Balham Hill;
Frank Robertson, Esq., Royal Crescent, Bath;
J. F. Cole, Esq., 1 Ashton Villas, Dalston; and
J. Collingwood Haile, Esq., Local Government Act Office,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Thirty-ninth Annual General Meeting of the Society.

The Council, in presenting the present Report, are again able to congratulate their constituents on the prospects of the Society, and of the science which it is intended to promote.

The Report of the Auditors, subjoined, will show the state of the finances:—

RECEIPTS.

	£	s.	d.
Balance of last year's account	290	18	6
By dividend on £1650 Consols	24	0	7
By ditto on £3500 new 3 per Cents	50	19	5
By ditto on £1700 Consols	24	19	5
By ditto on £109 8*s.* 5*d.*	1	12	9
By ditto on £3500 new 3 per Cents	51	8	2
Balance of Lee Fund	18	0	3
On account of arrears of contributions	100	10	0
156 contributions (1858–59)	327	12	0
4 compositions	84	0	0
16 admission-fees	33	12	0
13 [illegible]s contributions	23	2	0
Sale of Publications	94	18	6
	£1125	13	7

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Mr. Grant, 1 year as Editor of the Society's Publications	60	0	0			
Mr. Williams, 1 year as Assistant-Secretary	100	0	0			
Ditto commission on collecting £569 15*s.* 6*d.*	28	10	0			
				188	10	0
Investments:—						
Investing £50 Consols				48	12	6
Taxes:—						
Land Tax, 1 year	5	7	6			
Income Tax, ditto	1	9	2			
				6	16	8
Bills:—						
G. Barclay, printer	330	12	11			
J. Basire, engraver	7	16	10			
Mr. Russell, engraver	22	8	8			
J. Rumfit, bookbinder	13	17	10			
Mr. De La Rue, maps	12	12	0			
Mr. Paunceforth, marking instruments	9	17	9			
Mr. Julyan, boxes for instruments	8	10	6			
Sundry payments out of Turnor Fund	14	13	7			
				420	10	1
Miscellaneous items:—						
Charges on books, and carriage of parcels	13	15	7			
Postage of letters and *Monthly Notices*	33	10	5			
Porter's and charwoman's work	26	8	2			
Tea, sugar, biscuits, &c. for evening meetings	13	13	0			
Coals, &c.	12	0	0			
Waiters attending meetings	3	17	0			
Sundry disbursements by the Treasurer	33	5	8			
Mrs. Jones, Lee Fund	8	3	9			
Banker's deductions on country cheques	0	2	0			
Error in casting	0	1	0			
				144	16	7
Balance at Banker's				316	7	9
				£1125	13	7

Assets and present property of the Society, Feb. 8, 1859:—

	£	s.	d.
Balance at Banker's	316	7	9
7 contributions of 2 years' standing	29	8	0
20 ——— of 1 ditto	42	0	0
Various sums on account of arrears	37	15	0
	109	3	0
Due for publications of the Society	4	4	6

£1700 3 per Cent Consols.
£3500 new 3 per Cents.
£109 8*s.* 1*d.* (Lee Fund).
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.

Balance of Turnor Fund (included in Treasurer's account)	25	7	4

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	29	VIII.	189	XIX.	228
I. Part 2	61	IX.	195	XX.	227
II. Part 1	99	X.	207	XXI. Part 1 (separate).	317
II. Part 2	54	XI.	220	XXI. Part 2 (separate).	100
III. Part 1	114	XII.	225	XXI. (together).	130
III. Part 2	133	XIII.	240		
IV. Part 1	134	XIV.	428	XXII.	225
IV. Part 2	146	XV.	214	XXIII.	226
V.	161	XVI.	236	XXIV.	242
VI.	180	XVII.	215	XXV.	252
VII.	203	XVIII.	217	XXVI.	277

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1858 ...	155	184	40	5	384	52	436
Since elected	2	13	...	...	15	...	15
Deceased	−5	−6	−1	...	−12	...	−12
Resigned	...	−4	...	...	−4	...	−4
Removal	1	−1	...	5	...	...	...
February 1859 ...	153	186	39	5	383	52	435

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,

The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock,
The *Wollaston* telescope,
The *Lee* circle,

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.
22. Prismatic compass.
23. Mountain barometer.
24. Prismatic compass.
25. 5-inch compass.
26. Dipping needle.
27. Intensity needle.
28. Ditto ditto.
29. Box of magnetic apparatus.
30. Hassler's reflecting circle, with artificial horizon roof.
31. Box sextant and 2¼-inch glass plane artificial horizon.
32. Plane speculum artificial horizon and stand.
33. 2½-inch circular level horizon, by Dollond.
34. Artificial horizon roof and trough.
35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.
36. A pentagraph.
37. A noddy.
38. A small Galilean telescope, with the object lens of rock-crystal.
39. Six levels, various.
40. 18-inch celestial globe.
41. Varley stand for telescope.
42. Thermometer.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Beaufoy* clock, to the Rev. J. B. Reade.
The *Wollaston* telescope, to Mr. Rees.
The *Lee* circle, to Mr. Burr.
The *Herschelian* 7-foot reflector, to Messrs. Horne and Thornthwaite.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. De La Rue.
Ditto	ditto	No. 3, to Mr. Carrington.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 8, to Prof. Wheatstone.
Ditto	ditto	No. 10, to Sir J. Herschel.
Ditto	ditto	No. 19, to Mr. Dayman.

The other *Beaufoy* clock, the two invariable pendulums, and the brass quadrant (said to have been *Lacaille's*), reported for some years past as being in the possession of the Royal Society, but which could not be found by that Society, are still under inquiry, nothing definite having as yet been ascertained respecting them.

The Council have awarded the Medal to our Secretary, Mr. Carrington, for his *Catalogue of Stars*, from observations made at Redhill. Mr. Main will state the grounds of this award at a later period of the Meeting in the usual way. The Council think it right to add that the award of the Medal in two successive years to gentlemen who have been or are zealous and influential officers of the Society, has not been made without full knowledge of what may be said by those who have not examined for themselves the whole series of awards made since the commencement of the Society. But they further declare their conviction that the work now rewarded has a merit which it would be moral cowardice to pass over, for no other reason than that its author is employing time and talents in the service of the Society. And they rest quite assured that this will be the opinion of those who use Mr. Carrington's Catalogue at home and abroad.

At a special General Meeting, held on the 11th of last June, some alterations in the bye-laws were enacted, with which it is of importance that every Fellow should make himself well acquainted. The practice of submitting a list of Officers and Council for election, hitherto followed by the Council for convenience, is now enjoined by a bye-law. But, in addition to this list, any names forwarded by any two or more Fellows before the ordinary meeting of Council in December, are also to be submitted to the General Meeting in February; the common right of striking out any of these names and substituting others remaining unaltered. These lists are to be circulated as soon after the Meeting of Council in December as can be conveniently done. The effect of the change is that a much longer time is given for deliberation and concerted action; and any two Fellows now possess the power of making known their joint opinions as to the persons most fit to be the Officers and Council of the Society, without trouble or expense to themselves. This statement will account for the change of time and mode in the circulation of the balloting lists recently transmitted to all the Fellows. The details of this change will be found in the new edition of the Bye-laws recently published, together with some minor alterations which need not be noticed here.

In the Report of the Council to the last Annual Meeting it was remarked that the number and variety of the instruments presented to the Society by Miss Sheepshanks might be taken as evidence of the great attention bestowed by our late lamented Fellow upon the art of instrument-making generally. Some further particulars on this subject will not be uninteresting to members, many of whom were probably not so much acquainted with the nature of Mr. Sheepshanks' pursuits as to be able to appreciate fully the extent to which we are indebted to him for

improvements in the machinery so successfully employed of late in the various operations of practical astronomy.

We believe that Mr. Sheepshanks never undertook an uninterrupted series of observations of any particular class, his attention being directed principally to the examination of its means already at the command of the practical astronomer, and its improvement in future. Perhaps there is scarcely one of the many instruments that he possessed which did not undergo in his hands a systematic trial of its powers by application to some practical process which would subject those powers to a fair test; and in the course of operations of this description the defects and advantages of the several varieties of form were in most cases distinctly elucidated, and an improved construction was in many instances the result. Some contrivances, now considered almost indispensable to the astronomer, were, we believe, entirely his own; for example, the slipping-piece, so generally adopted as part of the apparatus applied to an equatoreal telescope employed in the measurement of small celestial arcs. And it was in the course of a long succession of experiments, undertaken with a view to the general object abovementioned, that he became the possessor of the many instruments which are now in the hands of the Society, and these derive therefore additional interest from the circumstances connected with them; from the just appreciation of their powers, which has been made in most instances, and from the improvements to which, in many, their examination led the way.

It must not, however, be inferred from the above remarks that any portion of the apparatus is in the present state of science unserviceable. Improvements there certainly have been; but as in some cases, in which it was possible, such improvements were introduced into these instruments themselves, and as they continued to be employed with success in many of the ordinary operations of practical astronomy by Mr. Sheepshanks himself, as well as by others, at whose disposal they were on various occasions placed, there is every reason to believe that in the hands of the Fellows of this Society they will yet do good service, and assist materially in the prosecution of the cause the advancement of which we all so earnestly desire.

The name of Richard Sheepshanks, so well known and so much respected in this Society, has been permanently connected with the University of Cambridge, and with his own college, by a splendid donation from his nearest representative. Ten thousand pounds Consols have been invested as the principal of a fund bearing the name of Sheepshanks, one-sixth part of which is to maintain an astronomical exhibition at Trinity College, while the remainder is to be devoted to the encouragement of astronomy in the University, under conditions which have been made public. The Council feel great pleasure in recording this noble tribute to the memory of an astronomer to whom this Society owes so much, and may reasonably hope that readers

of this Report, when the present generation has long passed away, will be reminded by these few sentences of useful labours and excellent results, for which the Sheepshanks fund shall have furnished the means. Nor can they omit to couple with this notice an allusion to another princely donation, of quite a different kind, made to the country not long ago by another member of our astronomer's family,—a family of many tastes, but of one spirit.

The twenty-seventh volume of the *Memoirs* is now all but ready for publication. It contains two papers by Mr. Cayley, which cannot fail to attract the attention of those who take an interest in the progress of physical astronomy. In the first of these papers the author has rigorously investigated the problem of disturbed motion, with especial reference to the circumstances depending on the variable plane of the disturbed orbit, and has deduced formulæ for the variation of the elliptic elements founded on his peculiar mode of treating the subject. In his second paper Mr. Cayley has exhibited an important application of his theory, by supplying a step which was wanting to Professor Hansen's Lunar Theory, in order that its results might be rendered readily comparable with those of other geometers.

In a short paper the Astronomer Royal has fully substantiated certain statements which he had made on a former occasion relative to Oltmann's calculations of ancient eclipses.

It is well known that the solar eclipse of March 15 was, in general, very imperfectly observed throughout England in consequence of the unfavourable state of the weather. At Cambridge, where the observers were rather more fortunate in this respect, some micrometrical measures were taken, which Professor Challis has employed in investigating the corrections applicable to the elements upon which the computations of the various circumstances relative to the eclipse depend. It appears from his researches that the line of central phase, as computed from Hansen's Lunar Tables, was situated a little to the north of the actual line, while on the other hand Burckhardt's Tables threw the line a little to the south of the position indicated by observation.

The series of communications in the volume concludes with a paper by Captain Clarke, "On the Reduction of Occultations of Stars by the Moon." By an elegant and simple process, the author has investigated the coefficients in the equations of condition for obtaining the small corrections applicable to the various elements upon which the time of an occultation depends. Observers of occultations will be enabled, by means of the results contained in this paper, to give a practical form to their labours with as little trouble of computation as the complexity of the subject can admit of.

The *Monthly Notices* continue to offer an easily accessible

channel of publication to observers and computers of all classes connected with the Society, while the importance of directing the attention of the Fellows from time to time to the labours of Astronomers in other countries has not been lost sight of. Since the last Anniversary it has been considered expedient to extend the distribution of this portion of the Society's publications to various individuals and Institutions, both at home and abroad, in addition to those whose names have hitherto appeared on the list for this purpose. At the same time, the successive numbers are now, in all cases, forwarded to their respective destinations as soon as they are received from the printer. The adoption of this practice will, no doubt, be attended with a small additional outlay on the score of postage, but the advantages derivable from an early circulation of the contents of each impression are obviously so great as amply to outweigh any considerations of this kind.

A plan has been carried into effect by which the *Monthly Notices* will become an integral part of the volumes of the *Memoirs*. All are aware that, for some years past, an octavo volume of *Monthly Notices* has always been given with each volume of *Memoirs* sold. It has been found that, by re-imposing the type of the *Monthly Notices* into a quarto form, with double columns, it is practicable to form an edition of the *Notices* which may be stitched up with the *Memoirs*, so as actually to form part of the volume. The expense of printing the annual report of each year twice will thus be avoided. It has sometimes been suggested that it was unnecessary to make the annual report a part of the volume of *Memoirs*, but those who have been students of old history have always protested against the omission. They have represented that it is a very serious defect of the older Transactions that they supply no materials for the histories of their several societies; from which it not unfrequently arises that the papers themselves are unaccompanied by information necessary to their being properly understood as historical monuments. Both ends are now made to meet; the annual report, and much current information besides, form a part of the very volume which contains the larger Memoirs; and the annual report is not printed twice. This saving, and other circumstances, will, it is hoped, enable the Council to carry into effect what every one wishes, namely, the gratuitous presentation of the *Memoirs* to all Fellows who make application for them.

Two fresh channels for the publication of astronomical intelligence and research have been opened during the year,—the additions to *M. Le Verrier's* daily *Bulletins* of the weather, and Dr. Brunnow's *Astronomical Notices*. In the first of these series, in which for the first time something like a general view of the daily meteorology of Europe is presented by the agency of the electric telegraph, and a first step taken towards out-

stripping the storm, by turning to the service of man one of its own elements; M. Le Verrier has taken advantage of its rapid dissemination by improved postal arrangements, to convey in all directions notices of discoveries, short ephemerides, and extracts from the valuable communications of his correspondents, many of which we may hope to see reprinted hereafter in a more permanent form. In the second, very recently started, Dr. Brunnow proposes to publish separately the researches and certain of the observations made at the Observatory of Ann Arbor in Michigan, now under his able direction. There may be need of both these channels of publication, and at present these may be early days for pronouncing an opinion on their ultimate advantage to astronomy; yet it may be remarked that the main feature of utility of such publications as the *Astronomische Nachrichten*, the *Astronomical Journal*, and our own *Monthly Notices*, is, that in their pages are *collected* together the shorter communications of observers, where, within moderate compass may now and hereafter be found the current astronomical literature of the time. If the practice of publishing in this manner should go beyond the cases in which distinct reason exists for anticipating the appearance of the *Nachrichten* and similar works, the secession, to give what would then be the appropriate name, would be rather an impediment than an assistance to those for whose use the new works are intended. Every additional subscription, moreover, is an increase of difficulty to the man of small means.

At Cambridge, the Lowndean Professorship, vacant by the lamented death of Dr. Peacock, has been conferred on Mr. Adams. By this very gratifying appointment, Mr. Adams is recalled to England from St. Andrews, in which university he has for a short time past held a professorship. The Lowndean Professorship was instituted about 110 years ago, as a combined professorship of astronomy and geometry—a term then synonymous with mathematics. The first three occupants were neither of them distinguished for the combination of the two kinds of knowledge; the fourth, Dr. Peacock, powerful in both, did not make their combination a pursuit. Mr. Adams, whose name is so remarkably connected with the application of mathematics to astronomy, has a claim to a chair thus named which no one can dispute.

The Council have to announce the loss by death* of the Dean of Ely; A. Baily, Esq.; Captain Bate; Edw. Bury, Esq., Lieut. Burdon; Rev. H. H. Jones; P. Kernan, Esq.; P. Legh, Esq.; Admiral Owen; H. L. Pattinson, Esq.; Lieut. Raper, R.N.; J. Reeves, Esq.; R. Taylor, Esq.

* Since this was read, the Council have received with deep regret the news of the death of our eminent transatlantic associate, W. Cranch Bond, on the 29th January.

ARTHUR BAILY was born about 1787, and died July 8, 1858. He was the younger brother of Francis Baily, whose name can never be mentioned in this Society without a word in remembrance of services which can never be too fully acknowledged. He was associated with his brother on the Stock Exchange, and continued in business long after his brother retired. Many years ago he was a frequent attendant at our Meetings.

Captain WILLIAM THORNTON BATE, R.N., was born in 1820; he became a Student at the Royal Naval College at Portsmouth in 1833, and remained there two years.

At school he had always shown a disposition for fun and hardihood, and amongst his companions at the Royal Naval College he was usually the leader in their various games and diversions; yet he was not the less distinguished in his studies, being endowed with great natural talents.

On leaving the Royal Naval College Mr. Bate was appointed midshipman to the Isis, the flagship of Rear-Admiral Warren, on the Cape of Good Hope and Coast of Africa station. He remained on this station for five years, where, in spite of every obstacle offered by a burning sun and unhealthy climate, he acquired those habits of activity and industry in the pursuit of his profession which enabled him to effect so much that was useful, and to gain such distinction in after-life. Mr. Bate was frequently selected for difficult service on boating expeditions. On one occasion, when his ship was sailing at a rapid rate in the Bight of Benin, a locality much infested by sharks, Bate jumped overboard without a moment's hesitation, and saved the life of a sailor who had fallen overboard.

In 1841, soon after war had been declared with China, Mr. Bate was appointed mate of H.M. Ship Blenheim; when he was actively employed in all the operations against Canton.

A detailed and well-written memoir of Captain Bate, by the Rev. John Baillie, has lately been published by Messrs. Longman, which contains so many characteristic traits of his disposition, and such a graphic account of his adventurous life, that we proceed to give some extracts from it.

"The Blenheim had been lying some weeks off Canton, when one morning all hands were ordered to 'prepare to assault the town.' Situated on a plain which is swept on two sides by the river, and having in the rear a considerable mountain called the White Cloud Mountain, the city was commanded by certain forts occupying some three or four slightly elevated hills immediately behind the town. The forts were occupied by Tartar troops, whilst the city itself, with its suburbs, containing a population of a million souls, was protected by a wall twenty-five feet thick at the base. Our own troops were a mere handful; but with the courage natural to Englishmen, the command was given to take the forts. The blue-jackets in-

stantly landed; and, almost in the twinkling of an eye, they were scaling the heights. Bate was among the first to mount the breach; and, just as he had reached the summit, he was struck below the chin by a ball. Instantly his whole chest was covered with blood, but the wound did not prove serious. The gallant fellow pushed on, pistol in hand, and the next moment his pistol was struck by another ball which cut it in two." (Memoir, p. 24.)

"The crisis was past. The 'braves' fled in precipitation, and the British force was in possession of the fort, since known as the 'Blue-jackets' Height.'

"Before the affair of Canton he had volunteered to Captain Collinson to be his assistant for the survey of the Chinese waters. The latter, having now obtained command of the Plover, at once secured Bate's services. 'His activity and energy,' writes Captain Collinson, 'were so conspicuous, that it was only by dint of great importunity that Sir Thomas Herbert, who had succeeded Sir F. Senhouse in the command of the Blenheim, consented to lose his services, permitting him to join my ship.'" (Memoir, p. 25.)

And thus he entered upon that special line of service in which he was to spend his remaining years with a distinction which placed him confessedly in the very front rank of his profession.

Mr. Bate was promoted to the rank of lieutenant in October 1841, for his gallantry in mounting the heights of Canton.

The war, however, was not yet ended; and wherever there was a post of danger there the young lieutenant was sure to be found.

"His ship had been despatched to the northern part of the Chusan Archipelago, to examine it preparatory to the movement of the fleet. On their arrival a party landed by two boats in a shallow creek, and had proceeded, with all but the boat-keepers, across a low level plain towards a small isolated hill, when leaving the others at the foot, Bate and Collinson went to the summit to look around. 'Suddenly,' writes the latter, 'I perceived him to run forward, at the same time drawing his sword; and I soon found he was chasing a Chinaman, who with sword and shield had been ensconced on the summit watching our proceedings. A horrid shout, however, distracted our attention from the individual; and on looking on the plain below we found the Chinese army drawn up in array to receive us. Nothing remained but a sharp retreat to the boats, from which we should have been cut off had it not been for the determined face which Bate, in command of the rear, maintained; keeping them in check by a cool, well-directed fire.'

"The next morning, twenty-five in number, they went on shore; and, in the course of forty minutes, without a single casualty, dispersed the Chinese forces, killing their leader and twenty others, capturing their military chest, and setting fire

to their junks. 'This success,' Captain Collinson adds, 'was mainly owing to the prompt manner in which Hall and Bate led their men along the plain.'

"Some weeks elapsed and another illustrative incident presented itself. It was at the assault at Chapoo. 'To the Plover,' writes Captain Collinson, 'was assigned the office of covering the landing of the troops; and, on Sir H. Gough leaving the beach, he accepted my tender of Lieutenant Bate's services as his aide-de-camp to keep up communication with the rear. Among the outward defences of the city were several horseshoe-shaped enclosures, whence the Chinese maintained a harassing fire. Supported by two men Bate made a rush at one of the enclosures, and was at once involved in a hand-to-hand conflict with the officer commanding it—a blue-button mandarin. In the course of the struggle both parties fell to the ground, but Bate by his superior agility remained uppermost, and succeeded in disarming his antagonist and in making him prisoner.

"A few minutes later a fresh achievement meets us. The troops were now at the gates of the city, but without any battering-train or field-piece to force them open. The commander, apprehensive that the Chinese might rally before any men could be brought to the front, was looking round on every side with anxiety, not knowing what to do, when, suddenly, Bate was seen sword in hand scaling the wall alone. The next moment he was on the summit; the Chinese, supposing him to be the leader of a party, precipitately abandoned the post; and the brave fellow, coolly descending on the other side, opened the gates to the troops. It seemed as if he bore a charmed life." (Memoir, p. 33.)

In the spring of 1846 Bate returned as the Plover's senior lieutenant to England.

Lieutenant Bate was not immediately employed in active service, he profited by this leisure to pursue professional studies, first at the Steam Factory at Woolwich and afterwards at the Royal Naval College.

He was promoted to the rank of Commander in Feb. 1848, and soon afterwards appointed to command the Royalist, and resume the survey in the Chinese waters.

Bate was most usefully engaged during the ensuing five years in surveying operations; his industry knew no bounds: as a specimen of his labour and skill it may be mentioned that he executed an elaborate hydrographic survey of the island of Palawan, three hundred miles long, with its harbours and adjacent waters, fixing all the mountains and prominent hills visible from the sea.

Commander Bate naturally anticipated being promoted on his return to England after all these services, but in this he was disappointed, and the Admiralty sent him once more to the China station, in command of a small brig, the Bittern; and it was not until August 1856 that he obtained the command of the

Actæon, a much larger and finer ship, in which he continued his surveys, and was also engaged in active warlike operations.

In consequence of Yeh, the Chinese Imperial Commissioner, having refused full satisfaction for an insult offered to the British flag, it was resolved upon by our commander to seize the defences of the city of Canton.

In these operations Bate bore a very conspicuous part; the Admiral, Sir Michael Seymour, writes with respect to the storming of the defences of Canton, "The way was most gallantly shown by Commander Bate, whom I observed alone waving an ensign on the top of the breach."

Just at this period the intelligence of Bate's being promoted to the rank of Captain reached him. Captain Bate distinguished himself in command of the Macao-fort, which he held with three hundred men against all the attacks of the Chinese after the ships had retired lower down the Canton river.

On the 28th December, 1857, Captain Bate wrote thus on board the Actæon: "'We opened our fire at daybreak. I am just going off with the Admiral to the landing-place, which is about two miles to the eastward of the city. We stop out all night and advance early in the morning.'

"At daylight the following morning the 'rocket-practice' gave place to a steady fire from a mortar-battery; and Bate's little party, now joined by the general and his staff, proceeded in the direction of the city wall.

"In a hasty reconnoitre they found, some dozen yards in advance, a ditch or gorge forty or fifty yards broad. In the intervening space was a low earthen fence, surmounted by bunches of high reeds, which interrupted the view of the spot where the scaling-ladders were to be placed to mount the broken embrasure. All around hurtled a storm of balls and rockets from the wall; and no one could cross to the edge of the ditch without imminent danger. Yet some one must run the gauntlet, if the ladders were to be set for the escalade.

"Captain Bate at once volunteered to go—as he rushed across the open patch, to look into the ditch, all eyes followed him, and more than one heart throbbed.

"'Our Captain,' says his coxswain, who was at his side, 'was in the act of taking the distance from the ground to the top of the wall with his sextant, when a shot from a gingall struck him in the right breast. He fell straight on the ground and never moved afterwards.'" (Memoir, p. 240.)

In half-an-hour he had ceased to breathe. And thus died one of the most promising officers in the Royal Navy, esteemed and regretted by all. His piety was most fervent and unpretending, his courage and professional skill beyond all praise. Captain W. T. Bate was only in his seven-and-thirtieth year when he was killed before the walls of Canton.

Capt. W. T. Bate was elected F.R.A.S., 9th March, 1849.

Commander WILLIAM BURDON, R.N., entered the naval service in 1841, he was made Lieutenant in 1848 and promoted to the rank of Commander in Dec. 1852, on the East India Station. Elected F.R.A.S. in May 1852.

He returned to England in a weakly state, and died at Sydenham in November 1857, aged thirty-two years.

The Rev. H. H. JONES was born at Brownsover, near Rugby, on the 6th of June, 1787. After completing the usual course of studies at Rugby he entered himself at Bristol College, and studied for the Baptist ministry under Dr. Ryland. On leaving the college he officiated for some time in the congregation where the Rev. John Foster, the celebrated essayist, was minister; and subsequently he became the co-pastor with the Rev. W. Morgan, at the Baptist Chapel, Birmingham. In 1821 he removed to Tamworth, where he continued in charge of a congregation for about two years. In 1823 the late Mr. Samuel Greg having built, at his own cost, a chapel for the work-people employed at his manufactories at Styal, in Cheshire, allowed them the privilege of selecting their own minister. Several persons of different denominations were accordingly invited and officiated in succession; but the choice of the congregation fell upon Mr. Jones, and he remained as their pastor for upwards of ten years. During 1833 he was elected Registrar and Chaplain of the Rusholme Road Cemetery, Manchester, which office he retained up to the time of his unexpected death, on the 21st December last.

Mr. Jones became a Member of the Manchester Literary and Philosophical Society in 1846, and officiated as one of its Honorary Secretaries for several years. He became a Fellow of the Astronomical Society in 1848. For many years he prepared the astronomical portions of the Manchester Almanacs, and was regarded both as a careful observer and a delicate manipulator. For the last few years he held the appointment of Astronomer to the Manchester Corporation, who provided him with a good transit instrument, &c., fitted up in a small observatory adjoining his house. He was thus enabled to regulate the public clocks in Manchester, and succeeded in keeping that in the vestibule of the Town Hall correct to a degree unknown before. The pendulum rod of his sidereal clock was of his own contrivance, and withstood the variations of heat and cold very satisfactorily. The pendulum itself was regulated by means of a small cup attached to the rod, into which were placed a few small shot, which were increased or decreased in number, according as the clock went too slowly or too fast. This could be done without stopping the clock. He also introduced some improvements into the apparatus for illustrating M. Foucault's experiments for proving the rotation of the earth, at the time when these were repeated by him before his friends in Manchester.

The literary and scientific papers by Mr. Jones are not numerous; but he read many papers before the Manchester Society which have never been published. In 1837 he published an essay on the *Philosophy of Education*, which was republished with additions in 1857. The lapse of twenty years between the first and second issues had not impaired its value, since it contains an excellent *resumé* of the leading principles of education. The tenth volume of the new series of the *Manchester Memoirs* also contains a Biographical Notice of Peter Clare, Esq. F.R.A.S., from his pen; and in the fourteenth volume he offers a few Remarks on the Occultation of *Jupiter* and his Satellites by the Moon, January 2, 1857. Among his unpublished papers may be noticed those On the Determination of the Distance of the Fixed Stars from the Sun, and a most elaborate one On the truth of the Nebular Hypothesis.

Peter Legh, of Norbury Booths Hall, near Knutsford, Cheshire, was descended from an ancient family in the county of Cheshire, where he was possessed of a fine ancestral estate.

He was the author of a work entitled the *Music of the Eye, or Essays on the Vitruvian Analysis of Architecture*, 1831, 8vo.; and the profits of the work, if any, were to go towards a fund for building a church at or near Torkington, in the parish of Stockport, Cheshire.

A few years before his death Mr. Legh commenced the building of a church at a very considerable expense, in a village near his mansion, which was completed in or about 1857. He published an Ombrological Almanac, of which the seventeenth number appeared in 1856.

He was an excellent scholar, an accomplished man, and an amiable, benevolent, and liberal friend. He was fond of painting, and the possessor of some fine works of the ancient masters and an extensive library. He was also a skilful mechanic and fond of turning.

He is perhaps best known by his Ombrological, or as others would call it meteorological, Almanac. He had a strong opinion of the possibility of arriving at a knowledge of the laws of the weather; and he claimed to have had some success in his own investigations. We fear that no palm can be awarded to him in this most tangled of all subjects; but he has the praise of great diligence, as well as of good temper, in maintaining that most difficult position of a speculator who stands alone in his conclusions.

Vice-Admiral William Fitzwilliam Owen, born in 1773, at Manchester, was the son of Captain William Owen, who, after losing an arm in the service of his country, was accidentally killed at Madras in 1778. Fitzwilliam was the younger brother of the late distinguished Admiral Sir Edward Owen, G.C.B., and was educated with him at the celebrated

Hanway School at Chelsea. After attaining the first rank in that seminary, he entered the Royal Navy in the summer of 1788, on board the Culloden, of 74 guns, commanded by his relation, Sir Thomas Rich. By this officer he was from time to time placed in several ships for the purpose of acquiring knowledge in his profession; but he rejoined the Culloden in time to be present at the great battle fought on the 1st of June, 1794.

Shortly after that glorious conflict, Mr. Owen sailed in the Ruby, 64, for the Cape of Good Hope, where he was present at the capture of a Dutch squadron of three sail of the line and six frigates and sloops, in Saldanha Bay, in August 1796. Returning to England after this exploit, he joined the London, 98, bearing the flag of Admiral Colpoys, with whom he quitted that ship during the alarming mutiny at Spithead, in May 1797; for his firm conduct on that trying occasion he was promoted in the following month to the rank of Lieutenant, and at the same time placed in command of the Flamer gun-brig. In this and other ships and vessels he experienced much active Channel service, till the close of the first French revolutionary war.

At the recommencement of hostilities, Owen was among the foremost to tender his services, in consequence of which, in July 1803, he was appointed to command the Sea-flower, a brig of 14 guns, in which he shortly afterwards sailed for the East Indies; on which station he was employed on various missions by the Commander-in-Chief. In 1806 he captured Le Charle, a mischievous French ketch, and explored several of the channels between the Eastern Islands, to the great improvement of the charts. Towards the close of that year he piloted Sir Edward Pellew's squadron through an intricate navigation into Batavia Roads. Here his bravery and skill were conspicuous in the command of a division of armed boats at a successful attack on a Dutch frigate, seven man-of-war brigs, and about twenty armed vessels, for which he obtained a very honourable mention in the Gazette. In the following year he contributed to the capture and destruction of the dockyard and stores of Griessik, in Java, together with all the men-of-war remaining to Holland in India, consisting of the Revolution, Pluto, and Kortenaar, of 70 guns each, with the Rustoff frigate and a flotilla of gun-boats.

In 1808, Lieut. Owen had the misfortune of being forced to surrender the useful little Sea-flower to a couple of French frigates, in the Bay of Bengal. The brig was soon retaken and commissioned by Lieut. George Steward; but Owen was carried prisoner to the Isle of France, where he was detained till June 1810, when he was exchanged. When preparing to depart, he playfully told the French Governor that perhaps they might soon meet again; at which General De Caen laughed — albeit not often in that mood — and hoped that he

would be brought to Port Louis in a vessel more worthy of capture than his last was. The *badinage* was rather predictive of coming events.

Meantime, Owen had been made a Commander, by a commission dated in May 1809; and, on his return to India, was occupied in assisting the authorities at Madras with his opinions, and in superintending the transports for the expedition against the Isle of France.

Our officer's next appointment was to the Barracouta, an 18-gun sloop-of-war, which he joined in time for assisting at the blockade of Batavia, previously to the invasion of Java. On the arrival of the forces under General Sir Samuel Auchmuty and Commodore Broughton, he assisted in the debarkation of the troops at Chillingching; and continued attached to the army until after the surrender of Batavia, in August 1811. He had been advanced to post rank in May of the same year, and after acting in command of the Piedmontaise a short time, he was appointed Captain of the Cornelia, of 32 guns. In this frigate he sailed from Batavia Roads in March 1812, with a small squadron, consisting of the Phœnix, Bucephalus, and other vessels under his orders, with detachments of the 59th and 89th regiments on board, to take possession of the commercial depôt at the mouth of the Palembang river, at the eastern end of Sumatra. Having achieved this object, he returned to England in charge of a convoy from China, in June 1813.

Having paid unceasing attention to the Hydrography of the East during his cruises, Captain Owen now rendered very material assistance to his friend, the late Captain Horsburgh, in the compilation of his well-known *Oriental Navigator;* and he moreover employed his half-pay leisure in correcting charts, and in making a translation of Franzini's *Sailing Directions* from the Portuguese. At length, in March 1815, he was appointed to the Surveying Service on the Lakes of Canada, where he opened the line of operations which has since been so ably completed by his *élève*, the present Rear-Admiral Bayfield. In August 1821 he was appointed to the Leven, 24, in which corvette, with the Barracouta, he was for upwards of four years employed in an examination of the west and east coasts of Africa,—an arduous duty carried on in the face of malignant fevers and deadly casualties. In the Ashantee war he was able to render an effective co-operation to General Turner, as was acknowledged by the latter in the London Gazette.

On his return from this mission, Captain Owen's representation was so strong that the island of Fernando Po, in the Bight of Benin, would not only prove more healthy than Sierra Leone, but would also afford greater facilities for the suppression of the slave-trade, that he was commissioned, in February 1827, to the Eden, of 26 guns, for the purpose of forming a settlement there, and completing his surveys. On

this he was occupied till the close of 1831, when he retired to half-pay, but not to idleness, for his charts, remark books, and attention to improving the means of maritime surveying, fully employed his time. In this Society he worked on our Council; and he presented us with two specimens of his professional ingenuity in a Double Reflecting Circle and a Quadruple Reflecting Sextant.

The island of Campo Bello, in Passamaquoddy Bay, New Brunswick, which belonged to the Owen family, had descended to the subject of our notice and his brother; and as William evinced a desire to settle there, Sir Edward surrendered his portion to him. Here he had full occupation for a time in getting it into order, and in establishing his family, consisting of a wife and two daughters. Soon after his arrival he was elected member for that locality in the House of Assembly at Fredericton, where he brought various abuses to light, and involved himself in the cares of a staunch reformer. As he was still zealous in the cause of hydrography, Sir Francis Beaufort procured an appointment for him to a fine steam-vessel, the Columbia, of 100-horse power, to survey the Bay of Fundy and and the coast of Nova Scotia. Being superseded on his promotion to flag rank in December 1847, he continued the rest of his life on half-pay.

In conduct and bearing our excellent Admiral was at once firm and kind, shrewdly sensible and unostentatious, with a manner bordering on the eccentric; on service he was authoritative without being at all tyrannical, a man of ready resources and unremitting zeal. In speech he was fluent and blunt. When a ministerial peer once made him a proposal which he considered as not quite proper, he replied, "My Lord, I may be poor, but still I am proud." On the Admiralty forwarding him a complaint which they had received from the Marquis Palmella relative to some differences at Mozambique, he closed his explanation with — he "trusted that the word of a Captain in the British Navy was as good as that of a Portuguese Marquis." He built a church at Campo Bello, which he endowed, and after considerable trouble prevailed on the provincial bishop to appoint a clergyman of the Church of England to it. This gentleman regularly officiated, until one Sunday the Admiral gave him notice that he wished to occupy the pulpit himself that day! So singular and abrupt a hint led to an altercation, and as Owen declared that he would take possession if necessary by force, the clergyman resigned the living, and the Admiral for a time regularly performed the clerical duties — the congregation attending even more regularly than before.

Vice-Admiral Owen, whose faculties had been declining for some time, died at St. John's, New Brunswick, on the 3d of November, 1857, at the advanced age of eighty-four years. He had been very long a Fellow of the Society, which he regularly

attended when in town; and he was for some time on the Council.

HUGH LEE PATTINSON was born and educated at Alston, in Cumberland, where his parents, belonging to the smaller landholders of that locality, had for generations resided.

A lecture on chemistry, which he accidentally heard, awakened his attention to that science, and to it he devoted all his spare time. When still very young he removed to Newcastle, having obtained a situation in an extensive soap-manufactory. At about the age of thirty he returned to Alston, having, on the recommendation of Mr. John Taylor of London, been appointed Assay-Master at the extensive lead-mines of the Commissioners of Greenwich Hospital.

While thus engaged Mr. Pattinson made a discovery which connects his name in a distinguished manner with metallurgic science. Previous to this discovery the silver originally contained in lead ore was obtained by the conversion of the whole of the lead into litharge, leaving the pure silver behind; the litharge had to be reconverted into lead, at considerable expense and with considerable loss of metal. After an immense number of unsuccessful attempts to effect the separation by a less circuitous method, Mr. Pattinson discovered that a mixture containing only a very few ounces of silver to the ton of lead, in cooling appeared to resolve itself into two portions, one remaining fluid after the other had assumed a solid and granular condition. These granular crystals were found to consist of lead nearly free from silver; the silver alloy remaining being fusible at a lower temperature than pure lead. Three or four years passed away before all the difficulties attendant on the application of this discovery were overcome, but all these gave way before his ingenuity and perseverance. Prior to the discovery of the *Pattinson process*, as it is called, the extraction of silver from lead could only be pursued with profit when present in the proportion of 20 ounces to the ton. The minimum was now reduced to 3 ounces, and lead-mines, before neglected, could now be worked with advancement.

Mr. Pattinson was one of the principal founders of the extensive chemical works at Felling and Washington near Newcastle-on-Tyne.

From an early age mineralogy and geology, also electricity and magnetism, received a considerable share of his attention, and he occasionally favoured his neighbours with lectures on scientific subjects. Every one is now familiar with "Armstrong's Hydro-Electro Machine." It was Mr. Pattinson who first brought before the public the phenomenon which, presenting itself at a steam-boiler at Cramlington Colliery, gave birth to that machine.

For many years before his death he derived considerable pleasure from the pursuit of practical astronomy. Mr. Pattinson

was one of the first and warmest admirers of the optical productions of Mr. Cooke of York. From him he procured the largest object-glass that Mr. Cooke had up to that time produced, $7\frac{1}{4}$ inches aperture, which was mounted equatoreally with graduated circles, clock motion, micrometer, &c., and more than realised Mr. Pattinson's most sanguine expectations. The equatoreal was erected in a neat observatory, near his residence at Scots House, which was also furnished with two clocks, chronometer, and a transit instrument formerly belonging to the late Francis Baily. The large equatoreal was lent to Prof. Smyth for his scientific expedition to Teneriffe, in which Mr. Pattinson took a warm interest. More than one 2-foot disk of glass was examined with a view to still larger results, but the optician reported "bad glass."

A slight digression may perhaps be pardoned to couple the names of two of our deceased Fellows in a manner alike creditable to both. The last night the writer spent with him at Scots House, a few months before his death, Mr. Pattinson spoke in glowing terms of approbation of Dr. Peacock's valuable treatise on algebra then on the library table. His leisure hours for some time previously had been spent in reading the more advanced portions of the second volume. Teacher and student died at nearly the same time, their names stood together on our alphabetical list of Fellows, and at the same anniversary their obituaries stand side by side.

In private life Mr. Pattinson was most kind and courteous. He was very liberal in his public subscriptions and private charities. He had a rich fund of anecdote, and a genial, humorous manner in conversation. He died on the 11th of November, in the sixty-second year of his age.

George Peacock was born at Denton, near Darlington, April 9, 1781. His father, Thomas Peacock, was a clergyman and schoolmaster at that place, and was the author of a manual of arithmetic, *The Tutor's Assistant Modernised*, which went through several editions, and also of a work on mensuration, *The Practical Measurer*. Both works were first published about 1810. The son finished his education under the Rev. James Tate, at Richmond, whom he always entertained the highest regard for, and to whom he dedicated the first of his works on algebra in most affectionate terms. He then removed to Trinity College, Cambridge, and took the degree of B.A. in 1813 with high distinction, being second on the list of wranglers, and second only to Sir John Herschel. A fellowship, and ultimately a tutorship, followed of course; and about 1817 he took orders. Of his works we shall presently speak separately. He continued to teach in Trinity College till 1839, when he was made Dean of Ely; and thus removed from a college life when he was close on fifty years of age, he entered on his second career without difficulty from its novelty

or hindrance from his previous habits. Once a dean, says one of the journals, he grasped and carried out all that a dean's life ought to be. The restoration of his cathedral, and the purification of the town, are among the successes which prove at once the goodness of his judgment, and the power which talent, judgment, and character united, gave him over the minds of others. His clerical duties were varied by those of the Lowndean Professorship, to which he was appointed in 1836, by the Standard Scale Commissions (1838, 1843), by the Cambridge Commissions (1850, 1858), by the Prolocutorship of Convocation (1841–47, 1852–57), and by various minor duties. Whenever a man of safe judgment was wanted, who united kindness and courtesy to a clear view of duty and a firm purpose, the government, the clergy, and the university, knew where to find him. In the midst of heavy duties, he died Nov. 8, 1858, of bronchitis. He had for many years suffered under bad health, to the frequent interruption of his scientific undertakings. In 1847 he married the sister of Bishop Selwyn. His own father and mother lived to extreme old age, and saw their son in his highest honours. A full detail of his life is given in the Notices of the Royal Society, drawn up, it is believed, by the only person who would omit to name his superior on the Tripos. That one of these men should pay this last tribute to the other was rendered most fitting as well by their early struggle, as by their subsequent association in forwarding many a useful undertaking.

Dr. Peacock had cultivated the Continental analysis, then very little known in England, at an early period of his studies. Professor Woodhouse, in 1803, had called attention to the foreign mathematics, as they were called, in his *Principles of Analytical Calculation*,—a work which was no doubt intended to recommend the change which afterwards followed, but was written by a man who saw that a severe examination and a discriminating criticism would better advance the object than the eulogium of a partisan. In 1813, there were found in the University a few very young men who had fully mastered the Continental system. Among these were Messrs. Peacock, Babbage, Herschel, and Maule (afterwards Justice). Peacock was distinguished by very extensive reading: his power in this respect was a talent, and a rare one; for when no duty compels, and books are not to be encountered as drudgery, none can bring themselves to face volumes by the hundred except those in whom the memory and the reflective power are so strong that every page is suggestive of comparisons. The young men alluded to formed an Analytical Society, which produced a small volume of Memoirs in 1813; and they proceeded to declare open war upon the studies of the University. A translation of Lacroix's *Differential Calculus* was prepared (1816), and a volume of examples to accompany it (1820); in *the latter* Mr. Peacock had the largest part. In 1817 he was

Moderator, and in this capacity he ventured to introduce the new system into the public examinations, his colleague retaining the old one. This old system made its appearance once more in 1818; in 1819 Mr. Peacock was Moderator, with a colleague of his own sentiments (Mr. Gwatkin), and the change was fully accomplished. All the chief actors in producing it have lived to see their work fully done, and their country in full communication with all the world after more than a century of nearly complete exclusion. Mr. Peacock subsequently published an anonymous Syllabus of Trigonometry and Algebraic Geometry, which was much wanted in the University; indeed the reformers had kept their eye too exclusively upon the differential calculus, of which this publication was a kind of confession.

In 1826 he published in the *Encyclopædia Metropolitana* his historical article on Arithmetic, by far the most learned article of the kind which exists: for though Chasles and others have written able and minute dissertations on individual points, there is nothing like a treatment of the whole subject, except the article of which we now speak, which treats details with accurate minuteness, and gives an amount of information which would have been thought impossible within the space. This finished work was not undertaken until the publication of the *Encyclopædia* suggested it: and it was among the earliest appearances of the work.

In 1830 appeared the first of his two works on Algebra, which are not to be called first and second editions of one work. The difficulties under which algebra had laboured as to the interpretation of some of its most essential symbols had begun to receive something like a solution; but it was rather a clear glimpse of what was to come than a full attainment. There was much need of a philosophical mind which should make the first principles of algebra coextensive with the conclusions founded upon them. Mr. Peacock meditated long upon this subject, and produced the first finished effort towards a *theory of algebra*. He was much assisted by the details which had been published by Argand, Mourey, Buée, and Warren,—writers who were all, more or less, possessed by the idea that they were constructing a new algebra, when, in truth, as was first fully seen by Peacock, they were explaining the old algebra. The next work on Algebra (two volumes, 1842 and 1845) is a systematic separation of universal arithmetic, or the science of numbers under general symbols, from that higher interpretation of symbols which alone is properly called algebra. The first volume is most admirably fitted to be the introduction of young students into the whole subject of symbolic language.

Between these two works Dr. Peacock presented to the British Association, in 1834, a report 'On the Recent Progress of Analysis.' This is a critical investigation of those higher difficulties which meet the student when he carries algebra into the differential calculus. It abounds with cases which are still

matters of opinion; but there are very few mathematicians who venture into full discussion of these difficulties, in the elucidation of which lies all our hope of future progress. Many a young analyst will owe his strength to his acquaintance with this vast thinking ground, in which a mind long accustomed to comparison has brought before him at one view the details which, singly and apart, present only difficulty without suggestion of the direction in which to look for light.

The Life of Dr. Young, with the edition of his miscellaneous works (in which he was assisted as to the hieroglyphical portion by Mr. Leitch), occupied Dr. Peacock for many years. The life itself is a very good specimen of judicious biography and of biographical research; while the knowledge to edit the miscellanies of so varied a writer as Young is possessed by very few. Of Peacock it may truly be said that he placed Young's statue on its pedestal.

His observations on the University Statutes, published in 1841, showed that he had deeply studied the University in which he was brought up, and was as ready to aid in corporate amelioration as in improvement of mathematics.

Dr. Peacock's last writing was a series of short answers to the questions on the Decimal Coinage proposed by Lord Overstone, one of her Majesty's Commissioners on the subject. Dr. Peacock was a strong advocate for this change in the coinage: and he had gone deeply into the question, both as an historian of arithmetic, and as a member of the Standard Scale Commissions.

He was one of the earliest members of our Society, though his residence at Cambridge and Ely prevented him from being a frequent attendant. To this short sketch of his labours the Council may add that any sketch, however short, supersedes the necessity of any description of character. One thing may well be added. A man may have been all that Peacock was, and have done all that Peacock did, without possessing that gentleness of nature, kindliness of feeling, courtesy of manners, and benevolence of action, which endeared him to all who came in contact with him.

Lieutenant Henry Raper, R. N., was the eldest son of the late Admiral Raper, an officer of much distinction in the naval service, and who was signal lieutenant to Lord Howe in the action of the 1st June, 1794.

Henry Raper's education was commenced at the Charter House School. He entered the navy as volunteer on board the Mars (74), commanded by his father, in 1811, and, in March 1812, became a student at the Royal Naval College, Portsmouth. During the two years and a half which Mr. Raper remained there, he was remarkable for talent and application, as well as for great steadiness and regularity, which, together with extreme amiability, were his distinguishing characteristics throughout life. He early evinced a talent for mathematics,

and received a silver medal at the Royal Naval College examination, in 1814, for his proficiency in that science.

Having gone through the course of instruction at the Royal Naval College with credit, he was appointed to the Nymphen frigate, employed on Channel service, in April 1815; and in October of the same year sent as midshipman on board the Alceste (38), Captain Sir Murray Maxwell, which ship was fitting out to convey the Earl of Amherst as ambassador to China.

The Alceste conveyed the ambassador to his destination in safety, but on her homeward voyage was wrecked in the Straits of Gaspar, in February 1817, when the subject of this notice bore his full share of the hardships experienced by the crew, who, from their forlorn condition on the rocky islet on which the Alceste was wrecked, were in danger of death from thirst, and threatened by large numbers of pirates, whose vessels assembled around in the hope of destroying all the ship's company, and thus obtaining valuable plunder. The Alceste's crew were relieved from this critical situation by vessels sent from Java.

Mr. Raper was next appointed midshipman to the Tyne (26), Captain Gordon Falcon, an officer of talent, who was deemed a remarkably good seaman and navigator. The Tyne was employed three years on the South American station, where Mr. Raper profited by the opportunity of improving his knowledge of nautical matters.

On the return of H. M. ship Tyne to England, in 1820, he was appointed to the Seringapatam, Captain Samuel Warren, in which frigate he served two years as midshipman, chiefly on the home and Mediterranean stations.

In 1822, he was appointed midshipman of H. M. ship Adventure, commanded by Captain W. H. Smyth, employed on surveying service in the Mediterranean, and was, indeed, extremely well qualified for such an appointment by his talents, his taste, and his previous services. Under the eye of the accomplished, scientific captain of the Adventure, Mr. Raper gained a very perfect knowledge of seamanship and navigation.

Admiral W. H. Smyth states, that "Admiral Raper was most anxious that his son Henry should join the Adventure; but in consequence of that ship being employed on the Egyptian coast, it was not till the spring of 1822 that he was able to get on board. On becoming acquainted with his peculiar bent in nautical studies, I placed him in charge of the chronometers, with his former college associate, the late unfortunate Captain Graves. In our next surveying cruise, along the shores of Tunis and the Lesser Syrtis, he proved himself very zealous in the acquirement of a fuller acquaintance with nautical science, and he won my high opinion of his zeal. In giving him certain directions one day, amongst the ruins of Carthage, of which he was about to draw a plan, I remarked to him

that we had not had a really good book on navigation since Robertson's *Elements*, ending with,—'And, Raper, if ever you get upon half-pay, such a work, with all the recent appliances and resources, will be a capital employment for your leisure time.' All the auditors laughed; but I am inclined to think that even then he plotted his most useful work, *The Practice of Navigation*."

Raper was next appointed to the Rochford, the flag-ship of Admiral Sir Graham Moore, in command of the Mediterranean fleet, with a view to his promotion; and in July 1823, he was promoted to the rank of lieutenant on board the Euryalus. He afterwards joined H. M. ship Dispatch, in which ship he remained until she was paid off in 1824.

Not being employed on active service in his profession, Lieutenant Raper assiduously studied the higher branches of the mathematics. He was selected by the Board of Admiralty, in December 1832, with other scientific persons, to form a committee to improve the method of measuring the tonnage of ships.

In 1840, Lieutenant Raper published his *Practice of Navigation*. This may be regarded as a very spirited undertaking, considering that there were already three or four very excellent treatises on that subject, whose well-earned reputation had rendered their titles and their methods of working problems so familiar to all naval men, that anything novel was unlikely to be adopted. Notwithstanding this, the work was so much approved of that it speedily came into general use.

The Royal Geographical Society bestowed a Gold Medal on Lieutenant Raper, in 1841, for this book. The Directors of the East India Company also ordered copies of the work to be supplied to the ships of their navy; and in January 1843, the Lords of the Admiralty directed that a copy of *The Practice of Navigation* should be supplied to each of her Majesty's ships —which is a high and unusual distinction.

Owing to the aid constantly afforded to Lieutenant Raper by the Hydrographic Department of the Admiralty, the Table of Maritime Positions forms a very valuable portion of this work, it being esteemed quite equal, if not superior in correctness, to any that has hitherto been published.

Lieutenant Raper became a Fellow of the Royal Astronomical Society in November 1829. He was for many years on the Council, during several of which he filled the post of Secretary. While thus engaged, he was diligently employed upon the higher mathematics, in pursuance of a plan, which he did not live to complete, of publishing a second volume of the *Navigation*, which should contain attempts at the highest theoretical discussion of all the mechanical questions which interest the scientific seaman, beginning from the time when the ship's keel is laid down. For several years Lieutenant Raper devoted his chief attention to the composition of this

Treatise, but in its progress the manuscript extended, and the work assumed the form of a scientific Treatise on various branches of Astronomy, Mechanics, and Physics, rather than an isolated application. It is to be feared that these writings of Lieutenant Raper's (who was at once so practical and so scientific in his ideas) were not left in a state by which their publication is rendered likely.

It is worthy of notice that Mr. Raper excelled in a peculiar kind of satirical writing, which, without being offensive to individuals, was very effective. There are various anonymous criticisms on nautical proceedings and projects, of which a person who knew the author of one could not help knowing the author of the rest.

In November, 1850, Mr. Raper married a daughter of the Rev. Frederick Ekins, rector of Morpeth, Northumberland. This notice of the life of our lamented Fellow and friend would be incomplete were we not to mention that, thus blessed with a happy home and a companion of kindred pursuits, Raper, although suffering severely during the last eighteen months of his life from rheumatism and neuralgia, continued his labours to the very last days of his existence. Shortly before his death, he sent a communication to the Royal Astronomical Society on "An Improvement in clearing the Lunar Distance;" and he was able to work on his manuscripts until a day or two of his death.

He had been ordered to Torquay by his physician, and died there on the 6th January, 1859, in his sixtieth year.

Richard Taylor, the second son of Mr. John Taylor, of Norwich, and great-grandson of Dr. John Taylor, of that city, was born in 1782. He was educated under the Rev. John Houghton, and early discovered a great fondness for the study of languages. On leaving school, he was apprenticed to Mr. Jonas Davis, printer, of Chancery Lane, to whose business he succeeded.

In 1817 he was unanimously elected one of the Common Council for the ward of Farringdon Without, which office he continued to hold for thirty-five years. In conjunction with Messrs. Wood, Waithman, and Favell, he soon distinguished himself as an enlightened and consistent friend of civil and religious liberty, advocating Parliamentary Reform, the repeal of the Corporation and Test Acts, Catholic Emancipation, and the abolition of every restriction on liberty of conscience and the right of private judgment.

As a member of the Corporation he took an active part in the establishment of the City of London School and the formation of the Corporation Library. He was one of the Committee intrusted with the rebuilding of the Royal Exchange and the restoration of Gresham College.

He joined with William Allen and other friends of edu-

cation in giving a cordial and active support to Joseph Lancaster, and he was one of the original subscribers to University College, of which his eldest brother, John, is still the Treasurer.

Mr. Taylor was a steady and consistent Nonconformist, descended from two of the illustrious band, who, in 1662, resigned their preferment in the Church for conscience sake: he adhered, from conviction, to those religious principles which have been hereditary in his family. His religious opinions were Unitarian.

His acquaintance with Sir Jas. Edw. Smith (in consequence of the friendship between the families at Norwich) began early in Mr. Taylor's life, and most of the works of the Linnean President issued from his press — the *English Botany*, the *Flora Græca*, and others. To this circumstance, probably, may be ascribed his fondness for the study of botany and natural history, as well as the appointment which he held in the Linnean Society.

In 1840 he edited a new edition of Warton's *History of English Poetry*, which he enriched with copious notes, containing many important additions and corrections. He had previously edited a new edition of Horne Tooke's *Diversions of Purley*.

For many years he was co-editor as well as printer of the *Philosophical Magazine*, a very useful publication, which would hardly have survived through so many changes of editorship, and so many commercial crises, if the printer had not been attached to the work by more than commercial ties.

He was early instructed in music, of which he was very fond, especially of the compositions of the old English masters. These he was accustomed to enjoy, and to join in singing at the Madrigal Society of the Purcell Club, having been for many years a member of both.

He died Dec. 1, 1858. For many years of his life he was a Fellow of our Society, and while his health lasted, a very frequent attendant. He was for some years the printer of our *Memoirs;* but his connexion with us neither began nor ended with business relations. His pleasant manners, his fund of information, and his readiness to be of use, made him an acquisition to every Society which he joined; and all felt that his removal made a void in their meetings. For years before his death the state of his health prevented his attending our meetings.

At the Royal Observatory of Greenwich the observations of all classes have been made with the same unremitting activity as in former years, and the reductions are made to keep pace with the observations with still greater punctuality than in former years.

The volume of Observations for 1857 has been for some time completely printed, and the publication of it may be daily ex-

pected. The printing of the volume for 1858 is proceeding rapidly.

The observations of γ *Draconis* with the reflex zenith tube are made without difficulty whenever the state of the sky permits the star to be visible.

The small planets lying between *Mars* and *Jupiter* are always scrupulously observed, though greater difficulty is experienced every year in identifying the whole of them, both from the want of correct ephemerides and from the exceeding faintness of some of those most recently discovered.

All the galvanic operations of the Observatory for the distribution of Greenwich time, for the drop of the balls in the Strand and at Deal, and for the regulation of the Post Office clocks in Lombard Street and at St. Martin's-le-Grand, are carried on with great regularity and with very few failures.

In the *Monthly Notice* for May last is an account of the operations performed at the Observatories of Greenwich and Edinburgh for determining their difference of longitude by means of galvanic signals. The operations were perfectly successful, as far as regards the passing of the current between the observatories; and a very good result would have been obtained, had not the weather at Greenwich been singularly bad throughout the week during which the experiments were made, so that stars could be observed on only one evening; on that evening the partial results were highly satisfactory, and the arrangements at both observatories were proved to be so complete as to leave nothing to be desired in the way of improvement.

The preparations for the mounting of the telescope of the great equatoreal were commenced at the beginning of the present year, and are proceeding rapidly. The polar axis has been framed and mounted with the hour-circle attached to it, and the mounting of the declination-circle is being proceeded with. There is reason to hope that this magnificent instrument will be completed and its telescope in use for astronomical purposes early in the approaching summer.

There is nothing new to report from the Radcliffe Observatory. The work has been carried on during the past year with its accustomed regularity. Since the last report, the 17th Volume of the *Radcliffe Observations* has been published; and the 18th volume is now printed, and will be published in the course of a few weeks.*

The Cambridge Observatory is diligently proceeding on its regular plan.

The planet *Leucothea*, which was looked for in vain in

* The Society will have learned with deep regret that, since our Annual Meeting, the distinguished Director of the Radcliffe Observatory has been carried off in the midst of his valuable labours. Mr. Johnson died suddenly on the evening of the *28th of February*.—A. De M.

1856, was re-discovered by Mr. Breen on October 19 of last year, and was afterwards observed on four other days. Its extreme faintness prevented its being observed a greater number of times. Professor Challis is not aware of any observations of it at other observatories, excepting one made with the Cambridge equatoreal of the United States of America by Mr. Bond, to whom Mr. Breen's observations of October 19 and October 24 had been communicated.

The planet *Calypso*, the fifty-third of the group of minor planets, discovered on April 4 by Dr. Luther, at Bilk, was observed in consequence of a request from the discoverer that, as it is a very faint object, it should be followed with the Northumberland telescope. The Comet II. 1858, discovered on March 8 by Dr. Winnecke, assistant at the Observatory of Bonn, is found to be identical with Comet III. 1819, although it had not been seen till this year since the first discovery. Being engaged with other objects, Professor Challis did not attend to this comet till he received a request for observations of it with the large refractor from Professor Argelander, Director of the Observatory at Bonn, who considered it important, after the comet was found to be one of short period, to obtain observations extending over as long an interval as possible. Mr. Breen succeeded in observing it on the mornings of April 18 and 19, about a fortnight after the latest observations hitherto published. The comet could only be seen a very short time before dawn, when its altitude above the horizon was so small that, to obtain the above observations, it was necessary to cut off the tops of several trees in the south-eastern part of the observatory grounds.

The observations of planets, comets, and stars, were carried on to the end of the year on the same plan as in the previous part of the year (that stated at the top of p. 2 of the Report). Mr. Breen did not succeed in finding *Leucothea* at the opposition in November of last year, either because it was too faint, or because it escaped detection by being in the midst of a multitude of stars in the "milky way."

Donati's comet received particular attention, and drawings were made, and notes recorded, of its physical appearances.

A new determination of the effects of the forms of the transit-pivots made in the autumn of last year, taken in conjunction with those of preceding years, will enable Mr. Challis to reduce with greater exactness the transit observations taken in 1852 for obtaining the longitude of the observatory by galvanic signals, and to apply a small correction to the value of the longitude previously calculated.

Volume XIX. of the *Cambridge Observations* is in course of printing.

Mr. Breen resigned his situation at the observatory at the end of last year, after being assistant something more than twelve years. His place is not yet filled up, and the future *arrangements respecting* the assistants are in some uncertainty.

The most memorable fact relating to the prospects of this observatory, which occurred last year, was the donation by the representatives of the late Mr. Sheepshanks of 10,000*l.* to the University, for the promotion of astronomy. The University has accepted the donation, subject to the conditions that one-sixth part of the proceeds shall be applied to found an Astronomical Scholarship at Trinity College, open to competition by undergraduates of the other colleges; and the remaining proceeds shall be employed in promoting astronomical science in the University, and such other sciences as are usually pursued at an observatory. Among the purposes contemplated in the disposal of this fund are, the payment of assistants and computers at the observatory, and the purchase of astronomical instruments.

There is nothing of especial note to record connected with the Edinburgh Observatory. The determination of the longitude is now most satisfactory. The appointment of another assistant will enable the astronomer to keep the time-ball, which has now been used for some years, in a state of complete efficiency. On the subject of a second expedition to Teneriffe, Professor Smyth remarks:—

"Were foreign opinions needed to confirm those of the Board, most favourable testimony to the expedition of 1856 might be adduced in several languages; and the French *savans*, while hailing the Teneriffe experiment as quite a new era in the methods and proceedings of astronomers, and considering that they also should have a summer mountain observatory, yet with their characteristic courtesy, yield the *pas* to us as having established a scientific right to the Peak of Teneriffe, and content themselves with the Pic du Midi, though nearly 3000 feet lower, and not so favourably situated in latitude. If, then, after having found the experiment to be practicable and eminently successful as soon as tried in 1856, the moderate scheme proposed to the Board in 1852 had been then organised, we should have had in 1857 none to dispute our occupying the best astronomical observing station in this hemisphere, and many to applaud our doing so. But if there be much more delay, this unique opportunity for the Edinburgh Observatory taking a distinguished position amongst rival European institutions will inevitably pass away."

The Madras Observatory has been enriched during the past year by the addition of a new meridional circle resembling those at Greenwich and the Cape, but of a smaller size; being more nearly a copy of that erected by Mr. Carrington at Redhill, with a few slight improvements, the principal of which is a larger object-glass. This instrument reached India in March last, and is now in operation; and will probably be employed chiefly in observations of the planetoids (for which it is well adapted by its powers and construction as well as by geo-

graphical position), and in cataloguing small stars near the equator.

The Director, Captain Jacob, was again compelled by ill-health to leave his post in April last, and will most probably not return to it; his *locum-tenens* being Major Worster of the Madras Artillery. Captain Jacob brought home with him a selection of the most important of his observations during the last two years, the results of which are now in the course of publication in the Society's *Memoirs*.

In his annual report to the Mersey Docks and Harbour Board, Mr. Hartnup states that no material alteration has been made during the past year in the general routine duties of the Liverpool Observatory. The Greenwich Mean Time has been regularly communicated to the port by the dropping of the time-ball, and by the regulation of the electric clocks. The astronomical observations have been published as heretofore in the *Astronomische Nachrichten*, and in the proceedings of the Royal Astronomical Society. The observations taken with the self-registering anemometer and rain-guage have been tabulated for each hour of the day throughout the year. Diagrams of the strength and direction of the wind for each month in the year and for the six years preceding December 31, 1857, have been lithographed by order of the Dock Board. The pressure, temperature, and humidity of the atmosphere, have been regularly observed daily, and compared with similar observations taken during the preceding twelve years. The results thus obtained have been communicated to the Liverpool Medical Officer of Health. In addition to these ordinary duties of the observatory, Mr. Hartnup states in his report that they have, after some years' perseverance in explaining its advantages to mariners, succeeded in establishing a systematic method of testing chronometers, which is now so well appreciated that a business of considerable magnitude has sprung up in that department. The applications to have chronometers tested are gradually increasing, and it will be necessary very soon to have a new and much larger room built for the purpose of testing chronometers in. Mr. Hartnup estimates that there are on the average, in the port of Liverpool, upwards of one thousand chronometers belonging to ships lying in the docks, and he finds it a necessary precaution to test all chronometers when new, or when they have been cleaned or repaired; but he is of opinion that when these instruments are in good condition the less they are out of the hands of captains, or those who use them at sea the better, since it is by keeping a record of their performances at sea and on shore, in warm climates and in cold climates, that their peculiarities will be best made known to mariners who have to use their judgment as to the degree of confidence they are justified in placing in their instruments. He attributes the practice of sending chronometers on shore so

frequently to the difficulty of obtaining accurate time on board the ships lying in the Liverpool Docks; and he is supported in this opinion by the members of the Mercantile Marine Service Association, who have memorialized the Dock Board on the subject of additional time-balls. The desirableness of carrying telegraph wires along the whole line of Docks is now decided on; and it is proposed to connect the various turret-clocks on the dock-quays with the normal clock at the observatory, and to control their movements in the same way that the old turret-clock on the top of the Town Hall has been successfully controlled during the past two years. By this arrangement mariners will, by watching for the first blow of the hammer of either of these clocks, be able to ascertain correct Greenwich Mean Time at any hour of the day.

There has recently appeared in the *Monthly Notices* a letter from Mr. Maclear to the Astronomer Royal containing an account of the results of the Trigonometrical Survey in the Cape Colony. This communication, brief though it be, is eminently worthy of attention. The Cape Arc of the Meridian now extends from Cape Point in latitude 34° 21′ 6″·26 N. to latitude 29° 44′ 17″·66 N.; it has consequently an amplitude of 4° 36′ 48″·60. A comparison between the latitudes at the principal stations as determined by astronomical observation, and the corresponding latitudes calculated from Airy's elements of the earth's figure exhibits a satisfactory accordance. Only at one of the stations, Kamies Berg, does the outstanding difference appear to exceed the probable error of the observations. Mr. Maclear, however, remarks that the attraction of the immense Bushman Table Land, the mean elevation of which above the level of the sea is no less than 3000 feet, is capable of accounting for the deflection of the plumb-line indicated at this station. Captain Clarke has shown that by adopting this hypothesis, Mr. Maclear's results agree admirably with the elements of the earth's figure deduced in the recently published *Account of the Trigonometrical Survey of the British Isles.* The Society, we are assured, will cordially unite with the Council in applauding the ability and indefatigable energy which Mr. Maclear has exhibited in the execution of his arduous undertaking, and in offering him their congratulations on the satisfactory results at which he has arrived.

Mr. Lassell's telescope has been erected in all its ample proportions for about two months, and the great speculum has been in it for some six weeks; but an accident, in itself trifling, which occurred on attempting to turn the telescope about, made him think it prudent to make additions for greater strength and more perfect equilibrium.

With all his desire to keep down the massiveness of structure as much as is consistent with safety, he has got up to a weight of about a dozen tons, supported, if not precisely on three points,

yet on three surfaces respectively of very small area, on which moreover, smooth and steady—and as far as possible easy—motion must be obtained. He believes this is on the point of being done, and as the tangent-screws and apparatus for revolution in right ascension and polar declination are prepared and ready for attachment, as well as the finder, small speculum, and eye-tube, it cannot be long before he will be able to test the figure of the speculum by turning it on a star.

Mr. De La Rue during the past year has continued his experiments in celestial photography, and has made several improvements in the process; his observatory has been devoted chiefly to the delineation of our satellite, of which a great number of very perfect negatives have been obtained. Transparent positive copies on glass, 8 inches in diameter, have been presented by him to several observatories and astronomers; and he informs the Council that Mr. Fox Talbot has kindly proposed to apply his newly-invented art of heliographic engraving to the reproduction of one of the lunar photographs: should this be successful, it is hoped that the distribution of these beautiful objects will become more general. When two of the 8-inch photographs, presenting a sufficient difference in libration, are viewed by means of the reflecting stereoscope, the elevations and depressions of the lunar surface are made remarkably conspicuous, and an opportunity of studying the physical structure of our satellite is afforded which is likely to prove of value. The stereoscope adapted for the display of these photographs was exhibited at one of the meetings of the Society. Mr. De La Rue is at present engaged with experiments in photography with a 4-inch silvered glass reflector made by Dr. Steinheil, who has undertaken to make for him a 13-inch silvered glass speculum, with which it is probable that a considerable advance will be made in consequence of the greater amount of light reflected by silver in comparison with speculum metal, and the probability that the time of exposure of the collodion film will be considerably reduced. He informs the Council that he made one attempt to obtain a photograph of Donati's comet, but that he did not obtain any trace of an image in 60 seconds; this he thinks attributable to the low altitude of the comet at the hour he made the experiment, and not to want of requisite brightness of the comet itself. A severe domestic calamity prevented a repetition of the experiment under more favourable circumstances.

Although Mr. De La Rue was unsuccessful in producing an impression of the comet with his telescope of 10 feet focal length, Mr. Usherwood, an artist residing on Walton Common, succeeded in obtaining, in 7 seconds, a good negative with a portrait lens of short focus. The camera was stationary, hence the image is somewhat imperfect, nevertheless it bears enlargement of four times tolerably well. Mr. Usherwood's residence is situated about 700 feet above the sea-level, and it is possible that

his success is in some degree attributable to this circumstance, but is chiefly due to the large area of the portrait lens and the relative shortness of its focal distance. So far as the Council has been informed this is the only instance of a photograph of Donati's comet having been obtained.

The Photoheliograph erected in the dome of the Kew Observatory under the direction of Mr. De La Rue for the Royal Society, has been at work since the beginning of last March, and excellent photographic pictures of the solar spots and faculæ are obtained. Certain alterations have been made by Mr. Welsh, the Director of the Observatory, in order to regulate the term of exposure of the collodion plate to the sun's action; with these alterations the instrument gives very good results. The time of exposure necessary to produce a wet collodion picture even when the aperture is diminished to about 1 inch, and the image enlarged by the secondary lens to 4 inches in diameter, is only a very small fraction of a second. The apparatus for regulating the duration of light-action on the collodion plate consists in a sliding plate, having a slit which can be increased or diminished in width, and situated near to the collodion plate; this is moved by means of a spring rapidly across the interior of the telescope; and the time of exposure is governed partly by the regulation of the opening in the slit, and partly by accelerating or retarding the motion of the plate across in front of the sensitive plate. The work of the Kew photoheliograph has been of late interrupted by the illness of Mr. Welsh, but it is intended to appoint an additional assistant, whose duty it will be to take photographs of the sun each day that the weather permits.

At the suggestion of Mr. De La Rue, M. Otto Struve has proposed to the Academy of St. Petersburg to establish a photoheliograph in Russia, and the subject has received favourable consideration by that body. Thus it is very probable that Sir John Herschel's suggestion that the sun's spots should be daily recorded at several stations will soon be carried out.

Father Secchi continues to devote his refractor to photography, and has recently succeeded in obtaining a photograph of *Saturn*. The photographs of the moon produced by him have been distributed to several observatories.

Mr. Bond has communicated to the *Astronomische Nachrichten* a series of experiments on stellar photography, made under his direction by the photographers Messrs. Whipple and Black, at the Observatory of Harvard College, in 1857 and 1858. These experiments were made on the double-star *Mizar* and its companion, and on *α Lyræ*, with different apertures of the object-glass, from 1 inch up to the full aperture, 15 inches, and with different times of exposure. The object of these experiments was to ascertain the possibility of classifying stars according to a scale of photographic or chemical magnitudes

analogous to the common optical scale, but differing essentially in the fact of its being based upon actual measurements in the place of the somewhat uncertain estimates hitherto resorted to. Mr. Bond finds that a certain definite exposure depending on the brightness of the star is required before any trace of light-action can be detected on the collodion plate. At the expiration of that interval the photograph is suddenly developed by the clustering together of from ten to twenty molecules within an area of about 1 second in diameter. The number of molecules increases rapidly with the time of exposure, while the boundaries of the photograph extend on all sides. Mr. Bond remarks that the diffusion of light-action over considerable areas is obscure, and that if it were due to the dispersion caused by imperfection of the object-glass, it ought to be checked by reducing the aperture, which is not the case, he is inclined therefore to think that atmospheric disturbances may in part account for the phenomenon. Mr. Bond also finds that the images of stars increase by the addition of equal areas in equal times. Hence that, in comparing the photographic images of two stars, the time of exposure of each has to be taken into account. Mr. Bond has given several tables of comparison between the observed diameters of star-images and those computed according to formulæ he has proposed, which show a remarkable coincidence between the two values, proving the value of the photometric process.

From the date of our last Report till the middle of the month of July, Mr. Carrington was engaged, according to the plan there named, in the observation and reduction of the observed positions of the solar spots, which, from their increased number, and the circumstance of his being for the time without an assistant, was as much as he could attend to. Reductions had just been brought closely up, and the first arrangements considered for a thorough discussion of his accumulated records, when one of those events occurred, which must necessarily disarrange any man's plans, and Mr. Carrington found himself called upon, by the sudden death of his father, to take a leading part in a business of some magnitude, without so much as a day's warning beforehand of the change of occupation which was to come over him. He has not, however, entirely abandoned the astronomical subject he was pursuing, but has found time to deduce one or two conclusions of interest which have already appeared in our *Notices*, and is continuing his usual observations at the opportunities which remain to him, looking forward to a future date when leisure may be again at his command.

Mr. Carrington informs the Council that Mr. Marth, of Durham, though not provided with exactly the kind of instrument required, has at his request taken up the observation of *the* solar spots on the plan pursued by himself, and that Prof.

Secchi, of Rome, has also arranged for similar observations to be continued regularly, though on a different system.

The results which were laid before the Society in November indicate so clearly the necessity of continuing these or very similar observations for many years to come in unbroken succession, that the Council share with Mr. Carrington the anxiety he feels that the thread of labours which are opening out now with a bright prospect of success, may not be broken from want of co-operation. It would give them much pleasure to learn that one of the many observers in the United States had resolved to pursue this study with the continuity and assiduity which it requires; and if they may be permitted to name one above others, they would appeal to Dr. C. H. F. Peters, of Hamilton College, who has already, some years since, shown a personal interest in it, to come to the rescue.

Dr. Nichol, of Glasgow, continues his literary labours, and besides recording and fully discussing the meteorological indications of his station, has for some time past paid particular attention to certain physical features of the moon's surface. He promises the Society shortly an essay on the nature and chronological relations of the long streaks or rays which diverge from many centres, intersect one another, and are often dislocated in their course. Dr. Nichol explains, in a recent letter, that he has hitherto been unable to make much use of the meridian circle with which the Glasgow Observatory is furnished, on account of the optical inferiority of its object-glass. He has lately ordered a new one at his own expense from Mr. Cook, of York, with which he hopes to work with better results. He has also lately brought the whole condition of the Observatory under the notice of the Glasgow University Commissioners, praying for the appointment of a Board of Visitors, and for various ameliorations of its material condition.

At the beginning of this year Mr. R. Norman Pogson, whose name is well known to the astronomical world as discoverer of some small planets, and an observer of variable stars, took charge of Dr. Lee's Observatory at Hartwell. A course of observation has been arranged, and it may be confidently expected that the Society will hear of the results.

The year 1858, generally remarkable for the discovery of several comets, is memorable for the appearance of one of the finest and most interesting of those bodies which have been observed in the present century. Equally interesting to the professional astronomer and to the unlearned gazer in the heavens, it has been the object which has attracted to itself during the preceding autumn the undivided attention of the civilised world, and been the subject of intelligent study and speculation to the philosopher, and the reviver of astronomical tastes to the other classes of the community. It is a remarkable

feature of the present age that every phenomenon which is capable of adding to our knowledge of the facts or the principles of astronomical science is sure to be watched by a sufficient number of skilful observers to prevent the escape of any material circumstance or phase which might afterwards prove of theoretical importance, and to insure the complete discussion of all the physical facts that have been collected. And this is remarkably illustrated by the untiring zeal and industry, joined to the use of powerful instrumental means, and of consummate skill in the management of them, which have been brought into play in the case of the present comet. Not only has Donati, the fortunate discoverer of the comet at Florence, devoted himself unremittingly to the task of observing its positions and physical peculiarities,* but a friendly rivalry has been manifested at all the great European and American observatories to take every advantage of the time of its visibility. Parkhurst discovered it independently at Perth Amboy, New Jersey, United States, on the 29th of June, 1858; Chacornac at Paris has added to his well-established reputation by a most interesting series of physical observations made with singular care and perseverance (Le Verrier's *Bulletin* for Oct. 12); Pape at Altona has not only observed it with every care, but to him also are we indebted for a most elaborate memoir on the subject (*Astronomische Nachrichten*, Nos. 1172–4), in which he has applied Bessel's theory of opposite polarities to account for the oscillatory motion of the nucleus, and the formation of the tail, and has rigorously, and, as far as we can see, successfully, compared the theory with the observed phenomena, at the expense of a great amount of mathematical and computing labour. Professor Mädler of Dorpat (*Astronomische Nachrichten*, No. 1167) has made perhaps the best existing series of measures of the singular disks and aureoles round the nucleus which have been so conspicuous, and excited so much attention in the examination of the head of the comet; while Professor Heis of Münster devoted himself almost exclusively to the observation of the length and boundaries of the tail (*Astronomische Nachrichten*, No. 1169). Professor Secchi was unfortunately absent from Rome till the end of the month of October, but his absence was supplied so efficiently by his assistant, M. Rosa, and he took such good opportunities of seeing the comet himself, whenever a visit to any of the principal observatories that lay in his route permitted him, that he has been able to produce a very instructive and interesting memoir, entitled *Osservazioni della Cometa Donati fatti all' Osservatorio del Collegio Romano*, accompanied by several well-executed drawings of its various appearances. In England good observations have been made by the astronomers at Greenwich during the most interesting

* Donati's physical observations will be found in Le Verrier's *Meteorological Bulletin* for Oct. 25.

period of the visibility of the comet, both for fixing its positions by means of the altazimuth,* and for illustration of its physical peculiarities. The results of the observations are given in the November number of the *Monthly Notices* of this Society, and a series of well-executed chalk drawings was exhibited at the meeting of November 12, made by Messrs. Carpenter and Christy, gentlemen employed as computers at the Observatory.

As might be expected, Messrs. Dawes and Lassell have examined the comet with great attention, and the results of their labours have been given to the Society in two elaborate memoirs, their observations being illustrated by a series of careful drawings, which were exhibited at the meeting of January 14.

At Cambridge an excellent series of observations was made by Professor Challis and his assistant Mr. James Breen, of which the results relating to the physical appearances of the comet are given in the *Monthly Notice* for November 12, 1858. Many other detached physical observations will be found in the *Astronomische Nachrichten* and the *Monthly Notices*, of considerable value for corroboration of important facts which admit of any doubt, but the observations mentioned above are those which will be chiefly referred to by future inquirers into the origin of those grand and mysterious phenomena connected with the development and expansion of comets in the neighbourhood of their perihelia, and of which the ordinary theory of gravitation does not afford an adequate explanation.

As might be expected, the comet was observed most sedulously for the determination of its positions at most of the great observatories in Europe and America. The chief observations yet published are those which were made at the observatories of Dorpat, Florence, Geneva, Greenwich, Kremsmunster and Liverpool in Europe, and of Ann Arbor and Washington in the United States. (*Astronomische Nachrichten*, *Monthly Notices*, and *Gould's Journal*.) The results of the observations made at Cambridge (England) have not yet been made public. Several sets of elements and corresponding ephemerides have been computed by various astronomers, which must of course be considered only as approximative, and as forming the basis of an orbit to be determined by a careful combination and elaboration of all the observations as soon as they can be collected. At first it was thought that a parabolic orbit would satisfy the observations; but this was soon found not to be the case, and several sets of elliptic elements have been computed, differing, as might be supposed, very widely in the periodic time of revolution. The extreme periods of all the elements which we have yet seen are by Bond and Löwy, the former giving 12 years and 32 years in two distinct sets,

* For the reduction of these observations a tolerably accurate ephemeris was required, and this was supplied by the spontaneous zeal of Mr. Lynn, who willingly devoted his leisure to the labour of the requisite computations.

(Le Verrier's *Bulletin* for Sept. 19) and the latter giving 2495 years (*Astronomische Nachrichten*, No. 1164.) Other elements computed respectively by Bruhns (*Astronomische Nachrichten*, No. 1161), by Stampfer (No. 1163) and by Watson (*Gould's Journal*, No. 117), give for the period of revolution 2101, 2141, and 2415 years.

But, interesting as the comet is in its geometrical aspect as describing an orbit of this enormous length, whose proportions we have hope of ultimately determining with some degree of exactness, yet it is in its physical aspects that it offers at the present time most inducement for research and speculation; and a short history of the various phases which it exhibited from the time of its discovery till it was lost in the evening twilight at the latter end of October will be instructive and profitable. The three sets of physical observations which will best repay the labour of attentive study are; 1st, those which were made at the Observatory of the Collegio Romano, by M. Rosa, during the absence of Father Secchi, and which have been incorporated in the excellent memoir before cited: 2ndly, those made by the discoverer, Signor Donati, at Florence: and 3dly, those made by M. Chacornac at the Imperial Observatory of Paris. The memoir published by M. Pape in the *Astronomische Nachrichten*, Nos. 1172–74, in connexion with the observations made by himself and Professor Peters at Altona, and by Professor Mädler at Dorpat, is however by far the most complete and elaborate which has appeared, and must be read carefully by those who would seek to know how far Bessel's theory of solar polarity or repulsive force is borne out by the phenomena exhibited by the comet under discussion. To it we shall recur in the sequel; in the meanwhile we will give a brief sketch of the chief peculiarities exhibited by the comet.

Its discovery took place at Florence on June 2, when it presented the appearance of a faint nebulous patch of light without any remarkable condensation. As the comet's motion towards the sun was slow at that time, it was not till the beginning of September that any of the interesting appearances which drew to it such universal attention were exhibited. About this time it became visible to the naked eye, and a short tail was seen on the side opposite to the sun. The development was afterwards rapid. According to Professor Heis, on September 10, the tail was 3° in length, and on the 27th it had increased to 12°; on October 3 the length was 25°, and on the 6th it was 36°. A greater length is attributed by other observers, but the estimates of Professor Heis are worthy of great respect from the pains which he took to observe this element as his own especial province. At the time of the greatest development, or about October 5 and 6, the spectacle exhibited to the naked eye in the heavens was magnificent; and few persons who were fortunate enough to observe the transit of the comet over *Arcturus* will ever forget the impression made on

them by that sublime spectacle. The part of the comet which passed over the star was but a short distance from the head, and the motion was so rapid that the appulse of the star to the border of the train, the time when it arrived at the comparatively dark space in its axis, and the time of its coming out at the other side, could all be observed without any great tax on the patience of even an ordinary spectator. At this time the tail was sensibly curved, the convex side being uppermost, and in the direction of the motion. It was also well defined on this side, while on the other it was much more vague and indistinct. At one period, October 9, it shot forth a smaller tail, to a certain extent coincident with the larger one, but exhibiting little brushes projecting from the convex side of it. Altogether its appearance was like that of a large ostrich-feather when waved gently in the hand. But though the general spectacle as shown in the heavens to all who chose to gaze on it was so grand and beautiful, yet it is with the phenomena which took place in the head of the comet from the commencement of September to the end of October, as viewed through powerful telescopes, that the astronomer is chiefly concerned. These may be divided into three classes, namely, 1st, those that occurred during the month of September till the time of the perihelion passage on September 30; 2ndly, those that took place after this time near the time of nearest approach to the earth or on October 11; and, lastly, those that were observed at the time of the nearest approach to *Venus*, or about October 17. It may be mentioned in this place that the nearest approach to the earth was about equal to half the distance of the earth from the sun, and that the nearest approach to *Venus* was about one-ninth part of that distance. The appearances observed at Rome will be chiefly followed, as they agree well, on the whole, with those of Donati and Chacornac, and in a summary sketch of the leading features of the phenomena like the present, it is not worth while to record or even to notice the trifling discrepancies which occur between the observers, excepting to mention that they consist, first, in the micrometrical measures of the nucleus, and, secondly, in the large number of envelopes or *aureoles* which Chacornac saw (eight in all) successively issue in a spiral form from the nucleus, and ultimately and successively stream back on each side into the train, as other observers seem not to have recognised so large a number, and a little confusion may have existed in the enumeration of them.

On September 4, according to M. Rosa, the nucleus was bright and well-defined, and on September 11 the tail was about 4° 42′ long, and the nucleus was eccentrically situated within the envelope. On September 16 began those peculiar phenomena, which were watched with such anxiety and interest as long as the comet continued visible. On this day two diverging streams of light shot out from the nucleus, and after separating at the distance of about a diameter from it and pro-

ceeding for a short distance towards the head of the comet, they abruptly turned backwards and streamed into the tail. This is remarkable as being the first observed instance of that violent action by which the matter of the nucleus was constantly ejected from it while the comet was in the neighbourhood of the sun, and which, primarily by rapid expansion, and secondarily by some force apparently projecting it into the tail, formed the type of the various phenomena which were afterwards observed. The appearance of the two streams of light is represented by M. Rosa as similar in form to that presented by long hair when brushed upwards from the forehead, and then allowed to fall on each side of the head.

On the 22nd of September the "hair-parting" had given place to a fan-like bright sector, surrounded by a darkish arc, to which succeeded a brighter semi-circle of nebulous light; and on the 27th the same appearance continued, except that the fan was more spread out, and resembled in some degree the crescent form of *Venus*, with the nucleus projecting a little below the inner boundary, and with the line of cusps sensibly inclined to a perpendicular to the axis of the tail. On September 30, the fan still continued, its axis making with the axis of the tail an angle of 25°, and the black division of the latter being very dark near the nucleus. From this date, or almost immediately after the perihelion passage, was seen a totally distinct class of phenomena in the exhibition of the luminous envelopes which, either in a circular or parabolical form, surrounded the nucleus during the remaining time of visibility of the comet. On Sept. 30 three distinct envelopes were visible, the outer one being very diffused, the second better defined and brighter, and in appearance like the glory round the heads of saints; and the third (separated from the second by an interval less luminous) increasing in brightness towards the nucleus, with which it was at length almost confounded. In the direction of the tail a sector of about 90° was cut off from each of these envelopes, and this space beneath the nucleus was very dark. On October 4 similar appearances existed, except that the obscure arc had become a decided dark aperture and two little rays which were observed on October 2 had become curved. On October 8 great distortions were observed in the form of the outer envelope; and, as was generally observed under all the circumstances of this outstreaming of the matter of the nucleus, it was in that direction very ill-defined, and something very like the ragged ends of heavy rain clouds during a violent shower. On October 9 an additional fan or envelope was observed; and M. Rosa states that "the large one is now widened out at the sides, where the streamers are confounded with or run into the tail; the shadow of the nucleus is now also enlarged, and it seems as if the two streamers issuing into the tail were tending to surround the nucleus again so as to render the nebulosity round as at the beginning of the apparition of the comet." Similar appearances

were observed with some variations till October 15, when another set of phenomena were exhibited in the shape of "comma-like" curved appendages to the nucleus, as if the nucleus had thrown out from itself a large mass of brilliant or burning matter originally in a straight line, and this, by the action of some other force, were twisted violently round, and were ready to fall back again into it. These "commas," or pear-shaped appendages, continued with various modifications of size and form until October 22, when the extreme point of the virgula, or comma, had so far turned back again towards the nucleus as nearly to complete an elliptically-shaped body with its axis very much inclined.

The above are the principal phenomena which were observed by M. Rosa, and which form the chief materials of Father Secchi's memoir. They are better adapted than any others which we have yet seen, both by the continuity of the observations and the care which has been used in the descriptions and illustrative drawings, to convey an intelligible idea of the various and sudden changes of form which rendered the head of the comet so interesting an object of study. It is generally understood that both Professor Donati and M. Chacornac are preparing memoirs which shall embody the observations made by them much more completely than the remarks which have been published in Le Verrier's *Bulletins*, and till these have appeared it will be proper to defer any attempts to reconcile the observed phenomena with theory. In the meanwhile, however, speculation has not been idle, and if no successful attempt has been made to account for all the appearances, yet something has been done in the right direction by various astronomers, and especially by M. Pape of Altona, in addition to the hints thrown out by Father Secchi and M. Faye, which deserve notice.

Secchi attributes the observed phenomena, first, to the action of the sun both by gravity and heat, the heat, as the comet approached its perihelion distance, enormously dilating the gaseous mass of which it is apparently composed, and the unequal gravitation on the different parts of this immense mass producing enormous tides and disturbances. Observing also that some of the most interesting phenomena and the most rapid changes of form were shown, first, about the time when the comet was nearest to the earth, that is, near October 11, and, secondly, when the comet was nearest *Venus* (approaching within one-ninth part of the earth's mean distance from the sun) that is, near October 17, he attributes a direct disturbing influence to those two bodies tending to produce some of the observed irregularities and rapid changes of form. He also observes, as almost all other astronomers have done, from the fact of the light of the tail being polarised in a plane passing through its axis, that the light by which it is rendered visible is reflected from the sun.

In the *Revue Contemporaine* is an article by M. Faye, entitled *Idées Théoriques sur la Comète de Donati*, of which the Abbé Moigno has given an abstract in the Paris *Cosmos* for November 26, 1858. M. Faye proves that no mechanical action of the sun and of the mutually attracting particles of the cometary mass is sufficient to account for the observed phenomena; and he attributes them to some as yet unexplored action of the rays of the sun, which he presumes may be endowed with electro-magnetic as well as with calorific properties, and he refers finally to the theory before hinted at of Bessel who "attributes to the solar rays the property of producing polar forces or energetic attractions and repulsions in the cometary masses, but who has not been able to give a name to this property, although he has found it necessary to assign to the special action with which he endows the solar rays an intensity nearly the double of that of the attraction of the enormous mass which emits them."

The hypothesis of Bessel then, imperfect as it undoubtedly is, is the only one which offers anything like a solution of the difficult problem of reconciling with theory the observed phenomena exhibited by comets in their approach to the sun; and astronomers no doubt will make this the basis of their researches. It at least offers this advantage, that it can be submitted, under certain assumptions, to mathematical computation, and thus, even if it be not true, by reducing to some kind of law the observed phenomena, it may be conducive to the ultimate discovery of a true theory. One fact deserves to be stated distinctly before entering upon an explanation of the theory, namely, that the comparison of the appearances of Donati's comet with those of other large comets, especially those of the great comet of 1744, as described by Heinsius (*Acta Eruditorum* for 1745), and of Halley's comet at its apparition in 1682, (as delineated by Hevelius and reproduced by Admiral Smyth in his paper on Halley's comet in the ninth volume of the *Memoirs* of this Society) and also at its next apparition in 1835, proves that the phenomena are much less arbitrary than is generally supposed, and that they only differ as the individuals of a class differ from each other, and may be all combined in the attempt to develope any theory, due regard being had to the distances from the sun, the velocities, and other circumstances which may vary the phenomena.

Bessel, at the close of his memoir on the Comet of Halley, in No. 302 of the *Astronomische Nachrichten*, expounds his theory in these words:—"My exposition of the possibility of a connexion of all the phenomena observed in the comet is the following:—Every action of one body on another can be resolved into two parts; of which one is the same for all particles of the latter, and the other consists of the differences of the actions on the different particles. If the action at very great distances of the bodies from each other be very small, then it is

the former portion of it which, on the approach from these distances to smaller ones, first becomes sensible; the other may acquire a sensible magnitude afterwards. In the case of a comet, therefore, which at a very great distance is coming back to the sun, the first action which is shown is that which is common to all its particles; I assume that it consists in a volatisation of particles, which have opposite polarity to the sun (*welche der Sonne feindlich polarisirt werden*). The other part of the action, which is not sensible till later, may have, as a consequence, a polarization of the comet itself, and thus an especial outstreaming towards the sun. If observations really show these phenomena, as was the case in the Comet of 1744 and Halley's Comet, it cannot be denied that the outstreaming, since it issues from a part of the surface turned towards the sun, and therefore having a polarity of the same kind (*ihr freundlich polarisirten Theile*), possesses also that polarity which endeavours to bring the outstreaming particles nearer to the sun. That the outstreaming particles are, notwithstanding, driven back by the sun, as the observations show, may perhaps be explained by this consideration, that the outstreaming takes place into a space which is already filled with matter having opposite polarity and continues to be filled with it, whereby the opposite polarities balance each other, and the outstreaming particles lose the more of their original property and acquire the more of the antagonistic, the farther they recede from the nucleus of the comet."

This is Bessel's own exposition of his theory of opposite polarities, which depends upon several gratuitous assumptions, and which is therefore subject, *in limine*, to grave doubts — still some hypothesis of this nature is evidently required to explain the phenomena observed in so many comets, of particles streaming with great force from the nucleus towards the sun, and then turning back again away from the sun to form the tail. Bessel himself has applied the theory to the observed phenomena of Halley's Comet, and Pape has done the same for that of Donati, the work in each case involving a large amount of computation; and both astronomers are, on the whole, well satisfied with the resulting agreement between theory and observation, both with regard to the oscillatory motion of the outstreaming matter, and to the formation of the tail. But, whatever may be thought of the theory as capable of explaining *all* the phenomena, still it has great claims on the attention of astronomers as the only one which gives a tolerably satisfactory explanation of *any*, and that, by the simple assumption of certain forces brought into play under certain conditions, and without any *à priori* assumptions whatever as to the law or intensity of the forces, except the variation according to the inverse square of the distance. The intensity of the force, as deduced from the observations by Bessel in the case of Halley's Comet, is nearly double that of the gravitating

power of the sun, and in the contrary, that is, a repelling, direction, while Pape, in the case of Donati's Comet, finds different values for different periods before and after October 9, at which time a rather abrupt change of figure took place.

The Council have thought it worth while to enter thus minutely into the details of the observations and discussions respecting Donati's Comet with the idea of impressing upon astronomers their sense of the importance of its appearance as tending to throw light upon some of the physical peculiarities of these mysterious bodies, and also of saving trouble to zealous inquirers by giving information with respect to the works wherein the chief materials at present exist for obtaining the data of future research, and of pointing out the state of the problem as it has been left by Bessel and other great astronomers. The apparition of so interesting an object as Donati's Comet has a wonderful influence in awakening an interest in astronomical science generally; but especially is it important in quickening the zeal of those who are capable, by their mathematical attainments, of taking part in the solution of the abstruse problems connected with any physical theory which may explain the phenomena of cometary bodies, and of deducing from the observations all the information which they are capable of conveying.

Since the preceding account was written, an excellent account of the observations made at the Observatory of Harvard College, Cambridge, U.S., has been published by Mr. G. P. Bond. The account is extracted from the *Mathematical Monthly Magazine* for November and December 1858, and is accompanied by two fine steel engravings and twenty-one woodcuts. Since it can be purchased at a low price, and will probably be in the hands of most English astronomers, the reference here given will be sufficient.

During the past year a volume has been published by Col. James, Superintendent of the Ordnance Survey, containing an account of the operations for determining the principal Triangles of the Survey, and the investigation of the figure, dimensions, and mean density of the earth, hence deducible. The researches recorded in this immense volume (extending to nearly 800 quarto pages) are executed in the most complete and masterly manner, and reflect high credit on the mathematical talents of Captain Clarke, to whom the more important portion of the volume is attributed. Some of the principal results have been already brought before the notice of the Society, both in the Annual Report of last year and in a subsequent number of the *Monthly Notices;* but the Council could not omit this occasion of expressing their unreserved appreciation of a work which is destined in future ages to occupy an important place in the history of geodesical science.

In the *Berlin Jahrbuch* for 1861 there appears a remarkable paper by Professor Encke, in which the author has collected together the results of his various researches on the existence of a resisting medium, extending over a period of nearly thirty years. As an abstract of this paper has already appeared in the *Monthly Notices*, it will not be necessary, on this occasion, to enter into any details with respect to its contents; but the Council cannot omit to express the high opinion which they entertain of its value as a contribution to physical astronomy. It may be stated that the researches of Professor Encke are founded exclusively on the observations of the comet which bears his name. Soon after his discovery of the periodicity of the comet in 1819, he was led to remark that the time of revolution, as determined by a comparison of the apparition of that year with the previous apparitions in 1805, 1795, and 1786, was gradually diminishing; and it occurred to his mind, at this early period of his labours, that the effect might be due to the influence of a resisting medium. On every subsequent occasion of the comet's passage of the perihelion he has calculated beforehand an ephemeris of its apparent place, extending over a sufficient interval of time, taking into account the effects of planetary perturbation, and introducing the hypothesis of a resisting medium; and the results thus obtained have been found, in general, to represent the actual positions of the comet within the limits of the probable errors of observation. The quantity by which each successive revolution of the comet has been found to diminish amounts to $\frac{11}{100}$ths of a day, or rather more than two hours and a half. While the existence of a gradual diminution of the time of revolution of the comet must now be universally admitted as a rigorously demonstrable truth, it must be borne in mind that the establishment of this important fact is wholly due to the able and persevering exertions of Professor Encke. The annals of astronomy contain few more brilliant examples of successful achievement, whether viewed in regard to the pertinacity with which the researches were pursued, the confidence by which the inquirer was invariably sustained throughout his arduous labours, or the importance of the final result. With respect to the physical cause of the fact discovered by Professor Encke, differences of opinion may be entertained in the present state of the question; but it may be reasonably expected that a result of such importance will in due time lead to conclusions of high interest in astronomical physics.

The Council would direct the special attention of the Fellows of the Society to the New Solar Tables by M. Le Verrier, recently published in the fourth volume of the *Annales de l'Observatoire*. This volume contains not merely the tables, but the whole materials of their construction, treated in the most masterly manner. First, the author gives the general

theory of the earth's movement, and rigorously calculates the effects of planetary perturbation. He then proceeds to a discussion of the observations, by means of which the elements of the terrestrial orbit are corrected. These consist of nearly nine thousand right ascensions of the sun, as determined at the Observatories of Greenwich, Paris, and Königsberg, between the years 1750 and 1850. Besides general tables, in which the values of the various inequalities are distinctly computed for successive values of the arguments, there are special tables for the computation of ephemerides, founded upon the results contained in the general tables. In the latter instance the sun's longitude and radius vector are computed with great ease by a process of admirable simplicity. It is worthy of remark, also, that the right ascensions may be found with the same facility, independently of the longitudes. Tables distinguished by practical advantages of such undisputed value cannot fail to recommend themselves to the astronomical computer.

The year 1858 saw the conclusion of the series of star-charts published by the Academy of Berlin. Distinguished as are the names of the contributors to this celebrated work, the retrospect of the undertaking can hardly be regarded as encouraging to those who look to great results from voluntary co-operation. Thirty years have elapsed since their commencement, and many a chart has had to be transferred to other hands than those which were pledged to its performance. A fair estimate of the time necessary for work thus apportioned would be three years for the observation, and five for the observation and publication; yet thirty have really been required. The result is, that though the series has at last been concluded, and the latter sheets prepared consistently with the original plan, other charts have long since been set on foot, of larger scale and greater detail, and the later members of the family have been much depreciated in value by the progress of discovery in the department for which they were specially designed. Nevertheless, the formation of these charts gained for Berlin the honour of the first recognition of the planet *Neptune*, and greatly promoted the search for minor planets in their earlier days. The execution of the engravings has been uniformly good throughout, and the muster-roll of names which appear upon them will ever have historical interest.

Dr. Argelander's survey of the northern heavens is steadily progressing at Bonn, and the third series of his lithographed charts has appeared. He has for some time expressed the wish that some countryman of ours, with "*Englische Geld und Englische Kraft*," will undertake a similar survey of the southern hemisphere from some station such as the Cape of Good Hope.

The sun was twice totally eclipsed during the year,—in the months of March and September. The augmented diame-

ters of the sun and moon were, on the first occasion, so nearly equal, that while the eclipse was complete in the neighbourhood of the Canary Isles, it was just annular in the part of its track which crossed England. Considerable preparations had been made for its due observation over a large tract of country; and, in curious contrast to the terror of bygone times, were to be seen on most of our railways bills announcing that excursion trains would convey amateur astronomers—schools at reduced charges—to the spot on their different lines previously indicated by the computers. Numbers availed themselves of the opportunity; but our cloudy spring climate doomed the greater part to disappointment, a broken pack of light clouds coming over the scene from the north-west, and, with a few exceptions, obscuring the view of successive groups of gazers shortly before the greatest phase. The physical appearances attending the rupture of a thin strip of sunlight were best seen at Cambridge by Mr. Breen, who, having the use of an instrument of great power, was able to add something to the particulars of former descriptions. The central line was found to deviate sensibly from its position computed from the tables of Hansen, but the deviation was subsequently shown by Mr. Farley to be accountable for by very minute errors of geocentric place. The *Monthly Notices* contain the chief accounts of observers on this occasion.

For the September eclipse further preparations were made, and a pamphlet was written by Mr. Carrington, containing some necessary information and suggestions. Whether from the partial failure in March, or from other discouragements presented by the climate, the distance, and the unavoidable expense, no observer in this country could be induced to volunteer for a voyage to the Brazils; and though some efforts were made to organise an expedition, and Mr. Robert Stephenson again offered his yacht Titania for the voyage to a well-known member of our Council, the opportunity was neglected as far as this country was concerned, and the French Government sent out the only European observer who, in the end, contributed to our knowledge on the occasion. M. Liais, the astronomer alluded to, connected himself with an expedition to Paranagua organised by the Brazilian Government; and the joint report produced by the Commission shows that their well-directed efforts were not baffled by climatic obstructions, though they ran more risk from this evil chance than had been anticipated. The report referred to will be found published *in extenso* in Nos. 1170, 1171 of the *Ast. Nachrichten*, and its main features in a condensed form in the *Monthly Notice* for November last. The sketch of the appearances seen during the total obscuration of the sun, which accompanied the Report, has been framed, and now hangs in our meeting-room. The corona visible during the interval of total obscuration, appears from the various accounts to have

exhibited an unusually complicated structure on this occasion. Several conical brushes of light, with convex sides, were seen around the moon's limb extending to an average distance of 13′, besides two radial emanations of greater length, darting out from beneath the opposite extremities of the moon's horizontal diameter. From an observation made by M. Liais, it would appear that the conical brushes of light were in reality appendages of the sun, and not of the moon. Several protuberances were remarked around the moon's limb during the totality of the eclipse. In some instances they exhibited the usual rose-colour, but in general they had a white aspect. The sun's disk had been carefully scanned a few days beforehand by various observers, with the view of being enabled subsequently to ascertain whether any of the spots or faculæ would correspond in position to the protuberances which might be seen during the eclipse, but no such relationship between the two classes of phenomena was discoverable.

Since the last Anniversary of the Society four new members of the group of minor planets have been added to the solar system. The first, *Calypso*, was discovered on the 4th of April, at Paris, by M. Goldschmidt; the second, *Alexandra*, was discovered by the same indefatigable observer on the 10th of September; the third was discovered also on the same evening by Mr. Searle, at the Observatory of Albany, U.S. The circumstances connected with the discovery of the fourth planet are of an unusually interesting character. It will be in the recollection of the Fellows of the Society that the planet *Daphne* was discovered by M. Goldschmidt on the 22d of May, 1856. At the time of its discovery the planet had considerably passed opposition, so that only four observations of it could be obtained before it ceased to be visible. An orbit was calculated from these observations by M. Pape, but it was justly feared that in consequence of the small extent of the arc of observation, considerable difficulty would be experienced in the rediscovery of the planet upon its coming to opposition in the following year. However, on the 9th of September, 1857, M. Goldschmidt, aided by an ephemeris founded on M. Pape's orbit, discovered a planet which was supposed to be *Daphne*, and a considerable number of positions of it were obtained at various observatories during the period of its visibility. It has already been mentioned in the *Monthly Notices*, that the object discovered by M. Goldschmidt on the 9th of September, 1857, was in reality not *Daphne*, but a new planet, and that the detection of this curious fact is due to M. Ernest Schubert. The aggregate number of small bodies constituting the group of minor planets now amounts to fifty-six.

There a practical consideration which the continued ad-

dition of planetoids to the known system brings each year more into view, which it may be worth while to repeat on this occasion with further distinctness. It is the constant and pretty regular decrease of brightness, or so-called magnitudes, of the successive additions. For instance, we find a mean-opposition-magnitude of 8·6 for the average of the first ten, of 9·6 for the second ten, of 10·4 for the third ten, of 10·9 for the fourth ten, and about 11·4 for the fifth ten; numbers which indicate pretty plainly what the optical requirements of the successful observer will be in the immediate future, if the course of discovery is to continue as of late. These numbers also indicate that a great eventual saving of labour may be made by *at once* carrying the mapping of stars in the regions of search to a degree of minuteness, many degrees of magnitude beyond that of any existing charts, and by furnishing observers engaged in the work of search with instruments of corresponding power. At present the sieve is too coarse.

These remarks will sufficiently show that the Council are not of those who think that the progress of discovery in this department is or may be too rapid; on the contrary, while the direct stimulant of personal reward seems scarcely needed by the untiring zeal of competing astronomers, they continue to point with increasing satisfaction to their labours as one of the most remarkable instances of continued progress in the history of astronomy, and to a dense body of atoms of fact which have changed the face of the solar system, and are essential as features in any great speculation as to its past history and formation.

The twelve months which have elapsed since our last Anniversary have been distinguished by the discovery of four new comets and the rediscovery of two periodic comets.

The first comet was discovered on the 8th of March at Bonn by Dr. Winnecke. A comparison of its parabolic elements with those of a comet observed in 1819, seemed to indicate that both bodies were identical, and, in fact, it was found by the discoverer that the observations would be best satisfied by an elliptic orbit with a period of 5·549 years. Supposing the comet to have made seven revolutions since its apparition in 1819, the time of revolution would be 5·541 years, a result agreeing very closely with that derived from theory. It can hardly be doubted, therefore, that both apparitions refer to the same object, and that the time of revolution has been determined with considerable precision.

The second comet was discovered on the 2d of May at the Observatory of Cambridge, U.S., by Mr. Tuttle. A parabolic orbit has been found to satisfy the observations.

The third comet was discovered on the 21st of May at the Berlin Observatory by Dr. Bruhns. In this case, also, the elements have been found to be sensibly parabolic.

The fourth comet was discovered on the 2d of June, at the Observatory of Florence, by Dr. Donati. This is the most splendid comet which has appeared during the present century, with the exception of the great comets of 1811 and 1843. The Council, in another part of their Report, have given a detailed statement of some of the more remarkable features exhibited by this interesting object. It may be mentioned briefly that the researches of Dr. Bruhns would seem to indicate that the comet revolves in an elliptic orbit with a period of rather more than two thousand years, but, of course, the definitive results can only be deduced by a discussion of the totality of the observations.

The two periodic comets which have reappeared during the past year are those of Encke and Faye; Encke's Comet was discovered at the Berlin Observatory on the 7th of August, by Dr. Förster. The remarkable researches of Professor Encke on the continued diminution of the time of revolution of this comet have been amply confirmed by the observations made on the occasion of its last apparition.

The rediscovery of Faye's Comet was effected on the 8th of September, at the Berlin Observatory, by Dr. Bruhns. It is much to be desired that a rigorous comparison be instituted between the observations of this comet and the results of theory, in order to ascertain whether there exist any indications of a resisting medium, as in the case of Encke's Comet.

Papers read before the Society from February 1858 *to February* 1859.

1858.

Mar. 12. Micrometrical Observations of Double Stars. Mr. Gubbins.

Ephemeris of Variable Stars. Mr. Pogson.

Observations of *Saturn* and the Sun. Lieut. Hardy.

Note on the Solar Spots. Capt. Shea.

Observations of a Solar Eclipse. Mr. Nicholson.

On a mode of representing very approximately the projection of a Great Circle on Mercator's Charts. Mr. Airy.

On the Problem of Disturbed Elliptic Motion. Mr. Cayley.

Elements and Ephemeris of Comet I. 1858. MM. Bruhns and Pape.

Phenomena attending Lunar Eclipse, Feb. 27, 1858. Lieut. Hardy.

On an Aurora Borealis. Dr. Forster.
Suggestions respecting the Annular Eclipse of March 15, 1858. Mr. Airy.
On D'Arrest's Comet. M. Argelander.
On some Modifications, &c., in Telescopes. Mr. Carrington.

April 9. Observations of the Solar Eclipse, March 15. Admiral Smyth.
Ditto ditto Mr. Weston.
Ditto ditto Mr. Breen.
Ditto ditto Mr. Whitbread.
Ditto ditto Sir W. K. Murray.
Ditto ditto Mr. Wilkinson.
Ditto ditto Messrs. W. & J. Simms.
Ditto ditto Rev. J. Slatter.
Ditto ditto Mr. Stuart.
Ditto ditto Mr. J. G. Barclay.
On the Changes of the Line of Cusps in Solar Eclipses. Mr. Dobson.
Observations of *Saturn*. Sir W. K. Murray.
On the Polar Distances of the Greenwich Transit Circle, Part I. Mr. Marth.
Observations of Solar Eclipse, March 15. Rev. W. R. Dawes.
Ditto ditto Prof. C. P. Smyth.
On the Evidence which the Observed Motion of the Solar Spots offer for the Existence of a Solar Atmosphere. Mr. Carrington.
Observation of a Bright Star near *Saturn*. Mr. Lassell.
Observations of Solar Eclipse, March 15. Mr. Glaisher.
Ditto ditto Prof. Challis.
Ditto ditto Mr. W. Simms, jun.
Ditto ditto Mr. Cross.
On Col. James' Geometrical Projection of the Sphere. Mr. Airy.

May 14. On the Projection of the Sphere. Sir J. Lubbock.
On the Determination of the Latitude with a Transit Instrument. Capt. Clarke.
Observations of Solar Eclipse, March 15. Mr. Lang.
Observations of Comets, 1857-58. Mr. Hartnup.
Ditto of *Saturn*, ditto Mr. Lassell.
On the Annular Eclipse, March 15. Prof. Challis.
On the Determination of the Longitude of the Observatory, Edinburgh, by Galvanic Signals of Star Transits. Mr. Airy.
Observations during Solar Eclipse, March 15. Rev. C. Pritchard.

June 11. On Observations of *Venus* near the Sun. M. Mädler.
On Personal Equation. Mr. Mitchell.

On the Probable Error in Eclipse or Occultations. Lieut Ashe.
Observations of the Solar Eclipse, March 15, at Greenwich. Mr. Airy.
Occultation of *Regulus*. Mr. Baxendell.
On a Solar Spot. Capt. Noble.

July 9. On the Reduction of Occultations. Capt. Clarke.
Observations of Small Planets at Greenwich. Mr. Airy.
On a Method of Clearing the Lunar Distance. Lieut. Raper.
On the Development of the Disturbing Function in the Lunar Theory. Mr. Cayley.
Observations of a Comet. Dr. Donati.

Nov. 12. Catalogue of 317 Stars exhibiting Proper Motions. Capt. Jacob.
Observations of Donati's Comet. Mr. Lassell.
Thoughts on the Formation of the Tail of a Comet. Mr. Waterston.
Observations of Donati's Comet. Lieut. Chimmo.
Note on the Figure of the Earth. Capt. Clarke.
Observations of Donati's Comet. Mr. Haile.
On Lieut. Raper's Mode of Clearing the Lunar Distance. Mr. Riddle.
Suggestions for Improving Refracting Telescopes. Dr. Eve.
Observations of Donati's Comet. Mr. Selby.
Ditto ditto Rev. J. Slatter.
On the Value of the Neglected Terms in the Ordinary Expression for the Equation of Equal Altitudes. Mr. Riddle.
Observations of Donati's Comet. Mr. Burr.
On the Solar Eclipse, Sept. 6. Major Lang.
On Donati's Comet. Mr. Drach.
Observations of Donati's Comet. Rev. T. W. Webb.
Observations of the Total Eclipse of the Sun, Sept. 1858, made at Paranagua. M. De Mello, and others.
On the Distribution of the Solar Spots in Latitude. Mr. Carrington.
On a New Variable Star R *Sagittarii*. Mr. Pogson.
Remarks on the Appearance of Donati's Comet. Mr. Airy.
Proper Motions of the Stars in the Greenwich Catalogue of 1576 Stars for 1850. Rev. R. Main.
Micrometrical Measurement of 120 Double and Multiple Stars 1856–8. Capt. Jacob.
On the Nomenclature of the Minor Planets. Mr. Hind.
Notice of an Error in the late Edition of the "Outlines of Astronomy." Sir J. Herschel.

Observations of Donati's Comet. Mr. Gray.
Dec. 10. Physical Observations of *Jupiter*. Sir W. K. Murray.
Measures of *Saturn* and his Rings. Capt. Jacob.
Ditto ditto Satellites. Capt. Jacob.
Physical Observations of *Jupiter*. Mr. Lassell.
Observations of Comet V., 1858. Mr. Hartnup.
On the Variable Star 83 Greenwich Catalogue of 1576 Stars. Mr. Airy.
Observations of Donati's Comet. Mr. Maclear.
Observations of Comet V., 1858. Lieut. Balfour.
Measures of *Jupiter* and his Satellites. Capt. Jacob.
Occultations of Stars observed at Madras. Capt. Jacob.
Remarks on Rev. C. Pritchard's Paper on the Conjunction of *Jupiter* and *Saturn* B.C. 7. M. von Gumpach.
Remarks on a Note on Oltmann's Calculation of the Eclipse of Thales, by G. B. Airy, Esq. M. von Gumpach.
First part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary
1859. Theories. Mr. Cayley.
Jan. 14. Occultations of Stars by the Moon. Capt. Noble.
Physical Observations of *Saturn*. Capt. Noble.
On the Determination of the Latitude from Observations made in the Prime Vertical. Capt. Tennant.
Observations of Donati's Comet. Mr. Lassell.
Notes on the Zodiacal Light. Capt. Jacob.
Observations of Eclipses of *Jupiter's* Satellites. Capt. Jacob.
On certain Phenomena in the Motion of Solar Spots. Mr. Carrington.
Observations of Donati's Comet. Rev. W. R. Dawes.
Observations of Minor Planets. Mr. Airy.
Supplementary Paper on the Theory of Disturbed Elliptic Motion. Mr. Cayley.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Institution.
Royal Dublin Society.
Geological Society.
Linnean Society.

Photographic Society.
Philological Society.
Zoological Society.
Cambridge Philosophical Society.
Society of Arts.
Philosophical Society, Manchester.
Historic Society of Lancashire.
Art-Union of London.
British Association.
Institute of Actuaries.
Liverpool Literary and Philological Society.
British Meteorological Society.
British Horological Institute.
The Lords Commissioners of the Admiralty.
The Registrar-General.
The Radcliffe Trustees.
The Royal Irish Academy.
Committee of Council on Education.
The Postmaster-General.
University College, London.
University of London.
The London Library.
Government of British Guiana.
Royal Society of Tasmania.
L'Académie Impériale des Sciences de l'Institut de France.
Le Dépôt Général de la Marine.
The Society at Cherbourg.
Imperial Academy of Vienna.
Royal Observatory at Munich.
The Royal Academy of Sciences, Munich.
Royal Academy at Berlin.
The Society of Physics at Berlin.
Royal Academy, Brussels.
Royal Observatory, Brussels.
Royal Society, Göttingen.
The University at Göttingen.
Royal Observatory, Königsberg.
Royal University of Christiania.
Royal Academy of Sciences, Amsterdam.
Royal Danish Academy of Sciences.
Royal Academy, Naples.
Royal Academy of Sciences, Lisbon.
The Society of Sciences, Leipsig.
Royal Academy of Sciences at Turin.
The Academy of Sciences, Stockholm.
The Society of Sciences, Zurich.
The Imperial Academy, St. Petersburg.
Royal Observatory, Palermo.
The Dudley Observatory.

The Observatory at the Collegio Romano.
Observatory at San Fernando.
The Department of State, Washington.
The Franklin Institute.
The Canadian Institution.
The Editor of the Athenæum.
The Editor of the Literary Gazette.
The Editor of the Critic.
The Editor of the Civil Engineers and Architects' Journal.

R. Abbatt, Esq.
G. B. Airy, Esq.
M. Moritz Allé.
M. J. B. Biot.
W. R. Birt, Esq.
Thos. Brett, Esq.
J. A. Broun, Esq.
Dr. Cabish.
R. C. Carrington, Esq.
Rev. Prof. Challis.
E. H. Coleman, Esq.
M. D'Abbadie.
Prof. De Morgan.
S. M. Drach, Esq.
Prof. Encke.
Chas. Frodsham, Esq.
Dr. J. G. Galle.
Prof. A. Gautier.
Jas. Glaisher, Esq.
Dr. B. A. Gould.
H. Hennessey, Esq.
J. Herapath, Esq.
M. M. Hoek.
Messrs. Hannay and Dietrichsen.
M. J. Hornstein.
M. J. Houel.
Dr. Hume.
Prof. Jolly.
Mrs. Hannah Jackson.
Dr. Lee.
M. Le Verrier.
M. K. Von Littrow.
M. Moritz Löwy.
Lieut. Maury.
M. L'Abbé Moigno.
Rev. W. Monk.
J. R. Morrison, Esq.
Prof. B. Peirce.
Dr. Peters.
M. Plana.
M. Plantamour.
Rev. B. Powell.
M. Quetelet.
Geo. Rennie, Esq.
Prof. A. Secchi.
W. H. Simms, Esq.
Prof. C. P. Smyth.
R. B. Smyth, Esq.
M. O. Struve.
R. Taylor, Esq.
Lieut. Tennant.
C. Todd, Esq.
M. Yvon Villarceau.
Mr. J. Williams.
Dr. R. Wolf.
M. Wolfers.

Address delivered by the Chairman, the Rev. Robert Main, on presenting the Gold Medal of the Society to Mr. Carrington.

Gentlemen,—The continued illness of our excellent and respected President has made it necessary for the Council to depute to another member of their body the task of laying

before you the grounds on which they have assigned the Gold Medal of the Society to our worthy Secretary, Mr. Carrington, for his *Redhill Catalogue of Circumpolar Stars.* The Council have done me the honour to request that I would take this duty upon myself; and I have not hesitated to accede to their wishes, first, because the recognised value and excellence of the work make the task comparatively an easy one, and, secondly, because my daily practice in the routine of a large observatory (of which the meridional instrument is a transit-circle, which the author of the Redhill Catalogue took as his model) has of necessity made me familiar with the processes both of observation and accurate reduction, which are described so luminously by him in his Introduction.

There are two points of view from which every work like the present must be regarded, in order that a judgment may be formed of its merits; namely, its utility, and the excellence of its performance. A work may be well executed, but it may be of no practical utility, or may be only a repetition of what has been done before; or, it may be needed, but it may be so ill executed that the results could not be confidently applied to their intended object. Astronomical science within the last fifty years will easily supply us with illustrations of both these defects in works of importance; but, for obvious reasons, it would not be proper to produce any. I need only show that, in the instance before us, the work is one of great utility, and at the same time of standard excellence, and performed under such circumstances as to render the bestowal of our medal only a proper and fitting acknowledgment, on our part, of the merit which the scientific world has universally acknowledged already.

The utility of any great work must be estimated, in some degree, with reference to the age which produces it. A nation emerging from barbarism, and acquiring by slow and painful degrees the arts of civilisation, invariably commences with those which are fundamentally necessary for its support and extension. It first provides for the bodily sustenance, and for the social and political security of its subjects, before it has leisure to attend to, or cultivation to appreciate, those arts and sciences which are ultimately as necessary to its well-being in its more refined state as were the rude arts by which its citizens were supplied with daily bread at the commencement.

The case has been precisely similar in the organised efforts made to construct and to extend the science of modern astronomy since its commencement in the seventeenth century. Beginning with Flamsteed, the fundamentals of every department of the science had to be determined almost without the help of any adequate theory. The places of the brighter stars were to be determined as fundamental zeros; the elements of the solar and lunar and planetary orbits were to be corrected; and the position of the equinox, as well as the quantity of the

precessional motion, was to be determined with some degree of precision. In a short time physical and mensurative astronomy began to go hand in hand, and mutually to lend assistance to each other. The discoveries of Newton had shown the physical causes of the irregularities in the apparent motions of the stars, detected by the powerful though ill-contrived instruments of the eighteenth century; and the genius of Bradley was adequate to the task of tracing the sources and the laws of aberration and nutation, after having himself observed their effects. Astronomy was thus placed upon a sure basis. The instrumental means employed towards the close of the eighteenth century, though far from faultless, were sufficient to give the values of all the constants needed in the various branches of astronomy within very narrow limits of error; and to English industry and skill must be conceded the merit of having almost exclusively supplied the materials for doing this.

But the progress of astronomy in the present century soon rendered necessary an extension of its objects and purposes. And it is a fact worthy of notice, gentlemen, that the illustrious astronomer to whom we are indebted for the elaborate and complete discussion of the English observations of the eighteenth century, was the first to set the example of a more extended and systematic survey of the heavens. It is just thirty years ago when the medal of this Society was presented to Professor Bessel for his zone-observations; and Sir John Herschel, who then occupied the presidential chair, used words, while inculcating the importance of carrying out effectually that grand sidereal survey of which the equatoreal zone was the first-fruit, which, coupled with succeeding discoveries, we may well consider to be prophetical.

"Could it be permitted to us," he says, "to look forward and draw aside the veil which a few centuries interpose between us and its interpretation, we might expect to see all the great questions which agitate astronomers set at rest, and new ones, more refined, and grounded on their solution, arising. Some minute and telescopic atom will perhaps have become the stepping-stone between our system and the starry firmament—its parallax will mark it for our neighbours—and either its fixity will demonstrate the equilibrium of our sidereal system, or its proper motion reveal to us the nature and extent of the forces which pervade it."

I need not remind you, gentlemen, that such a stepping-stone has been found, and through the labours of the same astronomer who set the example of the great sidereal survey. The parallax of the star 61 *Cygni* is a recognised element of modern astronomy, and its mass even has been estimated, though on data which still admit of dispute. Other great problems hinted at by Sir John Herschel are either in course of receiving their solutions, or have already received them; and no one now entertains any doubt that the lapse of two or three

centuries will, in sober earnest, produce all those grand results which to Sir John were then the objects of such faith and hope.

The Königsberg Zone-Observations, you are aware, gentlemen, consisted of two great portions, of which that completed first in the years 1821 to 1825 comprised all the stars down to about the tenth magnitude, lying within 15° of each side of the equator; and the other, executed in the years 1825 to 1833, took in the stars lying between the parallels of 15° and 45° of north declination. You are also aware that the equatoreal zone has been since catalogued with great labour by Professor Weisse, and that this catalogue of nearly 32,000 stars, with its admirable preface by Struve on the distribution of the stars in space, forms one of the most valuable additions to sidereal astronomy in the present century. The next great work of the same class was that of Argelander, who, under unfavourable instrumental circumstances, succeeded in completing the observation of stars lying in two other zones, the one extending from 45° to 80° of north declination, and the other, observed subsequently, from 15° to 31° south declination. Thus, with the exception of one zone, all the stars visible in Europe down to the tenth magnitude, or thereabouts, were provided for, those of Argelander's Northern Zone, to the number of more than 26,000 stars, having been recently, in a most praiseworthy way, reduced and catalogued by M. Oëltzen, assistant at the Vienna Observatory. The only zone of the northern heavens which remained unobserved was that lying within ten degrees of the pole; and this is the one which Mr. Carrington proposed to himself as the subject of his own labours, and for the successful termination of which the Council have awarded to him the Gold Medal of the Society.

This, gentlemen, is the state of the case with respect to the first part of our considerations,—namely, the utility of Mr. Carrington's work. For any general and systematic discussions or speculations respecting the stars in which astronomers may hereafter indulge, it is necessary that the whole of them, down to as low a magnitude as is consistent with the instrumental means ordinarily employed, be observed with moderate accuracy. This, I say, is necessary as the starting-point, or the lowest fundamental condition, if the aspirations of Sir John Herschel, before cited, are to be realised; and it is indispensable, too, that the whole work be accomplished within a moderate interval of time. The polar zone is as important as any of the others, but it is by far the most troublesome and difficult, and it requires far more resources, both instrumental and intellectual, than any of the other zones.

To any of our members who are well acquainted with our worthy Secretary, I need not say that the difficulty would be a recommendation; and to those who have not the advantage of his intimate acquaintance, the reading of his Introduction will *tell* the same fact quite as clearly. No one can read the intro-

duction to the Redhill Catalogue,—at least I could not do so myself—without gaining an idea of the positive zest with which he seems to grapple with difficulties and to conquer them one after another as they occur.

But that you may be better able to judge of this and other matters connected with the work, it is necessary that I state to you, briefly, a few points connected with its history and progress.

Mr. Carrington, after passing with distinction the examination for his degree at Cambridge, determined to devote himself to the cultivation of astronomy, and for this purpose accepted the situation of Observer at Durham, where he remained nearly three years. While thus engaged he formed the determination to complete the labours of Bessel and Argelander, by observing the polar zone omitted by them, and, finding that the instruments at his disposal were inadequate to the task, he resolved to resign his appointment and to provide himself with the means of carrying it out by means of his own private resources. The undertaking involved, as far as expense was concerned, the building of a house as well as of an observatory, the furnishing of the latter with an expensive transit-circle and equatoreal, as well as with the other instruments ordinarily needed as their accompaniments, and the payment of an assistant. To this must be added the purchase of the lease of the ground on which his residence stands, and the devotion of his own time, which, from the nature of his previous education, might easily have been so employed as to become a source of considerable income. So much with regard to the devotion of himself and his pecuniary resources to the cause of science,—a most munificent donation, and deserving of all the commendation in our power to bestow, if we were to look no farther.

His chief instruments were, first, a transit-circle made under his directions by Mr. Simms, similar to the Greenwich instrument, but smaller, having a telescope of 5 inches aperture, and $5\frac{1}{2}$ feet focal length; and, secondly, an equatoreal with a telescope of $4\frac{1}{2}$ inches aperture, and rather more than 4 feet focal length. The latter was employed chiefly for roughly observing the stars in the polar zone, and forming the preliminary or working catalogue.

The real work of observing the polar zone commenced in the year 1854 and occupied three years. Each star was observed at least three times, and the whole number which have been observed, thoroughly reduced and catalogued, amounts to 3735. This number includes nineteen stars within one degree of the pole, which, in the reduction, required special treatment, and which, on that account are placed by themselves, and denoted by the italic letters of the alphabet. Some time after the commencement of the observations, Mr. Carrington discovered that the polar zone had been observed by Professor Schwerd of

Speyer, in the years 1826 to 1828, the limiting parallel being different, and the number of stars observed within Mr. Carrington's limit being about 680. Schwerd's observations have been reduced and catalogued by M. Oëltzen, to whom astronomy is so much indebted for the cataloguing of the stars in Argelander's Northern Zone, and Mr. Carrington has brought up the places to his own epoch and compared them with his own results. This comparison of course greatly increased the labour, but gave occasion for a most elaborate and instructive dissertation on the whole theory of precession, nutation, and aberration, and especially on the application of it in the case of stars very near the pole. This dissertation is itself a remarkable production, though the author disclaims any novelty, and it will repay the attentive study of any astronomers who may be engaged in observations similar to those of Mr. Carrington.

At the risk of being tedious, I must say a few words respecting the processes employed in the observations and reductions. First, I will premise that every care was taken, previously to the commencement of the work, to test the quality and fitness of the instrument. The flexure of its telescope was examined and allowed for, and the rigidity of the cones forming its tube, and of those forming the axis, was tested by the application of weights. It was found difficult to provide for the examination of the divisions of the circle; and the accuracy of these, which were cut with Mr. Simms' dividing engine, as also of the cylindrical form of the pivots, has been assumed. The instrument is collimated by means of two collimating telescopes, one placed north and the other south of it, precisely as in the case of the Greenwich instrument; and the errors of level were observed by means of a Bohnenberger's eye-piece and a trough of mercury, precisely as at Greenwich. So steady was the instrument that the error of level can generally be pretty accurately represented by a formula containing the time from a fixed epoch, denoting permanent change, and a periodical term depending on the sun's longitude, that is, on the season of the year. The azimuthal errors were determined primarily and fundamentally by assumed right ascensions of the four fundamental polar stars of the *Nautical Almanac*, after severe scrutiny of their places as given in the Greenwich and Oxford Observations, and as found by several transits of *Polaris* above and below pole at Redhill, and immediately by the use of these stars, combined with sixteen others called 'Special Polars,' arranged in pairs for their transits above and below the pole.

In the treatment of the north polar distances, these were made fundamentally to depend upon the assumed north polar distances of the same four polar stars of the *Nautical Almanac*, the polar points thence derived being applied to all circle-readings.

I should only weary you, gentlemen, if I should attempt to

enter into detail with respect to those niceties of manipulation of the instrument. of the means taken for obtaining accurately the clock-errors, or of the ingenious processes employed for economising labour, which only the professional astronomer can appreciate. I have stated the facts concerning the most important features of the work, and I beg you to receive my assurance that everything has been done which honesty of purpose, unwearied industry, and consummate skill, could effect to render it—what I firmly believe it to be—an honour to the country which has produced it.

In whatever light we regard it, whether as a gift to science, or simply as the production of a skilful and laborious astronomer, —whether we regard the laborious fidelity of the details, or the symmetry, completeness, and finish of the accomplished work, it is equally admirable.

Like the painting of some great master, it must be judged by all the rules of art to ensure a correct appreciation of its merits; and, with the additional consideration of the imperishable value of its results, we may indeed say of it, in the language of the Roman poet,—

> "Exegit monumentum ære perennius,
> Regalique situ Pyramidum altius;
> Quod non imber edax, non Aquilo impotens
> Possit diruere, aut innumerabilis
> Annorum series, et fuga temporum."

(*The Rev. R. Main then, delivering the Medal to Mr. Carrington, addressed him in the following terms*):—

Mr. Carrington,—In the name of the Council and Fellows of the Astronomical Society I present you with this Medal as the best tribute which they have it in their power to pay to your industry and talents, displayed in the formation of the *Redhill Catalogue of Stars*, and as the best means at their disposal of expressing their sense of the splendid present which you have in it made to science. They sympathise with you in the domestic bereavement which has for a time had the effect of turning aside your energies into another channel, but they trust that this will be only for a time, and that many years may remain to you in which your devotion to science will enable you to achieve still greater things. Be assured that the best wishes of the Society will always attend you, both for your happiness and enjoyment of the honours you have already earned, and for your successful accomplishment of all that your zeal may in future induce you to undertake.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Rev. Robert Main, M.A.

Vice-Presidents:

G. B. Airy, Esq. M.A. F.R.S. Astronomer Royal.
John Russell Hind, Esq. Superintendent of the Nautical Almanac.
John Lee, Esq. LL.D. F.R.S.
Admiral W. H. Smyth, K.S.F. D.C.L. F.R.S.

Treasurer:

Samuel Charles Whitbread, Esq., F.R.S.

Secretaries:

Richard C. Carrington, Esq.
Warren De La Rue, Esq. F.R.S.

Foreign Secretary:

Admiral R. H. Manners.

Council:

Arthur Cayley, Esq. F.R.S.
Captain Alexander Ross Clarke, R.E.
Augustus De Morgan, Esq.
Rev. George Fisher, M.A. F.R.S.
Charles Frodsham, Esq.
James Glaisher, Esq. F.R.S.
Robert Grant, Esq. M.A.
Richard Hodgson, Esq.
Manuel J. Johnson, Esq., M.A. F.R.S. Radcliffe Observer.
William Lassell, Esq. F.R.S.
Rev. Baden Powell, M.A. F.R.S.
Rev. C. Pritchard, F.R.S.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *March* 11, 1859. No. 5.

Rev. R. MAIN, President, in the Chair.

John James Mellor, Esq., Rochdale, Lancashire;
Charles Edward Smith, Esq., Fir Vale, near Sheffield;
Rev. Frederick Burn Harvey, Great Berkhampstead; and
James Clarke, Esq., 14 Park Road, Dalston,

were balloted for and duly elected Fellows of the Society.

The late Director of the Radcliffe Observatory.

The President, in rising to announce that the Astronomer Royal was prepared to offer to the meeting an oral explanation of his elaborate paper on the Movement of the Solar System in Space, begged the indulgence of the meeting while he spoke a few words on a subject which he had no doubt was very near the hearts of all,—namely, the lamented and sudden death of their revered friend and member, Mr. Johnson, the Radcliffe Observer at Oxford. He endeavoured in few words to point out those rare qualities of head and heart which rendered their departed friend so universally beloved and respected by all classes of men. To a keen and sagacious intellect sharpened by an originally good education, was added a power and a love of work, and a singular honesty of purpose in performing the duties of his office, which had raised himself, together with his observatory, to a more than European reputation; but to this was also added a kindliness of nature and a generosity of disposition which attracted the love as much as the respect of all who were in any way connected with him. He, the President, had been happy in securing the friendship of Mr. Johnson, and it was to him now a melancholy pleasure to be able to recall to memory occasional visits to the Radcliffe Observatory, where he had had the opportunity of seeing him in all the relations of life, in the bosom of his family, in his social intercourse with his friends, in his connexion with the university, as well as in his character

of an eminent astronomer. It is pleasant now to remember the respect and affection which was exhibited towards him. His love of art as well as of science, his knowledge of languages and extensive reading in many branches of literature, the unaffected and good-humoured readiness with which he could assimilate himself to the tastes of his various visitors and friends, these are qualities which could only be properly known and appreciated by visiting him in his own social and domestic circle, and as such it was pleasant now to dwell upon them.

Before sitting down the President drew attention to the loss of distinguished and eminently useful members that the Society had experienced within a comparatively few years. Beginning with Francis Baily, who had well been called the father of the Society, it had had at short intervals to deplore the loss by death of such men as Galloway, Stratford, Sheepshanks, Raper, and others of almost equal value. In addition to these there were others who, as time went on, were obliged from necessary considerations of health and other circumstances to assist less frequently at our Councils, though they were still ready to lend the Society their best help and support on all emergencies. Yet the Society had good cause to be thankful that these losses, great as they are, have been in a great degree compensated by the zeal and activity of the younger members, who, according to the natural law, spring up to supply the places of those withdrawn from it. He had no fear for the Society while its members continued to be actuated by the same love for science and the same interest in its welfare which had thus far been exhibited. Let each in his own degree add his mite; let him do, however little, what is in his power to advance astronomical science, and to confirm and extend the operations of the Society; and there would be no fear that the future would not be as honourable as the past, and that the Society would not continue to retain that respect and consideration which it had already won for itself.

The Astronomer Royal, before commencing what he had to say on the subject of his paper, referred to the remarks of the President on the respect and love universally paid to their regretted member, Mr. Johnson, and recalled to the recollection of the members, how this was illustrated during a time of discord in the Society, arising from the disputed claims of Adams and Le Verrier to the medal for the discovery of the planet *Neptune*. In this contest Mr. Johnson advocated the claims of Le Verrier with all the impetuous generosity which characterised him, and supported his opinions with considerable heat, and yet it was remarkable that every member went away with a greater respect and regard for him, even though opposed to his opinions, than they entertained before.

On the Silvering of Glass Specula. By Warren De La Rue, Esq. Ph. D. F.R.S., and Hugo Müller, Esq., Ph. D. F.C.S.

Our attention having been directed to the silvering of glass specula, we have made experiments with a variety of processes published and unpublished, but we have found that, on the whole, none ensure so good a result as that described by Baron Liebig in the *Annalen der Chemie und Pharmacie*, xcviii. p. 132. Among the processes submitted to trial and found to give good mirrors, we may mention the reduction of an alkaline solution of silver by means of tartaric acid, saccharic acid, glyceric acid, grape sugar, oil of cloves, and the aldehydes; but generally it is very difficult with these reducing agents to obtain the silver surface in a proper condition for polishing, although with due care this may be accomplished.

Liebig's published process is as follows:—10 grammes of fused nitrate of silver are dissolved in 200 cubic centimètres of water, and just enough liquid ammonia is added to this solution to redissolve the brown precipitate first formed. This solution is mixed with 450 cubic centimètres of a solution of caustic soda, specific gravity 1·035. A purplish black precipitate forms on the admixture of these solutions, which is to be dissolved again by the careful addition of ammonia. After which 800 cubic centimètres of water are added and so much of a dilute solution of nitrate of silver as to occasion a permanent slight precipitate of oxide of silver; 50 cubic centimètres of water finally are added, and the solution filtered. This is then the normal alkaline solution of silver which is retained for use when required.

Just previous to the silvering operation eight parts of this alkaline silver solution are mixed with one part of a solution of sugar of milk, containing one part of sugar of milk in ten parts by weight of water.

In order to prepare the surface to be silvered so as to insure the adhesion of the silver film, it is rubbed over with a solution of cyanide of potassium by means of a cotton wool; after the surface is thoroughly cleansed in this matter, it is washed with distilled water, and, just previous to its immersion in the silvering liquid, it is wetted with strong spirit of wine.

All things being ready, the silvering solution is poured into a vessel sufficiently deep to contain the speculum in a vertical direction; but instead of placing the mirror exactly vertical, it is allowed to lean slightly over to one side, the surface to be silvered being placed downwards.

In a few seconds the solution darkens, becomes brown, and deposits the silver on the glass in the first instance as a black or purple transparent mirror; this gradually brightens and becomes more and more metallic, and finally, in about three quarters of an hour, a film is obtained at ordinary temperatures which is extremely thin and regular in thickness; this mirror

has a slight bronzy hue by reflected light, and if of the proper thickness, transmits a deep blue light when the sun is viewed through it. It readily, after drying, receives a polish when rubbed with a piece of chamois leather and dry rouge (peroxide of iron): it is much harder than ordinary silver, and bears the same relation to it as electro-precipitate, copper, iron, &c. bear to those metals prepared by the ordinary metallurgic methods.

Results of Observations of Small Planets, made with the Transit Circle at the Royal Observatory, Greenwich, during the months of January and February, 1859.

(*Communicated by the Astronomer Royal.*)

Astræa (5).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Feb. 18	14 16 6·5	12 10 7·72	86 42 53·25
25	13 45 21·8	12 6 53·91	85 51 42·27

Hygeia (10).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Jan. 22	9 52 20·1	5 59 10·98	65 49 1·40
Feb. 7	8 42 35·6	5 52 20·00	66 4 15·68

Victoria (12).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Jan. 7	11 49 6·9	6 57 8·59	77 3 31·42
22	10 35 2·4	6 42 0·35	76 52 21·06

Jan. 22, Obs. of N.P.D. — The zenith distance pointer has been increased 1°.

Irene (14).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Feb. 22	14 52 26·3	13 2 19·73	80 5 9·67
23	14 48 21·5	13 2 10·84	79 58 39·22
24	14 44 14·5	13 1 59·66	79 52 6·30
25	14 40 6·2	13 1 47·26	79 45 25·22

Thetis (17).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Feb. 7	8 54 9·7	6 3 55·97	69 4 37·95

Calliope ⑧.

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1859, Feb. 22	14 34 38·3	12 44 28·86	74 31 44·49
23	14 30 12·1	12 43 58·43	74 25 49·19
24	14 25 44·5	12 43 26·68	74 19 55·00
24	14 21 16·2	12 42 54·21	74 14 2·53

Harmonia ㊵.

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1859, Jan. 22	13 51 10·0	9 58 40·10	72 30 25·05
Feb. 22	11 19 7·3	9 28 25·68	69 8 59·42
23	11 14 14·2	9 27 28·39	69 4 5·90
24	11 9 22·6	9 26 31·87	68 59 26·25

Jan. 22.—This object is not quite so certain to be the planet as the others.

Isis ㊷.

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1859, Feb. 23	12 43 57·6	10 57 26·45	70 0 44·45

Pales ㊾.

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1859, Jan. 28	11 17 50·9	7 48 35·19	69 40 3·64
31	11 3 34·0	7 46 5·60	69 36 22·46
Feb. 23	9 20 1·2	7 32 56·64	69 21 20·43

Extracts from a Letter to Mr. Carrington, from Dr. C. H. F. Peters, of Hamilton College, U. S.

". . . I write to-day only in haste, that I may not delay giving you the information desired. I am going to copy and send you soon the whole set of computed positions of the solar spots observed by me. . . . My heliographic longitudes *are corrected in a direction opposite to the direction of rotation;* so that, looking from the sun's centre in the direction, for example, of the first point of *Aries*, greater and greater longitudes come into that direction. Your beautiful remark on the longitude-drift of spots being opposite in higher and lower latitudes is therefore completely confirmed also in sign by my observations (at least as far as I see from the Table B which you mention). This remark seems to me of no little importance, disclosing a fact which (as a cause or as a consequence ?) may have something to do with the two belts of spots situated

at or near the border of the opposite currents. . . . In latitudinal sense, however, I cannot make agree my observations with what yours seem to indicate. Besides the latitudes of spots exhibited in my Table A (and these are *all* the spots observed during a whole, or rather, more than a half, revolution), which indicate, as it seems, an unexceptional drift towards the equator, I have just examined in this respect the individual positions of the totality of spots before me; and though, with more leisure, the single isolated spots ought to be discriminated from the participants of systems, I write, in the annexed paper, an abstract presenting a general survey of the motions in latitude. In drawing inferences from individual positions, a good allowance must be made for the mean error of the observations. But, on the whole, I do not recognise in the annexed conspectus a regular law depending on the distance of the spots from the equator. . . . Distracted in the latter years by other business, I have not done anything more upon solar spots. Your researches, however, as published from time to time, have kept up with me a lively interest for this subject; and I think I may take it up again, when the measuring apparatus for my refractor is in order; though, to be sure, the fine climate of Italy offers a much greater reward for this kind of work."

Note.—I would remark that, in my paper printed in the *Monthly Notices* for January, when I spoke of a reverse current in the higher latitudes, I did not affirm the existence of advancing and retrograde motions in longitude absolutely, but as exhibited by the use of a mean period of rotation. I have not yet concluded that the drift of all spots is not in the direction of the general rotation, the drift being, in that case, most rapid in the region of the equator of the sun; and with respect to Dr. Peters's result of the drift in latitude being general towards the equator, I may here remark that M. Laugier came to the opposite conclusion, and that my own examination would seem to explain the possible origin of the difference between two observers so capable and so exact; for it may well be that the critical latitudes at which the directions of drift in latitude change sign may be different at different epochs. The question is rather a minute one, when the possible errors of observation are considered, and at present requires the expenditure of more labour and investigation.—[R. C. C.]

Hofrath Schwabe has recently communicated to the *Astron. Nach.* his customary abstract of his enumerations of spots for the previous year. From observations on 335 days in the year 1858, he recorded the passage of 188 groups of spots, and on no day found the disk free of spots. The increase of action over that of 1857 is very decided, and such as was anticipated. The next maximum may be predicted for the year 1860.

On the Movement of the Solar System in Space.

By G. B. Airy, Esq., Astronomer Royal.

The object of the author in this memoir is to develope a new method for the determination of the direction and amount of the movement of the solar system in space, from a conviction of the inadequacy and defects of the methods hitherto in use. The first of these methods is that of Sir William Herschel, proposed by him when the number of known proper motions was very small. It consisted mainly in drawing upon a celestial globe a great circle representing the direction of proper motion of each star, and finding the point at which any two such circles intersect each other, which point, supposing the apparent proper motion of each star to arise entirely from a real motion of the sun, would be that to or from which the sun's motion is directed. This method was most happily adapted to the limited facts of observation then known, and the whole investigation considered one of the best specimens of Sir W. Herschel's enterprise and sagacity; but it is evidently insufficient for the discussion of proper motions of a great number of stars.

The second method, which is applicable to the discussion of the proper motions of larger catalogues of stars, has been in use up to the present time, and is of the following nature. A point is assumed as a probable approximation to the apex of solar motion; the direction of the arc of great circle drawn from the star's place to this point is ascertained as also the direction of the star's apparent proper motion; and the angle between these two directions multiplied by a certain factor is considered as the small residual errors in ordinary problems of combinations of observations; and all the equations thus formed are to be treated by the method of least squares.

In the author's opinion there are many objections to this method, of which the following are the most important:— First, that it requires the point to be approximately known, since an error of its assumed position equal in amount to the smallest which could be probably assigned to Sir W. Herschel's determination causes great confusion in the change of direction of the great circle drawn from the star's place to the assumed point of direction of solar motion, when the star is not very distant from that point; and, secondly, that the proper motions of some stars are so nearly the opposite to those implied in the assumption of the point, that a small change in the place of the point will change the angle of errors *per saltum* from $+179^\circ$ to -179°.

On such grounds as these the author considers the method employed very imperfect, and proposes one totally different, of which the following sketch will give a tolerably correct idea:—

Instead of using the apparent angular motions of the sun and stars, as exhibited on the surface of a globe, their linear

movements are treated of by reference to rectangular co-ordinates, and the following advantages result from the adoption of this method. It is quite independent and complete, and does not require the assumption of a point determined by preceding investigations; and it gives exactly due weight to each observation subject to our antecedent judgment as to the general weight-multiplier to be attached to any class of stars defined by brilliancy or any other characteristic, with the exception of magnitude of proper motion.

In applying the method, the author takes for the origin of co-ordinates the centre of the sun at a fixed epoch, and for the plane of xy, the plane passing through the sun parallel to the earth's equator, the axis of x being drawn towards the first point of *Aries*, and that of y to the point whose right ascension is 90°; z is drawn parallel to the earth's axis and is positive towards the north.

Taking the sun's linear motion in a year or other definite time, and resolving it in the direction of x, y, z, it is not difficult to express the R.A. and N.P.D. of a star at the end of the year in terms of the initial values of R.A. and N.P.D., and of the sun's resolved motion in x, y, and z, multiplied into certain functions of R.A. and N.P.D. easily calculated. In fact, for a star at distance r, the expressions for its apparent R.A. and N.P.D., at the end of the year, will be respectively,

$$\text{Initial R.A.} - \frac{\sin \text{R.A.}}{r \cdot \sin \text{N.P.D.}} X + \frac{\cos \text{R.A.}}{r \cdot \sin \text{N.P.D.}} Y,$$

and

$$\text{Initial N.P.D.} + \frac{\cos \text{R.A.} \cos \text{N.P.D.}}{r} X + \frac{\sin \text{R.A.} \cos \text{N.P.D.}}{r} Y - \frac{\sin \text{N.P.D.}}{r} Z$$

where $-X$, $-Y$, and $-Z$, are the resolved parts of the yearly motion of the sun.

From which the change of star's apparent place produced by sun's real motion in direction of a parallel to the equator will be

$$-\frac{\sin \text{R.A.}}{r} X + \frac{\cos \text{R.A.}}{r} Y$$

and the change of apparent N.P.D. will be

$$\frac{\cos \text{R.A.} \cos \text{N.P.D.}}{r} X + \frac{\sin \text{R.A.} \cos \text{N.P.D.}}{r} Y - \frac{\sin \text{N.P.D.}}{r} Z.$$

The author then proceeds to show that where a number of equations given by different stars, the elements of linear motion of the different stars must be considered absolutely as chance-quantities to be treated in the same way as chance-*errors of* observation, subject, however, to one consideration

(not treated of in this memoir) as to views which may be entertained respecting the probable inequalities of motion of the stars forming our nebular cluster, as measured in various directions. To this consideration he calls the attention of future inquirers, but as he has no means of forming a judgment on the nature of the motions of individual stars, he supposes that for every star the probabilities of movements of given magnitude in all directions are equal.

He next enters on the consideration of the effect of systematic errors which may have crept into the computations by which the proper motions have been determined. These proper motions are taken from Mr. Main's paper printed in vol. xix. of the *Memoirs*, and from another paper recently printed, and were deduced by comparison of the places of Bradley's stars as given in Bessel's *Fundamenta*, with the places given in the Greenwich Twelve-year and subsequent Six-year Catalogue, and therefore will be affected by any errors in Bradley's observations or Bessel's reductions. In addition, they will be affected by incorrect values of the constants of precession used in reducing the stars from Bradley's epoch to the modern Greenwich epochs. On the whole, they may consist, for right ascensions, in the error of equinox used in reducing Bradley's observations, together with errors of the constants of precession, and from north polar distances, in errors of the constants of precession, together with those arising from the defects of the Greenwich quadrants. To these systematic errors must be added the chance-errors of observation of each individual star not connected in any way whatever.

By comparison of the aggregates of the various terms before specified with the tabulated results of observations, equations of the following forms arise:—

$$-\frac{\sin \text{R.A.}}{r}X + \frac{\cos \text{R.A.}}{r}Y + m' \sin \text{N.P.D.} + n' \sin \text{R.A.} \cos \text{N.P.D.}$$

$$+ \frac{M\xi}{r} + \text{Error in parallel} = \text{Proper Motion in parallel.}$$

and

$$\frac{\cos \text{R.A.} \cos \text{N.P.D.}}{r}X + \frac{\sin \text{R.A.} \cos \text{N.P.D.}}{r}Y - \frac{\sin \text{N.P.D.}}{r}Z + q -$$

$$n' \cos \text{R.A.} + \frac{M\eta}{r} + \text{Error in N.P.D.} = \text{Proper Motion in N.P.D.}$$

where $\frac{M\xi}{r}$ and $\frac{M\eta}{r}$ are the real apparent angular motions in the directions of parallel and meridian, m' and n' are the possible errors of m and n, the constants that enter into the formula of precession in R.A. and N.P.D., and q is the possible error of

Bradley's observations in N.P.D., arising chiefly from the defects of the Greenwich quadrants.

Equations may now be formed on two extreme suppositions; first, that the irregularities of Proper Motion are entirely due to chance-error of observation; secondly, that they are entirely due to peculiar motions of the stars. The author is inclined to think that the second supposition corresponds most nearly with the truth, especially when the stars to which the calculation is applied are taken from a catalogue of large proper motions; he, however, proceeds to show how the final equations for determination of X, Y, and Z, must be found, according to the rules of the Theory of Probabilities, on both these extreme suppositions.

For the values of r, Mr. Airy has been guided by the assumptions of Struve (*Introd. in Cat. Nov. Stell. Dup.*), the stars being divided into seven classes from the 1st magnitude to the 8th.

As it would have been a work of enormous labour to make use of all the stars in Mr. Main's catalogues, amounting to about 1200, those only have been used which have proper motions above a certain limit assigned by the author of the catalogues, but without any distinct motives of preference.

The author here makes a remark which is of great importance in the determination of the solar motion, and which it is best to express without abridgement in his own words:

"It appears to me that the propriety or impropriety of retaining the stars whose proper motions are extremely large will depend entirely upon our assumption as to the practical reference for zero of space. In void space, there can be no zero either of linear ordinate or of angle. Any person who endeavours to form a notion of such zero will find that his mind sinks under the effort. We are compelled therefore to adopt, as our zero of space, the place of one body, or the mean of the places of many bodies. And, to whatever extent we adopt a number of bodies in forming our mean, to that extent our deductions of all kinds will apply. Thus, if we include in our treatment the stars with very large proper motions (61 *Cygni*, μ *Cassiopeiæ*, &c.), we may expect to obtain the movement of the solar system, as referred to the mean of all the stars of which we treat, including 61 *Cygni*, μ *Cassiopeiæ*, &c.; if we exclude them, we obtain the movement as referred to the mean of all the stars except those. The retention or exclusion of those stars appears then to be quite arbitrary, provided that we give the proper interpretation to our final results. In the investigations of this paper, I retain these stars, so far as they are included in Mr. Main's catalogue."

The author then proceeds to the solution of the equations on the two suppositions before-mentioned, and obtains the following results; R being the sun's whole proper motion expressed

in seconds of arc as seen from a star of the first magnitude, and A and D the right-ascension and north declination respectively of the point to which that motion is directed.

First supposition.

$$R = 1''.269 + 0''.245 \times n' - 1''.690 \times q$$
$$A = 256^\circ\,54' - 1'.393 \times n'$$
$$= 256^\circ\,54' - 79^\circ\,82' \times n'$$
$$D = 39^\circ\,29' - 0'.159 \times n' - 1'.616 \times q$$
$$= 39^\circ\,29' - 9^\circ\,12' \times n' + 92^\circ.61 \times q$$

Second supposition.

$$R = 1''.912 + 0''.472 \times n' - 2''.190 \times q$$
$$A = 261^\circ\,29' - 1'.998 \times n'$$
$$= 261^\circ\,29' - 114^\circ\,47' \times n'$$
$$D = 24^\circ\,44' - 0'.114 \times n' - 2'.486 \times q$$
$$= 24^\circ\,44' - 6^\circ\,51' \times n' - 142^\circ.43 \times q$$

The author considers the results of the second assumption to be more worthy of confidence than those of the first, and proceeds to give the numerical evidence of the probability that the motion of the solar system is not very different from that given by it, by forming the sums of the squares of the observed proper motions in parallel and in N.P.D., and then forming the sums of the squares of the residual errors of the proper motions after the application of the terms depending on the solar motion.

The results according to the second supposition are,

Sum of squares of Motion in Parallel	Uncorrected	54·4450
	Corrected	48·4217
Sum of squares of Motion in N.P.D.	Uncorrected	48·1206
	Corrected	45·3129

"From these the reader may form an opinion on the legitimacy of the general conclusions. I do not believe that the fundamental suppositions are sufficiently certain to justify any investigation as to the probable range of error in the assigned position of the line of the sun's motion.

"The most remarkable feature in the result is, however, the very great difference between the velocity of the solar motion as found from this investigation, namely, 1″·912, and that found by M. Otto Struve in his *Bestimmung der Constante der Præcession*, namely, 0″·339." This Mr. Airy attributes to the fact that the stars used in his own investigations are exclusively those having large proper motion, and enters largely into the explanation which depends, he considers, on the choice made in

the apparent evidences of proximity of the stars, that is between brightness and magnitude of proper motion.

To the memoir is attached an appendix containing the elements of every sort used in the calculations, including the values of the functions of R.A. and N.P.D. which are necessary in the formation of the equations of condition.

Note on the Development of the Disturbing Function in the Lunar Theory. By Sir J. W. Lubbock, Bart.

Mr. Cayley, in a paper recently published, has pointed out some errors in the development of R given by me in my work on the Lunar Theory, and amongst others in the numerical coefficient of the term of which the argument is $2\tau + \xi - 3\xi'$, which I called $+\frac{45}{64}$, but which should be $-\frac{845}{64}$

$$\frac{dR}{de} = \frac{r dR}{dr}\frac{dr}{r de} + \frac{dR}{d\lambda}\frac{d\lambda}{de}$$

λ being the true longitude.

If we consider the term of which the argument is $2\tau + \xi$, in order to have the part multiplied by e, it is sufficient to take,

$$\frac{r dR}{dr} = 2R_1 \cos 2\tau \qquad \frac{dR}{d\lambda} = -2R_1 \sin 2\tau$$

$$\frac{dr}{r de} = -\cos\xi \qquad \frac{d\lambda}{de} = 2\sin\xi \quad \text{see p. 26 Lunar Theory, Part 1.}$$

$$\frac{dR}{de} = 2R_1 \cos 2\tau \times -\cos\xi - 2R_1 \sin 2\tau \times 2\sin\xi$$

$$= R_1\{-\cos(2\tau+\xi) - \cos(2\tau-\xi) + 2\cos(2\tau+\xi) - 2\cos(2\tau-\xi)\}$$

$$= R_1\cos(2\tau+\xi) - 3R_1\cos(2\tau-\xi).$$

When the term only involves the first power of e, as in this instance, $\int \frac{dR}{de}\,de = R$, or the coefficient of the term in R is the same as that of the corresponding term in $\frac{dR}{de}$.

The preceding result is entirely independent of ξ', and, moreover, would have been the same whatever multiple of ξ' had accompanied 2τ, so that as

the coefficient of $\cos(2\tau+\xi)$	=	the coefficient of $\cos 2\tau$	
,, $\cos(2\tau+\xi-\xi')$	=	,, $\cos(2\tau-\xi')$	$= -\frac{21}{8}$
,, $\cos(2\tau+\xi-2\xi')$	=	,, $\cos(2\tau-2\xi')$	$= -\frac{51}{8}$
,, $\cos(2\tau+\xi-3\xi')$	=	,, $\cos(2\tau-3\xi')$	$= -\frac{845}{64}$

so also the coefficient of $\cos(2\tau+\xi+\xi') =$ coefficient of $\cos(2\tau+\xi') = \frac{3}{8}$

" $\cos(2\tau+\xi+n\xi') =$ " $\cos(2\tau+n\xi')$

whatever n may be.

Many other similar equations of condition obtain. When I published the development in question I was not aware of the extent to which this means of verification may be carried in the lunar theory, but, as owing to Mr. Cayley's recent work, no uncertainty can exist with regard to the numerical order of any coefficient in this stage of the approximation, it is unnecessary to give any further examples.

March 14, 1859.

Note on a Group of Solar Spots observed on the 23d of February, 1859. By W. R. Birt, Esq.

Mr. Birt's observations were made with Mr. Slater's refractor of 15 inches aperture, which was liberally placed at his disposal for the purpose. They were accompanied with a sketch exhibiting the details of the phenomena seen by him.

The nucleus of the principal spot presented a *striated* appearance, the direction of the *striæ* being that of the longest diameter of the spot. The illumination of the cloudy stratum was of a varied character, conformably to Mr. Dawes' observations (*Monthly Notices*, vol. xii. p. 168). It also exhibited unmistakeable traces of a striated formation, which seemed to be connected with the jagged or uneven edge of the nucleus. In this respect the observations of the author do not accord with those of Mr. Dawes.

The author thus proceeds :—

"The most interesting feature of the nucleus consisted in two luminous patches presenting nearly if not quite the same intensity of light as the surrounding penumbra. These patches during the time of observation exhibited a somewhat fluctuating character, not so much in respect of luminosity as of position. They were confined to the upper part of the nucleus, but underwent changes of configuration.

"*The Penumbra* manifested the striated or ridged appearance spoken of by Mr. Dawes in a most characteristic manner. I am strongly disposed to regard the penumbra of this particular spot as *radiated*. Mr. Dawes' sketch before alluded to exhibits very distinctly the kind of radiation which I noticed in the spot now under consideration; the edges of the penumbra were notched, jagged, and uneven, and the outline was not coincident with the outline of the nucleus. I particularly remarked that the luminosity of the penumbra was nearly uniform, and broken only by a bright spot near the horns, forming the upper

angle of the nucleus, and by a brighter space between the nucleus and largest detached spot within the penumbra: the latter, however, might have resulted from contrast."

The observations were made with a magnifying power of 220.

On some Indications of Rotation in a Solar Spot.

By W. R. Birt, Esq.

Finding that a somewhat large spot had entered on the visible hemisphere of the sun on February 22, I availed myself of Mr. Slater's kindness, and obtained two views, one shortly before its passage of the centre, and one exactly three days after the first. The two drawings accompanying this communication indicate the nature and extent of the changes that occurred during the three days.* Power employed, 220; aperture, 15 inches, reduced to 4 inches for subduing the light.

		h m h m
Sketch No. 1	February 28	2 30 to 4 0 G.M.T.
Sketch No. 2	March 3	2 45 to 3 45 G.M.T.

The two sketches are strictly comparable, having been executed about the same time of the day.

Nuclei.—The nuclei of the two principal spots in the group presented a more or less uniform appearance. I did not remark the peculiar *striated* appearance so distinctly apparent in the spot of the 23d of February, an account of which I communicated to the Society. It will be seen from the drawings that the nuclei of both spots underwent external configuration of form.

Penumbra.—The radiated appearance of the penumbra, as seen in the spot of February 23, was not detected in the penumbra of the two principal spots of this group. In Sketch No. 1, Feb. 28, three well-marked luminous patches were discernible near the left-hand upper edge of the penumbra (as seen in the telescope); also an intense luminous space between the principal nucleus and the smaller one in the largest spot.

On taking the second sketch, the penumbra around the two nuclei of the large spot appeared to be quite uniform in its luminosity; the smaller nucleus had increased considerably in size, and the intense luminous space between it and the principal nucleus had increased in brilliancy. It appeared to me that between the two nuclei the penumbra, properly so called, was absent; or this increase of brilliancy might have resulted from contrast.

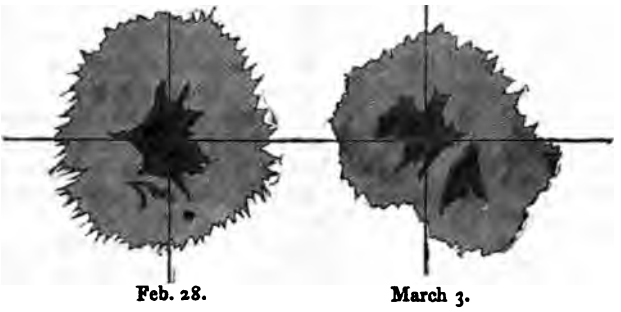
The penumbra of the smaller spot underwent, in the three days, a marked change of form; the direction of the longer

* These drawings were exhibited at the meeting of the Society.

diameter, on the 3d March, was nearly at right angles to its direction on the 28th February. The change of form of the nucleus during the same interval is considerable; it is, however, possible, by means of the angular points, to determine somewhat of the nature of the change of shape; and if we take the principal projecting point to the left, in Sketch No. 1, to be the same as the principal projecting point on the upper part of the nucleus in Sketch No. 2, then we have a somewhat similar disposition of the penumbra in both sketches, from which it would result that during the interval this spot had shifted its position about a quarter of a rotation.

The smaller Spots.—Numerous changes appear to have taken place among the smaller spots in the neighbourhood. Most of the spots below the large one, on Sketch No. 1, had disappeared on March 3. A small triangle only was visible, and it is somewhat difficult to ascertain if the spots forming it, and seen on March 3, were any of the smaller spots seen on February 28. While these spots had mostly disappeared, a somewhat considerable chain of small spots had formed below the smallest of the principal spots, and in the line of prolongation of the two principal spots. The nearly rectilinear division of the penumbra of the smallest of these is interesting; the same kind of arrangement was detected on Feb. 28.

Indications of Rotation of the largest Spot.—In order to exhibit in the clearest light the indications, afforded by the



Feb. 28. March 3.

two sketches, of rotation in the largest spot, I have placed views of them in juxtaposition. The change in the direction of the longer diameter of the penumbra, the alteration of place of the principal angular point of the larger nucleus, the new locality of the secondary nucleus, and other interesting features, bear testimony to a change of position of the spot itself apparently of the nature of rotation. This mode, however, of dealing with the subject may not be quite satisfactory. The spot itself has advanced considerably on the sun's disk in the interval between the sketches. It will therefore be important to compare the positions of the principal points of the largest spot with those of the spots surrounding it. A glance at the two sketches will

at once show that the longest diameter of the largest spot occupied two different directions, on the two days, with regard to the smallest of the two principal spots; in point of fact, that the largest spot had so moved in the interim as to bring its longest diameter into a line with the smallest spot, this having apparently changed its position by an independent rotation of its own.

The variations among the smaller spots appear to have been so rapid as to preclude the detection of any rotation among them.

March 4, 1859.

Note on Saturn's Ring. By the Rev. W. R. Dawes.

In the *Monthly Notices* for January last are some remarks by Captain Noble on the appearance of *Saturn's* obscure ring as seen with his excellent 4·2-inch refractor. He describes it as seeming to *dip* under ring B; "having never been able satisfactorily to trace the junction of those portions of it in the ansæ with the part crossing the ball." This seems obviously to arise from the want of more illuminating power than an object-glass of only four and a quarter inches aperture can afford, however perfect its defining power may be. I have on several fine nights during the present apparition of the planet been able, with the $7\frac{1}{4}$-inch refractor I now have in use, to trace the perfect elliptic outline of the inner edge of the ring C in the ansæ and across the ball, without the slightest break or irregularity: but with a telescope of considerably less illuminating power (only as 1 to 3), the narrow portion of it which is inclosed, as it were, between the ball and the ring B might very probably be overpowered by the incomparably brighter objects in contact with it on each side.

With reference to the curvature of the shadow of the ring on the ball, it may be remarked that when the altitude of the sun above the plane of the ring considerably exceeds that of the earth, the shadow of the ring on the ball is curved in the opposite direction to the edge of the ring, and especially near the edges of the ball. This is most remarkable when the minor axis of the outer ring is not much less than the polar diameter of the ball. The edge of the shadow then falling near the pole the visible curvature is very considerable; but I have never seen it greater on one side of the ball than on the other. The circumstances existing during the earlier part of the present apparition were particularly favourable for the display of this phenomenon, the sun's elevation exceeding that of the earth by considerably more than two degrees, and the edge of the outer ring being only about three seconds from the northern edge of

the ball. In both these respects the aspect was far less favourable at the epoch of Captain Jacob's observation in November 1852, from which his excellent picture is drawn. Captain Noble does not give the *date* of his own observation.*

Haddenham, Thame, March 1859.

Observations of Donati's Comet taken at Mussooree in India (Lat. 30° 17′ 19″ N., Long. 5^h 12^m 17^s·7 E.) By Capt. Tennant, R.E.

(*Extract of a Letter to Professor Piazzi Smyth.*)

Date. 1858.		Comet — Star. Diff. R.A.	Diff. N.P.D.	
	h m s	m s	′ ″	
Oct. 5	19 15 35·6	− 3 58·96	−23 31·3	(*a*)
7	20 1 3·6	− 3 51·45	− 6 4·2	(*b*)
8	19 37 56·1	+ 4 20·74	− 4 19·8	(*c*)
9	19 54 9·0	+ 2 0·93	+22 9·9	(*d*)
10	19 51 15·1	+ 5 13·18	+ 2 23·0	(*e*)
12	19 47 46·2	−11 3·78	−16 26·33	(*f*)

Stars of Comparison.

		App. R.A.	App. N.P.D.	Authority.
		h m s	° ′ ″	
(*a*)	α Boötis	...	...	Nautical Almanac.
(*b*)	ζ ...	...	...	Greenwich (6-year C.)
(*c*)	✱ (6 m.)	14 38 4	78 49 0	
(*d*)	✱ (7 m.)	14 51 33	81 39 0	
(*e*)	✱ (7 m.)	15 1 58	85 34 30	
(*f*)	μ Serpent.	...	...	Greenwich (6-year C.)

Deduced Places of Comet.

		R.A.	N.P.D.
	h m s	h m s	° ′ ″
Oct. 5	19 15 33·6	14 5 13·48	69 41 15·9
7	21 1 3·6	14 30 23·59	75 33 55·8
12	19 47 46·2	15 31 11·50	92 43 16·8

From the three places above determined Captain Tennant computed the following parabolic elements, employing the method given by Sir John Herschel in his article on Astronomy in the *Encyclopædia Metropolitana*:—

Perihelion Passage, Sept. 28	h m 16 16·5	G.M.T.
— Distance	0·575236	
Longitude of Perihelion	° ′ ″ 16 36 4	M. Equinox 1858·0
— Ascending Node	168 25 11	
Inclination	66 20 35	

Motion retrograde.

* The date of the drawing which accompanied Capt. Noble's paper is 1858, Nov. 19, 13^h 30^m G.M.T.—Ed.

On the 10th of October the tail of the comet extended to a point in the line between β *Herculis* and η *Coronæ;* distance from β one-third of distance of η *Coronæ*.

Note from the Astronomer Royal to the Editor on the Approaching Conjunction of Venus and Jupiter.

"Adverting to Professor Wolfers' remark in the *Monthly Notices* for January 14, on the near approach of *Venus* and *Jupiter* on 1859, July 20, I will call your attention to the circumstance that this conjunction is fully predicted in the *Nautical Almanac*, 1859, page 508. The prediction stands there in the following form:—

July 20, $15^h\,45^m$ ♀ ☌ ♃ ..... ♀ 0° 1′ S.

"*Royal Observatory, Feb.* 9, 1859."

Description of an Improvement in the making of large Reflecting Telescopes with Silvered Glass Specula. By M. Leon Foucault.

(*Communicated by Mr. De La Rue.*)

At the meeting of the Academy of Sciences of February 1857, the author described a new system of telescopes, the speculum of which was made of glass silvered by Mr. Drayton's process. Mirrors, so constructed, upon their removal from the bath of silver, already possess the necessary brilliancy for being used in the telescope; but the brilliancy may be still further increased by spreading, with a piece of chamois leather, the pulverulent and non-adhering layer which partly hides the polishing of the silver-coating.

Objections have been advanced against telescopes of this construction, on the ground of the fragility of the metallic layer, but experience has proved them to be unfounded. In the outset of his labours it occurred to the author that he could not do better than apply all his attention to the obtaining of spherical surfaces, the only ones which are produced regularly by mechanical means. In endeavouring to ascertain and to correct all the defects which subsist in such surfaces, he succeeded in executing at pleasure all the ellipsoidal surfaces which establish the transition between the sphere itself and the paraboloid of revolution. As a preliminary to such labours he made himself acquainted with the various processes employed in the working of glass by practical experience in the workshop of M. Secretan, the well-known optician of Paris.

As soon as a piece of glass which was being worked out began to reflect the light to a focus it was submitted to a course of optical experiments, by means of which it was possible to find the real shape of the surface. In the course of these experiments the author became convinced that surfaces as formed by opticians acquire their real value only at the final period of the polishing. They further served to establish this unexpected result, that the mechanical processes actually used in arts and industrial pursuits do not realise the spherical surface with such a degree of approximation as to withstand optical test. This circumstance suggested to him the idea of retouching polished surfaces with the hand in those instances wherein there existed defects, and of modifying them by means of local corrections, until light showed them to be irreproachable.

"This operation, which is repugnant to practical men, answered better than one could have hoped. When attempted for the first time on a speculum of 36 centimètres (14 inches) in diameter, which exhibited a central eminence very prejudicial to the formation of images, it brought the surface back in a few hours to a figure which was sensibly spherical. Afterwards, by dint of several successive tests to which the surface was submitted, I established beyond doubt the fact that it is possible by such means to arrive at a true spherical surface. I then conceived the idea of realising the parabolic surface with the degree of approximation necessary for the application I had in view.

"The method employed in effecting this transformation of the surface of the sphere is founded on the direction which the aberrations follow in the different positions of the conjugate foci of a concave speculum.

"If the speculum be exactly spherical, a luminous point placed at the centre of curvature produces at that centre an image which is free from aberration. But when the luminous point approaches the principal focus, the image recedes and is surrounded by an image of aberration which increases with the distance.

"First, let us conceive that the luminous point undergoes only a very slight displacement, in order that the image formed in its vicinity exhibits only a slight aberration, or the first indications of such. The speculum may then be corrected by means of a polisher of a suitable form until the aberration disappears; and as the mirror was spherical it becomes now ellipsoidal by the duplication of the primitive centre in two corresponding foci at the places occupied by the luminous point and its image.

"This correction being made for a first distance of the two foci it would be easy to find the value of that distance. Upon bringing the luminous point nearer to the mirror, new aberrations would reappear, which in their turn might be corrected by a similar process, and then the ellipsoid to which the surface of the mirror belongs *would be increased* in length.

"Pursuing the same course by a small quantity at a time the ellipsoid is lengthened progressively until it is transformed into a paraboloid of revolution, that is to say, until the mirror has acquired the property of reflecting light to an infinite distance without any perceptible aberration.

"This method was first applied to a mirror of 25 centimètres (9·8 inches) in diameter, and was carried out only to a certain limit, so that the instrument collected in one focus only the rays emitted from a distance of 10 mètres (32·8 feet). However it has been thought desirable to maintain the instrument in this condition, in order that it may undergo other experiments, which, upon being repeated on a limited space, would demonstrate in a most decisive way the efficacy of the reflexion of the rays by the whole surface.

"A second mirror of the same diameter, the figure of which has been corrected in such a way that it became almost parabolical, and formed in the tube of the telescope at the distance of 1 mètre (3·28 feet) an image free from aberration, of objects placed at an indefinite distance.

"A third mirror, the diameter of which was 33 centimètres (13 inches), and the focal length of which was $2^{m}\cdot25$ ($8^{feet}\cdot87$), prepared like the others, above mentioned, with much care in Mr. Secretan's workshop, presented, just after being taken from the workman's hand, a figure of revolution well centred upon its axis, but which deviated very much from the figure of a sphere. Upon being subjected to a slight re-touching it was very much improved, and the parts which were most salient have completely disappeared. Subsequently a second re-touching, effected in a very short time, gave to the mirror a parabolic figure, and upon being directed towards celestial objects it was found to perform correctly.

"Although a surface of glass reflects only $\frac{1}{20}$th part of the incidental light, it is not necessary to have the mirror silvered in order to become acquainted with the state of the surface. The light of a lamp transmitted towards the observer, by means of a partial reflexion upon the glass mirror, possesses sufficient intensity for exhibiting all the details which are interesting for the discovery of the formation of the focus. As for the asperities which modify the geometrical properties of curvature, they are in reality so infinitely small that a single rubbing of the polisher is sufficient to cause them to disappear, without the necessity of continuing the process too long. In the dimensions I have operated on until now it has taken me only six hours to retouch a whole surface; a few minutes suffice to exhibit a sensible change. This mode of attacking the substance of glass admits of the operation being suspended at any time, and definitively terminated as soon as it is thought that the real figure has been acquired. Then comes the silvering, which increases the reflecting power without the least *alteration* of curvature being apprehended."

The author then proceeds to explain a method imagined by him for ascertaining the limit of the optical power of the instrument. He concludes with the following account of a trial which he made with a mirror of 33 centimètres (13 inches) aperture, and 2·25 mètres (8^{feet}·865) focal length :—

"On the night of the 21st of July, under a pretty favourable condition of the atmosphere, we undertook an attentive examination of *γ Andromedæ*. This star (which, in refracting telescopes of 33 centimètres or less, is seen double, the principal star being of a yellow colour mixed with a little red, and the other a blue star of a greenish tint) is really triple, as Struve has shown with the Pulkowa refractor. The duplicity of the blue star is well known.

"During the whole night this feature of the double star remained doubtful; but at three o'clock in the morning, as the first rays of the sun were perceptible, the definition became very much improved; and upon applying a magnifying power of 600, the star was seen plainly divided into two small points very near each other.

"Two days afterwards, at the same hour, but with not so good definition, this observation was confirmed. Moreover, in order to guard against any imperfection which might attach to the instrument, a single star was observed, in order to ascertain if the image of the star was perfectly free from duplicity.

"An experienced observer took part in this examination. We have drawn on paper, separately and at sight, the same impressions; and afterwards, when the catalogue was consulted, we have succeeded in ascertaining that the positions were exact.

"It is, then, an established fact that the star *γ Andromedæ* was seen double with a parabolic mirror of silvered glass 13 inches in diameter and 8·865 focal length.

"These results, very satisfactory in themselves, also offer interest on account of the small expenditure which has sufficed to obtain them. Thanks to the liberal disposition, to the disinterestedness of the learned artist Mr. Secretan, who for the last two years has continued to place at my disposal all the mechanical resources of a great establishment, the expense of the process for obtaining them has been confined within such limits that a private individual would have borne them easily. I have no doubt that, by the expenditure of a sum of money comparable to what is usually laid out on astronomical instruments, it is possible to construct a telescope of comparatively moderate dimensions which would be capable of revealing regions of space hitherto inaccessible to vision."

At the close of the proceedings the Astronomer Royal favoured the meeting with an oral exposition of his views on the Motion of the Solar System in Space. An abstract of a paper embodying his recent researches on this important subject will be found at page 175.

MISCELLANEOUS INTELLIGENCE.

Death of Dr. Wichmann.

Died on the 7th of February, 1857, at five o'clock in the morning, Dr. Moritz Ludwig George Wichmann, Observer of the Königsberg Observatory. Death gently terminated a long course of suffering, which a milder climate might have alleviated, but could hardly have removed.

Wichmann was born on the 14th of September, 1821, at Celle, in the kingdom of Hanover. He first studied at Göttingen, and subsequently, in 1843, proceeded to Königsberg. Here, in 1844, he was appointed by Beud Assistant at the Observatory. In 1847 he established himself as a tutor in the University, and upon Dr. Busch being appointed Director of the Observatory in 1849, he obtained in the following year the situation of observer in that institution, which he continued to hold till his death.

He had been long in delicate health, but in 1853 he fell seriously ill from a complaint of the chest, and in the following year he proceeded to Italy in hopes of deriving benefit from the climate of that country. In August 1855 he returned, his health somewhat improved, to Königsberg after an absence of fourteen months. He now, in conjunction with Professor Luther, discharged the duties attached to the directorship of the observatory, which had become vacant by the death of Dr. Busch. About the middle of December in last year he was attacked by a severe illness which terminated in death.*

Admiral Smyth has forwarded to the Editor a copy of the Report of the Committee of the Overseers of Harvard University, appointed in February of last year to visit the Observatory of Harvard College. The visitation took place on the 15th of December last, and it acquires a melancholy interest from the circumstance of its being the last at which our late dis-

* Translation of a notice by Dr. Peters, inserted in No. 1181 of the *Astronomische Nachrichten*.

tinguished Associate, Professor W. C. Bond, officiated as Director. We extract the following passages from the Report:—

"The Director acknowledges that the dome as well as the pier which supports the great equatoreal are all that can be desired. Indeed, the fact that for several years there has been no re-adjustment of the polar axis of the telescope, furnishes unmistakeable testimony to the firmness of the structure which sustains it. This noble instrument, the Committee are happy to say, is still in perfect preservation; and continues to give the satisfaction of former years. The lenses of the object-glass have been separated during the last year, and no enlargement of the minute spots heretofore noticed has been detected. The observations and measurements made by this instrument appear to the committee to be of the utmost value in extending astronomical science; and the course pursued by the observers, not so much to accumulate observations on the same object, as to apply its power to the examination of the more remote ones, appears to us to be judicious. Observations for the revision of the position and magnitude of the stars in the interesting nebula of *Orion*, were commenced in October 1857, and brought to a close in March 1858. A systematic and thorough examination of this astonishingly rich region of the heavens was called for by singular variations recognised at different periods. From the data thus obtained, a chart may be constructed, which will faithfully represent the relative position and brightness of the separated stars. The great equatoreal has also been applied to the frequent examination of the planets *Jupiter*, *Saturn*, and *Neptune*, as well as many of the asteroids and comets.

"Since our last visit to the Observatory, the improved Bowditch comet-seeker, in the hands of Mr. Horace P. Tuttle, has been surprisingly successful. No fewer than five independent discoveries have crowned his efforts. The fourth in the order of these discoveries was the great comet of Donati, first seen at Cambridge on the 28th of June last; and it having early, and while yet telescopic, given promise of great brilliancy, extensive preparations were made to subject it to close scrutiny. The expectations of the observers were fully realised; and from the 28th of June to the 25th of October, their exertions were unremitting. If we except the comet of 1811 (its rival in beauty and its superior in the period of its visibility), no recorded comet gave greater facilities for the investigation of its physical properties. Of these favourable circumstances the observers availed themselves; and the fruits of their labours, so far as we have been able to learn, are unequalled. The various drawings of this interesting object, faithfully and skilfully executed by Mr. Henry G. Fette and the young Bond were examined by the committee with admiration. They exhibit at a glance the various changes and vagaries of the comet. These drawings have since been engraved, and in connexion with the explanations and observations of the assistant have been pub-

lished and circulated extensively in a treatise which reflects the highest honour upon the Observatory and upon its enlightened author.

"In their last year's report, the Committee dwelt upon the labours of the preceding year in the delineation of celestial objects by photography. They believed they then saw the germ of an invaluable mode of celestial research. In this they have not yet been disappointed. Their anticipations of the progress of a single year have been more than realised. The Director in his report of last year, alluded to the possibility of distinguishing stars by their chemical action, and the investigations of the assistant observer leave no question of its practicability; and the curious fact is given that the stars *α Lyræ* and *Arcturus*, though of nearly the same magnitude as seen by a telescope, or unassisted vision, yet in photographic power the former surpasses the latter by seven times.

* * * * * *

"At the instance of the Superintendent of the Coast Survey, a large number of occultations of the stars constituting the *Pleiades* have been observed simultaneously with observers at several positions near the northern extremity of the Gulf of California, as well as on the border of the Lakes Michigan and Superior. It is gratifying to perceive that the electric recording apparatus so long in use in this Observatory is appreciated in Europe, and under the title of the American method is now extensively used.

* * * * * *

"Finally, the Committee are satisfied from the report of the Director, and much other testimony and observation, that a great amount of labour has been performed, and well performed, during the last year, and they regret that the means are not in hand to give to the world, by way of publication, the large collection of results which are constantly accumulating."

The Report concludes with an allusion to the death of Mr. Bond, expressed in the following terms:—

"The Committee have hitherto dwelt on matters of an interesting and pleasing character. They come now to a most melancholy part of their duty, it is to speak of the death of the able Director of the Observatory, Prof. William Cranch Bond, which occurred on the evening of the 29th ult. On the 15th of December last, in somewhat feeble but usual health, and depressed by a family affliction, the death of Mrs. George P. Bond, he read to us in his wonted dignity and calmness his annual report, from which most of the facts in this report are derived. His oral communications on the occasion, though given with his characteristic calmness, were strongly marked with the zeal in his favourite pursuit which has distinguished his whole life. Known and appreciated as an accurate and truthful observer, wherever the science of astronomy is cultivated, *his* death will be justly deemed a great loss to science.

Several members of this Committee have enjoyed the privilege of his counsel and friendship for more than thirty years, and in yielding to the mandate that time to him shall be no longer, their humble consciousness is, that a great and a *good* man is no more."

The Society will be gratified to learn that Mr. G. P. Bond has been appointed Director of the Observatory of Harvard College in the room of his lamented father.

RECENT PUBLICATIONS.

Report on the Recent Progress of Theoretical Dynamics. By A. Cayley.*

In this report Mr. Cayley confines himself to the general theories of dynamics. The special problem of the variation of the elements of a planet's orbit is alone alluded to in consequence of its historical connexion with the subject of the report. The author gives a masterly survey of the progress of research from Lagrange's establishment of the equations of motion by a combination of the principle of virtual velocities with D'Alembert's principle, down to the labours of the geometers of the present day. At the close of his report he sums up the leading steps in the series of investigations. These are: first, the establishment of the Lagrangian form of the equations of motion; secondly, Lagrange's theory of the variation of arbitrary constants, a theory perfectly complete in itself, and it would not have been easy to see, *à priori*, that it would be less fruitful in results than the theory of Poisson; thirdly, Poisson's theory of the variation of arbitrary constants, and the method of integration thereby afforded; fourthly, Sir W. R. Hamilton's representation of the integral equations by means of a single characteristic function determinable *à posteriori*, by means of the integral equations assumed to be known, or by the condition of its simultaneous satisfaction of two partial differential equations; fifthly, Sir W. R. Hamilton's form of the equations of motion; sixthly, Jacobi's reduction of the integration of the differential equations to the problem of finding a complete integral of a single partial differential equation, and the general theory of the connexion of the integration of a system of ordinary differential equations, and of a partial differential equation of the first order, a theory, however, of which Jacobi can be considered only as the second founder; seventhly, the notion arising from the researches of Lagrange and Poisson, and ulterior development of the theory of a system of canonical integrals.

* *Report of the Twenty-seventh Meeting of the British Association for the Advancement of Science for* 1857. London, 1858.

Ordnance Trigonometrical Survey of Great Britain and Ireland:—Account of the Observations and Calculations of the Principal Triangulations; and of the Figure, Dimensions, and Mean Specific Gravity of the Earth as derived therefrom. Published by order of the Master General and Board of Ordnance. Drawn up by Captain Alexander Ross Clarke, R.E., F.R.A.S., under the direction of Lieut.-Col. H. James, R.E., F.R.S., M.R.I.A., &c., Superintendent of the Ordnance Survey, pp. 782. 4to. London, 1858. (Accompanied with a volume of Plates.)

It is well known that the Triangulation of the British Isles originated in an operation undertaken in the year 1783, under the superintendence of General Roy, for the purpose of establishing a geodetical connexion between the observatories of Greenwich and Paris. Details of the subsequent operations have been published from time to time by the Ordnance department. It has been already mentioned in the *Monthly Notices* (vol. xviii. p. 113) that the operations embrace an arc of the meridian, extending from Dunnose in the Isle of Wight to Saxavord in the Shetland Isles, and possessing an amplitude of 10° 10′ 31″·43. The present volume contains a systematic exposition of the entire triangulation, with an investigation of the figure, dimensions, and mean density of the earth hence deducible. With reference to the preparation of the work Colonel James makes the following statement:—

"The voluminous computations connected with the reduction of the observations have been made by Lieut.-Colonel Yolland, Captain Cameron, and Captain Alexander Clarke, assisted by Quarter-Master Young and Mr. O'Farrell, and a great number of computers; but the chief and most important portion of the work, including all the calculations connected with the determination of the figure, dimensions, and mean specific gravity of the earth, has been performed by Captain Clarke."

A list is given of the officers and non-commissioned officers who were personally engaged, either in the measurement of the bases or in taking the trigonometrical or astronomical observations.

We can of course do little more than merely give a statement of the principal contents of this immense volume.

The work is divided into fourteen sections.

Section I.—*Description of Stations.*

Section II.—*Description of Instruments.*

The principal instruments used in the triangulation were four large theodolites: two of these were three feet in diameter, another was two feet, and the fourth was eighteen inches. The two largest of these instruments and the 18-inch instrument were constructed by Ramsden; the remaining theodolite, of two feet in diameter, was constructed by Troughton and Simms, at the commencement of the Irish Survey.

For the determination of latitudes, the instrument used from the commencement of the survey till the year 1836 was a zenith sector constructed by Ramsden.* It consisted of an arc of $15\frac{1}{2}°$, divided to every $5'$, and having a micrometrical division by which quantities of about a tenth of a second could be estimated. The radius of the arc was eight feet, and the aperture of the telescope was four inches. The instrument was read off by means of a plumb-line. The apparatus for placing it truly vertical, for reversing its position, and for bringing the plumb-line over the centre of the instrument, were all very perfect; but notwithstanding the general excellence of the instrument, it was not well adapted to constant transport from station to station, by reason of its complex construction and the number of separate parts of which it was composed. The necessary reversal of the instrument on successive nights, and the consequent delay during bad weather, was also a source of considerable inconvenience.

"In consequence of these deficiencies, an application was made to the Astronomer Royal by Major-general Colby for a new zenith sector, by which the observations of a single night might give an accurate determination of the latitude. The design was furnished, and eventually the construction of the instrument superintended by Mr. Airy.

"The first principle in this instrument, now known as Airy's zenith sector, was the arrangement for making successive observations in two positions of the instrument, face east and face west, at the same transit. The second principle was the substitution of a level or system of levels for the usual plumb-line. The third principle was the casting in one piece, as far as practicable, of each of the different parts of the instrument, in order to avoid the great number of screws and fastenings with which most instruments are hampered, and to secure, if possible, perfect rigidity."

These remarks are followed by a description of the instrument, and of the method of making observations with it.

Section III. *Reduction of Observations.*—In this section an exposition is given of the method, founded on the theory of probabilities, for reducing the observed to the most probable mean bearings.

Section IV. *Observations, Terrestrial and Astronomical, for the principal Triangulations.*—The first part of this section contains extracts from the reduced theodolite observations of terrestrial objects; the second part contains the principal azimuthal observations in detail, and the results of those which are less important, together with their range and probable error; the third part contains the results of the observations made with Airy and Ramsden's zenith sectors for the latitudes of various points in the triangulation.

* This instrument was destroyed by the fire which happened in the Tower of London in 1841.

Section V. *Measurement of Base Lines.*—Some details relative to the subject of this section have been already given in the *Monthly Notices* for April 1858 (vol. xviii. p. 221).

Section VI. *Principles of Calculation.*—The theory of spheroidal triangles is investigated. The error resulting from the solution of such triangles by spherical trigonometry is demonstrated to be inconsiderable. It is further shown that no appreciable error can arise from the application of Legendre's theorem to the computation of spheroidal triangles. This theorem is accordingly employed in the calculations, in preference to the chord method which was exclusively used in the earlier part of the operations. Formulæ are then investigated for calculating the longitudes, latitudes, and azimuths. The section concludes with an exposition of the method employed for determining the most probable values of the corrections to the observed mean bearings of the triangulation.

Section VII. *Reduction of the Triangulation.*—In order to obtain the most probable values of the corrections to the observed bearings, Colonel Yolland directed that the whole triangulation should be reduced according to the method of least squares. As the number of equations of condition amounted to 920, their solution in one mass was impracticable. However, a mode of treatment was devised which was equally effective for the object in view, and was naturally suggested by the relations in which the corrections stood towards each other in the equations. This consisted in breaking up the triangulation into a number of separate parts or figures, each of which afforded a limited number of equations of condition, and then applying the theory of probabilities to the solution of each group of equations.

Section VIII. *Triangles and Distances.*—The definitive values of the absolute distances are made to depend on the Lough Foyle and Salisbury Bases. The distance between the parallels of Dunnose and Saxavord is found to be 3,729,334·07 feet. The distance between Easington and Saxavord was computed by two different series of triangles. By the one the distance was found to be =2,288,427·29 feet; by the other it came out =2,288,427·38 feet. Both Easington and Saxavord lie nearly on the same meridian. The distance of Saxavord from the meridian of Easington was found

By the first train of calculations	= 222·56 feet.
By the second ,,	= 221·94 ,,
Difference	0·62

Section IX. *Observed Zenith Distances and Altitudes.*—The theory of probabilities is applied to the determination of the mean observed zenith distances. The coefficient of refraction is investigated in two different ways: first, by direct observation from one point to another, the heights of both points

being exactly ascertained by levelling; secondly, by combining reciprocal observations of zenith distance. The results are interesting. With regard to 31 values of the coefficient deduced from levelled heights, it is found that in the cases wherein the rays cross the sea, the mean value of the coefficient amounts to 0·0817; while for the rays which pass over the land, the mean value of the coefficient is 0·0772. Omitting the case of Ben Nevis, in which the atmosphere appears to have been in an abnormal state, the value of the coefficient is found to be 0·0756 for rays not crossing the sea. The following are the results deduced from pairs of reciprocal observations:—In 35 cases wherein the ray passes for a considerable portion of its length over the sea, the value of the coefficient $k = 0{\cdot}0801$; in 13 cases wherein the ray passes also over the sea, but for a smaller portion of its length, the value of $k = 0{\cdot}0778$; in the remaining cases, wherein the ray is inland, the value of $k = 0{\cdot}0744$. The definitive result is,

$$\text{Coefficient of refraction} \begin{cases} = 0{\cdot}0809 \text{ for rays crossing the sea.} \\ = 0{\cdot}0750 \text{ for rays not crossing the sea.} \end{cases}$$

The heights of a considerable number of the stations were determined by spirit-levelling; the heights of others were deduced from the levelled heights, combined with the observed zenith distances. Thus from the levelled heights of Ben Nevis (4406·3 feet) and Ben Lomond (3192·2 feet), the heights of four other stations were determined. In this way the height of Ben Macdui was found to be 4295·6 feet. This result has recently received a remarkable confirmation. By levelling up the western side of the mountain, the height has been found to be 4295·70 feet; and by levelling down the eastern side, the result was 4295·76 feet.

Section X. *Connexion of Geodetical and Astronomical Observations.* — In this section the subject of local attraction is generally considered, and formulæ are investigated for computing the amount of deflection upon various suppositions with respect to the disturbing mass. From observations made at Arthur's Seat, near Edinburgh, in 1855, the relative deflection at the north and south stations of the hill was found to amount to 4″·07, whence the mean density of the earth was concluded to be $5{\cdot}316 \pm \sqrt{3{\cdot}725\,\epsilon^2 + 0029}$, ϵ being the probable error of the mean density of the hill, which is assumed by Colonel James to be 2·75, from the examination of a great many substances.

Section XI. *Determination of the Amount of Local Attraction at various Stations in the Triangulation.* — An explanation of the method of computation is given, and is applied to several of the stations for which sufficient data are available. The attraction is also approximately ascertained for a few other stations which have indicated an unusually large amount of disturbance.

Section XII. *Determination of the Spheroid most nearly representing the Surface of Great Britain and Ireland.*—Airy's elements of the earth's figure are assumed as the basis of investigation. The distance between Dunnose and Saxavord, when computed from these elements, is found to be 3,729,335·69; the triangulation of the survey gives 3,729,334·07 for the distance between the same stations. The difference, consequently, amounts to only 1·62 feet. However, a comparison of the observed and computed latitudes exhibits systematic indications of a small error in the elements. This leads to a thorough investigation, founded upon a comparison of the computed latitudes, longitudes, and azimuths, with the corresponding results of observation. The following are the elements of the spheroid which is found to agree most nearly with the surface of the British Isles:—

$$a = 20{,}927{,}c05 \pm 295.$$
$$c^* = 280{\cdot}4 \pm 83.$$

Section XIII. *Of the Length of the Degree, &c.: Longitudes and Latitudes and Directions of the Meridian at the different Stations.*—The following is the expression obtained for the length of a degree of the meridian in the British Isles:

$$\text{Length} = 364{,}594{\cdot}1 - 1953{\cdot}8 \cos 2\lambda + 4{\cdot}4 \cos 4\lambda$$

the unit being a foot.

Section XIV. *Figure of the Earth.*

In the *Philosophical Transactions* for 1856 there is a paper containing a new determination of the Figure of the Earth, founded upon the arcs used by Bessel, but including the extensions of the English and Indian arcs (*Monthly Notices*, vol. xviii. p. 220). The investigation in the present volume includes also the Russo-Scandinavian arc, extending from Ismail, in lat. 45° 20′, to Fuglenas, in lat. 70° 40′, and having, consequently, an amplitude of 25° 20′ (*Monthly Notices*, vol. xiii. p. 201). The data of this arc were furnished to the Ordnance by Professor Struve; the latitudes are not final, but the probable errors of the latitudes stated are less than half a second. The elements of the earth's figure are determined on two suppositions, the non-elliptic and the elliptic. On the first hypothesis the mean value of a correction to an observed latitude is $\pm$ 2″·064; on the second the mean value is $\pm$ 2″·098. The figure deduced, independently of the elliptic assumption, does not differ sensibly from a spheroid of the same axes, the amount of the protuberance of a meridional section being represented by

$$\delta r = (177^{\text{feet}}{\cdot}5 \pm 70{\cdot}9) \sin^2 \lambda,$$

λ denoting the latitude.

But the deviation is found to be greater, when the absolute values of the axes are considered.

* The reciprocal of the ellipticity.

The following table affords a view of the definitive results derived from the two hypotheses:—

Quantities.	Elliptic.	Non-Elliptic.	Diff.
a	20,926,348 ± 186	20,927,197 ± 385	849
b	20,855,233 ± 239	20,855,493 ± 237	260
$\frac{1}{2}\cdot\frac{a+b}{a-b}$	293·76 ± 1·06	291·36 ± 1·39	2·40
Degree	364,613·33 ± 3·00	364,624·57 ± 5·36	11·24

Combining Colonel Everest's Indian arc from Daumergida to Kaliana with the English arc from St. Agnes to Saxavord, the value of the mean degree is found to be 364,623·50 ± 7·15 and $a : b = 292{\cdot}39 : 291{\cdot}39$; probable error ± 2·67.

Combining the same Indian arc with the Russian arc, the value of the mean degree is found to be = 364,616·07 ± 6·59 and $a : b = 294{\cdot}58 : 293{\cdot}58$; probable error ± 2·27.

The three arcs combined together give 364,619·19 ± 6·14 for the value of the mean degree, and $a : b = 294{\cdot}44 : 293{\cdot}44$; probable error ± 2·26.

The foregoing determinations conclude with this remark:—

"Until the exact latitudes of the thirteen stations in the Russian arc are known we cannot state the precise elements best representing all the geodetical operations. They cannot, however, be far from the following:—

Mean Degree of Meridian	364,616 feet of Ordnance Standard
Ratio of Semi-Axes	293 : 294."

The work concludes with some remarks upon the Indian arc.

Errata in the Art. Physical Astronomy (Encyclopædia Metropolitana) communicated by Captain Tennant.

Page 671, in equation (87), read

$$\tan \Omega = \frac{\sin l'' \tan b - \sin l \tan b''}{\cos l'' \tan b - \cos l \tan b''}$$

And in equation (90) of the same page,

$$\phi = \frac{l'' - l}{2}.$$

To be disposed of, an Iron Equatoreal Stand adapted for carrying a refractor of from 3 to 4 inches aperture. May be seen at the rooms of the Society.

ERRATA.

Page 28, line 18 from bottom, *for* Lieut. Chinmo, *read* Lieut. W. Chimmo.
— 79, — 10–12, 18–20, the numbers here stated refer to seconds of arc.
— —, — 23, *for* uninterruptedly, *read* interruptedly.
— 86, — 20, *for* 11 A.M. *read* 11 P.M.

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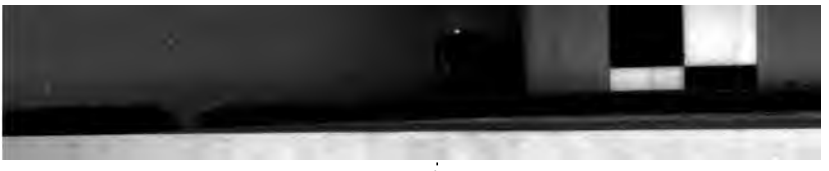

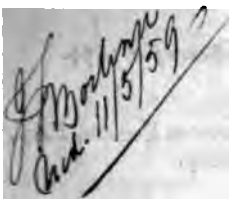

ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *April* 8, 1859. No. 6.

The Rev. R. MAIN, President, in the Chair.

The Rev. Wm. Williams Walton, M.A., Waddesdon, Bucks, was balloted for and duly elected a Fellow of the Society.

Elements of Europa. By M. Lepissier.

(*Extract of a Letter to Mr. Carrington.*)

"I herewith send you the last approximation I obtained of the elements of *Europa*, derived from eighty-one observations, extending from the 6th of February, 1858, the day of discovery, to the 7th of June. These elements represent the observations very well, the maximum value of the excess of the observed over the calculated results being, for the two extreme normal positions,

$1^s{\cdot}48$ and $-0^s{\cdot}74$ in right ascension,
and $+1''{\cdot}83$ and $-5''{\cdot}16$ in declination.

Epoch 1859, January 1, Paris M.T.

$\varepsilon - \varpi$	..	..	100 14 56·10
ϖ	..	..	101 18 14·41
ϑ	..	..	129 59 6·48
i	..	..	7 24 48·37
φ	..	..	5 47 51·45
n	..	..	650″·1866

Accompanying these elements was an ephemeris of the planet, computed from them, and extending from March 27 to May 6, 1859.

Ephemeris of the Variable Stars for 1859. By Norman Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

In the following ephemeris the initials in the sixth column represent the following names:—A., Argelander; An., Anvers; B., Baxendell; K., Krüger; P., Pogson; S., Schönfeld: W., Winnecke.

Star.	Probable Mag.	Mean Place, 1860. R.A. h m s	N.P.D. ° ′	Times of Maxima.	Authority
T Piscium	9·5	0 24 46	76 14	End of July and middle of December	S.
ω Cassiopeiæ	2·0	0 32 35	34 14	March 9, May 27, Aug. 14, Nov. 1	A.
S Piscium	9·0	1 10 15	31 48	Middle of April	—
R Piscium	7·5	1 23 25	87 50	End of June	—
o Ceti	2·0	2 12 17	93 37	October 6	A.
R Tauri	8·0	4 20 38	80 9	September 14	W.
S Tauri	10·0	4 21 33	80 22	Middle of March	W.
R Orionis	9·0	4 51 22	82 5	May 14	W.
ω Orionis	1·0	5 47 36	82 37	Early in May and middle of November	—
R Geminorum	7·3	6 58 56	67 5	Not until 1860, January 2 ...	P.
R Can. Min.	8·0	7 1 0	79 45	Middle of April	—
S Can. Min.	8·5	7 25 7	81 23	September 11	S.
S Geminorum	9·2	7 34 38	66 14	May 30	P.
T Geminorum	8·9	7 40 54	65 55	March 17, December 31 ...	P.
U Geminorum	9·1	7 46 48	67 38	Feb. 21, May 29, Sept. 3, Dec. 9	P.
R Cancri	6·0	8 8 57	77 53	March 25	A.
U Cancri	10·0	8 27 45	70 37	April 7	W.
S Hydræ	8·5	8 46 15	86 24	March 31, December 11 ...	S.
T Hydræ	6·5	8 48 51	98 37	End of April	W.
R Leonis	5·0	9 40 2	77 55	March 8 (Minimum (10·0), Aug. 13)	P.
R Urs. Maj.	7·0	10 34 41	20 29	Jan. 5, Nov. 3 (Minimum, July 15)	P.
R Comæ	8·0	12 57 4	70 26	End of October	W.
R Virginis	6·5	12 31 24	82 14	March 21, August 13 ...	A.
S Urs. Maj.	7·9	12 37 48	28 8	Feb. 17, Sept. 28 (Minimum, June 25)	P.
U Virginis	7·8	12 44 0	83 41	Possibly early in March and Dec.	—
R Hydræ	4·0	13 22 5	112 33	May 31, irregular	B.
S Virginis	6·0	13 25 42	96 28	April 23	P.
S Serpentis	8·0	15 15 8	75 11	April 9	A.
R Coronæ	6·0	15 42 49	61 25	Early in October; very irregular	—
R Serpentis	6·5	15 44 15	74 26	September 17	A.

Star.	Probable Mag.	Mean Place, 1860. R.A. h m s	N.P.D. ° ′	Times of Maxima.	Authority.
R Herculis	8·0	15 59 56	71 15	May 26	B.
S Scorpii	9·0	16 9 17	112 33	End of April; doubtful ...	P.
R Scorpii	9·0	16 9 21	112 35	Very doubtful; not expected in 1859	P.
S Ophiuchi	9·5	16 26 12	106 53	May 13, December 28 ...	P.
S Herculis	9·0	16 45 32	74 49	May 12	B.
R Ophiuchi	7·6	16 59 44	105 54	March 13	P.
α Herculis	3·0	17 8 16	75 27	Feb. 7, April 14, June 20, Aug. 25, Oct. 30	A.
T Herculis	7·9	18 3 48	59 0	February 20, July 30 ...	K.
R Scuti	5·0	18 40 1	95 50	Jan. 28, April 9, June 20, Aug. 31, Nov. 10; irregular ...	A.
13 Lyræ	4·3	18 51 4	46 14	Jan. 15, March 2, April 17, June 3, July 19, Sept. 3, Oct. 19, Dec. 4	B.
R Aquilæ	6·5	18 59 40	81 59	June 23	W.
R Sagittarii	8·2	19 8 28	109 33	Possibly early in October ...	P.
R Cygni	8·0	19 33 4	40 7	June 12	P.
χ Cygni	5·0	19 45 11	57 27	January 29	A.
U Capricorni	10·7	20 40 22	105 18	Possibly about the end of December	P.
T Capricorni	9·0	21 14 17	105 45	July 26	S.
β Pegasi	2·0	22 56 59	62 41	Feb. 5, March 21, May 3, June 16, July 29, Sept. 10, Oct. 24, Dec. 6	An.
R Pegasi	8·5	22 59 38	80 13	March 21	A.
R Aquarii	7·0	23 37 15	106 3	June 2	A.
R Cassiopeiæ	6·0	23 51 18	39 23	April 12	P.

Minima of the Short Period Variable Stars during 1859 *in G.M.T. and for the Earth's Mean Distance from each Star.*

Algol.

Maximum 2·3; Minimum 4·0. Whole variation completed in seven hours.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	3	13	9	April	2	10	24	Sept.	21	11	16	Nov.	9	5	7
	6	9	58		5	7	13		24	8	5		20	16	22
	9	6	47		25	8	55	Oct.	8	16	9		23	13	11
	26	11	40	June	24	14	1		11	12	58		26	10	0
	29	8	29	July	17	12	32		14	9	47		29	6	49
Feb.	1	5	18	Aug.	6	14	14		17	6	36	Dec.	10	18	4
	18	10	11		9	11	3		28	17	51		13	14	53
	21	7	0		26	15	56		31	14	40		16	11	42
Mar.	10	11	53		29	12	45	Nov.	3	11	29		19	8	31
	13	8	42	Sept.	1	9	34		6	8	18		22	5	20

λ *Tauri.*

Max. 4·0; Min. 4·5. Variation rapid.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	1	6	8	Aug.	10	14	51	Nov.	1	15	7	Nov.	21	9	28
	5	5	0		14	13	43		5	14	0		25	8	21
Mar.	17	8	40		18	12	36		9	12	52		29	7	13
	21	7	32		22	11	28		13	11	44	Dec.	3	6	5
Aug.	6	15	59	Oct.	28	16	15		17	10	36		7	4	57

ζ *Geminorum.*

Max. 3·8; Min. 4·5. Increasing $5^d\,6^h$.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	9	5	24	Mar.	21	8	0	Oct.	20	15	48	Dec.	30	18	24
	19	9	12		31	11	48	Dec.	10	10	48				
	29	13	0	Oct.	10	12	0		20	14	36				

S *Cancri.*

Max. 8·0; Min. 10·5. Variation rapid.

R.A. $8^h\,35^m\,56^s$; N.P.D. 70° 28′.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	16	15	19	April	2	12	15	June	17	9	10	Dec.	14	13	51
Feb.	4	14	33		21	11	28	Oct.	18	16	10				
	23	13	47	May	10	10	42	Nov.	6	15	24				
Mar.	14	13	1		29	9	56		25	14	37				

β *Lyræ.*

Principal Minimum (4·3); First Max. (3·4) about $3^d\,2^h$ after.
Second Min. (3·8) about $6^d\,9^h$ after.; Second Max. (3·4) about $9^d\,10^h$ after.

	d	h	m		d	h	m		d	h	m		d	h	m
Feb.	11	17	50	Mar.	9	13	21	July	16	14	53	Aug.	11	10	23
	24	15	36		22	11	6		29	12	38		24	8	8

η *Aquilæ.*

Max. 3·6; Min. 4·4. Increasing $2^h\,9^d$.

	d	h	m		d	h	m		d	h	m		d	h	m
Feb.	15	18	12	June	10	13	57	Aug.	21	8	18	Nov.	8	6	53
Mar.	23	15	22	July	16	11	7		28	12	32	Dec.	14	4	3
April	28	12	33		23	15	22	Oct.	3	9	42				

δ *Cephei.*

Max. 3·7; Min. 4·8. Increasing $1^d\,14^h$.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	4	16	31	Mar.	15	10	51	July	11	12	20	Nov.	6	13	48
	15	10	6		31	13	14		27	14	43		17	7	24
	20	18	54	April	16	15	37	Aug.	23	10	41		22	16	11
	31	12	29		27	9	12	Sept.	8	13	4	Dec.	3	9	47
Feb.	11	6	5	May	13	11	35		24	15	27		19	12	10
	16	14	52		29	13	58	Oct.	5	9	2		30	5	45
	27	8	28	June	25	9	57		21	11	25				

The corrections to be applied to the observed times of minima of the short-period variables, before comparing with the ephemeris may be found by the formulæ on page 27 of vol. xvii. of the *Monthly Notices*.

With very few exceptions, the same elements have been employed in preparing this as in the last year's ephemeris. When no authority is stated, the times of maxima must only be regarded as indications as to when a search will prove most beneficial for the determination of the elements of variation of such star. If invisible, a mere record of such being the case will, by contradicting the assumed elements, remove an erroneous prediction from the next ephemeris, and thereby possess great interest and value. For the short-period variables (all of which except λ *Tauri* are from Prof. Argelander's elements), those minima only are tabulated which occur when the sun is considerably below, and the star at least ten degrees above, the horizon.

I have again to acknowledge my obligation to Mr. Baxendell for having obligingly communicated the ephemerides of λ *Tauri*, 13 *Lyræ*, R and S *Herculis*, based upon elements derived from his own observations.

The unavoidable delay in the preparation of this little annual contribution is a matter of much regret, but one which, I am happy to say, is not likely to happen again. Change of residence, and other pressing interruptions, all conspired to derange my plans, and throw me behind in their execution. The variable stars are, however, marked out as special objects of observation and investigation in Dr. Lee's Observatory, and I shall therefore hope to have the ephemeris for 1860 in print before the close of the present year.

Hartwell Observatory, April 11, 1859.

Results of the Observations of Small Planets, made at the Royal Observatory, Greenwich, in the month of March, 1859.

(*Communicated by the Astronomer Royal.*)

Astræa (5).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 7	12 59 34·0	12 0 24·08	84 29 56·20
9	12 50 12·9	11 58 54·56	84 13 7·18

Metis (9).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 31	14 14 41·2	14 50 20·95	100 57 18·71

Irene (14).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 1	14 23 12·3	13 0 36·80	79 18 8·95
8	13 52 30·0	12 57 25·39	78 29 5·68
9	13 48 0·1	12 56 51·22	78 22 9·56
10	13 43 29·1	12 56 16·06	78 15 15·83
18	13 6 31·1	12 50 44·45	77 22 58·30
30	12 9 21·4	12 40 43·99	76 22 53·26
31	12 4 33·5	12 39 51·87	76 19 15·35

Calliope (22).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 1	14 3 9·5	12 40 30·72	73 50 35·11
7	13 34 28·7	12 35 24·57	73 16 37·40
8	13 30 48·3	12 35 40·09	73 11 12·98
9	13 26 7·3	12 34 54·86	73 5 57·04
10	13 21 25·7	12 34 9·10	73 0 45·07
18	12 43 30·0	12 27 39·55	72 23 13·97
21	12 29 9·7	12 25 6·61	72 11 29·37
31	11 41 19·4	12 16 33·90	71 43 24·75

Themis (24).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 9	14 55 38·1	14 4 40·43	102 19 20·85
31	13 18 42·3	13 54 12·87	101 26 51·27

Harmonia (40).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 5	10 26 29·2	9 19 1·01	68 25 12·60
15	9 41 21·5	9 13 11·53	68 5 9·02
18	9 28 25·3	9 12 2·78	68 2 41·09
19	9 24 10·1	9 11 43·45	68 2 11·08
23	9 7 29·0	9 10 45·84	68 2 6·48
28	8 47 20·6	9 10 16·90	68 5 38·67
31	8 35 38·4	9 10 22·46	68 9 33·37

Isis (42).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Mar. 7	11 45 26·0	10 46 3·94	68 47 19·37
10	11 30 49·9	10 43 15·12	68 32 27·74
21	10 38 4·7	10 33 43·30	67 52 3·55

On the Secular Variation of the Eccentricity and Inclination of the Moon's Orbit. By Professor Adams.

"In a memoir read before the Royal Society in June, 1853, I showed that the secular variation of the moon's mean motion is given by means of the equation

$$\frac{dn}{n\,dt} = \frac{e'\,de'}{dt}\left\{-3m^4 + \frac{3771}{32}m^6\right\},$$

in which the coefficient of m^4 is totally different from that in Plana's result.

"I have since carried the approximation to the seventh order in m, and find that

$$\frac{dn}{n\,dt} = \frac{e'\,de'}{dt}\left\{-3m^2 + \frac{3771}{32}m^4 + \frac{34047}{32}m^5 + \frac{306865}{48}m^6 + \frac{17053741}{576}m^7\right\}.$$

This reduces the coefficient of $\left(\frac{t}{100}\right)^2$, in the expression for the acceleration to $5''{\cdot}7$, only about one-half of the value hitherto received. M. Delaunay has recently verified my coefficient of m^4; and he informs me that he shall very soon have carried the approximation to the eighth order in m, and included the terms depending on e^2 and γ^2.

"In my memoir above referred to I mentioned that other elements of the moon's orbit suffer secular changes which had been overlooked.

"I find the following expressions for the secular variation of the eccentricity and inclination of the moon's orbit, adopting Plana's definitions of e and γ:—

$$\frac{de}{dt} = ee'\frac{de'}{dt}\left\{\frac{235}{64}m^2\right\}$$

$$\frac{d\gamma}{dt} = \gamma e'\frac{de'}{dt}\left\{-\frac{221}{64}m^2 + \frac{779}{256}m^3 + \frac{199631}{4096}m^4\right\}$$

"I am engaged in carrying on the approximation to the value of $\frac{de}{dt}$ to the same extent as I have done in the case of $\frac{d\gamma}{dt}$, and in finding the part of the secular variation of the mean motion which depends on e^2 and γ^2. These terms, however, can only very slightly affect the numerical value of the secular acceleration."

Supplement to the foregoing.

(*Note to the Editor.*)

"Since I sent my result respecting the secular variations of the eccentricity and inclination of the moon's orbit to the Society the other day, I have found the leading terms of the secular acceleration of the mean motion which depend on the eccentricity and inclination of the orbit. The result is one of remarkable simplicity, considering the nature of the calculations which have led to it; and I should be glad if you would let it appear in the *Monthly Notices* as soon as you conveniently can, as a supplement or a note to my former communication. The result is,

[I have not written down the coefficients of higher powers of m, as given in my former note.]

$$\frac{dn}{n^2\,dt} = \frac{e'\,de'}{dt}\left\{-3m^2 + \frac{3771}{32}m^4 + \&c. - \frac{27}{8}m^2e^2 + \frac{27}{8}m^2\gamma^2\right\}.$$

"It is curious that the coefficients of e^2 and γ^2, in this expression, are equal and of contrary signs, although they are found by totally distinct processes. The effect of the terms in e^2 and γ^2 on the magnitude of the secular acceleration is, as I anticipated, very insignificant. The term in e^2 increases the coefficient of the square of the number of centuries by $0''{\cdot}036$, and that in γ^2 diminishes the same coefficient by $0''{\cdot}097$; so that, on the whole, the coefficient $5''{\cdot}70$, which I previously found, must be diminished by $0''{\cdot}06$, or reduced to $5''{\cdot}64$. This value I believe to be within one-tenth of a second of the true *theoretical* value of the coefficient of the secular acceleration. Whether ancient observations admit of such a small value of the acceleration is a different question.

"J. C. Adams.

"*St. Andrews, 16th April*, 1859."

On the Apparent Projection of Stars upon the Moon's Disk in Occultations. By G. B. Airy, Esq., Astronomer Royal.

It is well known that on occasions of the occultation of a star by the moon the star has been frequently observed to advance apparently upon the moon's disk, and after remaining visible in this manner for a very short time has then vanished instantaneously. In the paper above entitled, the author inquires into the origin of this curious phenomenon.

Nearly thirty years ago the author himself saw the phenomenon of projection in great perfection. The singularity of

the phenomenon made a strong impression on his mind, and induced him frequently to consider the possibility of explaining it on recognised optical principles. The only remark which appeared to lead towards a conjectural explanation was one by Sir James South, that in many instances a red star was the subject of projection. It seemed then not impossible that, if the light of the moon and the light of the star differed materially in refrangibility, the rays of the star (supposed the less refrangible), though actually touching the moon's body at the instant of occultation, might be so much less bent than the rays of the moon by the action of the earth's atmosphere, that at that instant the star would actually be seen lower than the moon's limb. If, then, the occultation took place at the moon's upper limb, there would be projection of the star on the moon's disk. If it took place at the lower limb, the star would disappear at a small distance external to the limb. If the rays of the star were more refrangible than those of the moon, these appearances would be reversed. Here, then, was indicated a distinct criterion upon which the fate of the hypothesis must depend. It became necessary only to ascertain whether, in the instances of recorded projection, the star was uniformly higher or uniformly lower, as referred to the zenith of the place of observation, than the moon's centre. If either of these laws held uniformly, the hypothesis would be plausible; if neither law was sufficiently maintained, the hypothesis would be certainly false.

In order to test the hypothesis by this criterion, it was necessary to compute, to a considerable degree of approximation, the apparent zenith distances of the moon and star for all the occultations on the occasion of which the phenomenon was observed. The author has accordingly collected together and subjected to computation all the recorded observations bearing upon the question which were accessible. This laborious operation was carefully executed by Mr. Ellis, one of the assistants at the Royal Observatory. The author has divided the observations into four distinct classes. The results of the calculation are shown not to favour the conjecture which gave rise to them.

The author then proceeds to point out a circumstance which appears to him to throw light upon the phenomenon. It is contained in an account of the disappearance of *Aldebaran* at the moon's bright limb on October 23, 1831, as observed by Mr. George Innes at Aberdeen. (*Ast. Nach.* vol. x. p. 211.) Mr. Innes states that when *Aldebaran* came within about six seconds of the moon's limb, it passed through the remaining distance with great rapidity, its apparent velocity becoming five or six times as great as before. It appeared to hang on the moon's limb for about five seconds of time, and then suddenly disappeared. In other words, the disk of the moon appeared suddenly to swell or to extend itself into what was previously

the dark space. The author does not doubt that the statement made by Mr. Innes involves a severely accurate account of the appearances which might have been observed in other cases if the observers had been equally cool and accurate. He accordingly proceeds to consider whether the apparent swelling of the moon's disk can be explained on recognised optical and physiological principles.

The opinion which the author entertains of the origin of the phenomenon is founded on the undulatory theory of light. According to that theory the image of a luminous point in a telescope (whether of a star or a portion of the moon's illuminated surface) is not a point. The bright rings which appear to surround the image of a star as seen in a good telescope, afford an apt illustration of the actual distribution of light in such cases. The author thus expresses his views on the subject:—

"It is to be conceived that every luminous point of the moon's disk is accompanied with a similar system of rings, and therefore that the aggregate of light produced by the aggregate of all the luminous points of the moon's disk, is not a luminous image bounded by a sharp outline at what we consider the geometrical outline of the image, but that the geometrical outline is fringed by a band of illumination, produced by the interlacing and superposition (not interference) of all the systems of rings. The integral which determines the brightness is so unmanageable that I am unable at present to assign the numerical values of the brightness at different distances from the geometrical outline (though there would be no difficulty in finding them by the troublesome method of quadratures); but as the light from the different sources is actually superposed and aggregated, it is certain that there must be a considerable quantity of light external to the geometrical limb. And when, with a very fine telescope, we see the moon's limb very sharply defined, and apparently surrounded by immediate darkness, we do in reality see it erroneously. Probably some operation of the mind, under the conviction that the outline of light ought to fall in a given curve, acts on the animal faculty of sensation so as to incapacitate the visual organs from perceiving the fainter light beyond that curve.

"But in the excitement and intentness of observing an occultation, the state of the sensational organisation is probably much changed. And as the presumed time of the phenomenon comes nearer and nearer, the eye probably becomes more and more sensible to the faint diffused light, and the visible boundary of light extends further and further into the darkness. And this presents that appearance of rapidly increasing velocity of approach which is so graphically described by Mr. Innes.

"In numerous instances, when the boundary of the moon's light has swelled till it touches the star, it swells no further, and the star 'hangs' on the moon's limb. It seems perfectly

conceivable that the mental contemplation of the relation of the positions of the moon and star which is implied in the phenomenon that is to be observed, may frequently so far act on the sensibilities that when that relation (namely, contact) is once gained, the mental effort does not make the sense more acute than is necessary, and may even somewhat relax as the denser light of the moon approaches the star.

"The explanation which I have offered of these curious phenomena does in reality bring them under the general category of irradiation. But it is a kind of irradiation which has not, so far as I know, been noticed in other instances, an irradiation which sometimes exists and sometimes (under the same astronomical circumstances) does not exist, which at the same place is seen by one observer and not by another, which sometimes remains apparently constant for at least several seconds of time, and sometimes varies from instant to instant. It is probable that there is irradiation of a similar kind when the wire of a meridional instrument is placed on the moon's limb; but critical observations are yet wanting."

Results of a Comparison of the Lunar Tables of Burckhardt and Hansen, with recent Meridional and Extra-Meridional Observations of the Moon, made at the Royal Observatory, Greenwich; with accompanying Remarks by the Astronomer Royal.

"In the last autumn a correspondence took place between Mr. Hind, Superintendent of the *Nautical Almanac*, and myself, on the subject of comparing Professor Hansen's new lunar tables with the observations of the moon made at the Greenwich Observatory. Mr. Hind entered most warmly into the matter; and, with a zeal for which astronomers cannot be too grateful, at once undertook to superintend, in the *Nautical Almanac* Office, the laborious operations for computing the moon's place from Hansen's tables, provided the funds for supporting the necessary expenses should be supplied by the Government. The Lords Commissioners of the Admiralty without hesitation granted the necessary sums, and the calculations were soon commenced on the scheme which had been arranged.

"In explanation of the plan of comparison, it is to be remarked that the Greenwich observations of the moon consist of two series, one made with the meridional transit-circle, and the other made with the extra-meridional altazimuth. The meridional observations are, nearly without exception, confined to those parts of the moon's orbit in which her transit takes place between 6^h and 18^h mean solar time. The extra-meridional

observations extend over almost all parts of the orbit, but the mean solar hour of the actual observation with the altazimuth is usually between 6^h and 18^h, and though, from the nature of the observations (which are not made simultaneously on the two elements) it is not possible, as in the meridional results, to state *precisely* the hour and minute to which the inferred tabular errors apply, yet the hour can be *very nearly* stated, and that hour will not be very far distant from midnight.

"The Greenwich observations exhibit the apparent errors of the places of the *Nautical Almanac* (Burckhardt's) as compared with every day's observations of the moon. These apparent errors are corrected (so far as applies to the present purpose) for the faults of the *Nautical Almanac* parallax (Burckhardt's) in a table at the end of the introduction to the Greenwich observations 1856. As great pains have been taken to use a correct value also of the semi-diameter, the final results of that table may be received as exhibiting, with the utmost accuracy, the errors of Burckhardt's longitude and latitude deduced from observations made on the moon with two first-class instruments, at hours not very distant from the midnight of every specified day.

"Now, as on many days, observations were made with both instruments, but at different hours; and as one of these hours is a little indefinite, and as the errors of the lunar tables alter very little in two or three hours, there was evidently a great gain of convenience, without danger of sensible inaccuracy, in referring the errors in all cases to the midnight. And the adoption of this time very greatly facilitated the remaining calculations. For the *Nautical Almanac* contains ready calculated the longitude and latitude of the moon at every midnight, and it would be necessary therefore only to compute Hansen's places for midnight, a limitation which greatly alleviates the labour of applying the tables.

"At the present time, I have received from Mr. Hind the comparison of Burckhardt's and Hansen's places only for the year 1852. But the results of the comparison for that year are so astonishing that I deem it a duty at once to lay them before the Society.

"In the following tables, to avoid a slight confusion arising from the mixture of results with the two instruments, I exhibit separately the results obtained from the transit-circle and from the altazimuth. I use the symbol B − O to denote the excess of Burckhardt's place (in either element) above the observed place, as taken from the appendix to the introduction to the *Greenwich Observations*, 1856. And I use the symbol H − B to denote the excess of Hansen's place above Burckhardt's place, as found by Mr. Hind. The sum of these, or H − O, is evidently the excess of Hansen's place above the observed place.

For Observations with the Transit-Circle.

Day. 1852.	Longitude.			Ecliptic N.P.D.		
	B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
	″	″	″	″	″	″
Jan. 3	+ 7·1	− 9·9	−2·8	− 5·2	+ 5·5	+0·3
4	+ 9·8	−11·8	−2·0	− 6·4	+ 5·5	−0·9
5	+ 9·8	−13·0	−3·2	− 6·1	+ 5·8	−0·3
6	+11·9	−14·3	−2·4	− 5·1	+ 5·8	+0·7
7	+15·1	−14·7	+0·4	− 5·5	+ 5·9	+0·4
9	+14·3	−14·5	−0·2	− 6·4	+ 5·3	−1·1
11	+16·1	−16·2	−0·1	− 2·4	+ 3·7	+1·3
25	+ 1·7	+ 0·5	+2·2	− 0·8	+ 0·5	−0·3
26	− 1·3	+ 0·5	−0·8	+ 1·2	+ 2·1	+3·3
28	− 0·1	− 2·7	−2·8	− 3·7	+ 3·5	−0·2
29	+ 3·3	− 5·5	−2·2	− 6·4	+ 5·0	−1·4
30	+ 5·3	− 8·4	−3·1	− 6·6	+ 5·1	−1·5
Feb. 1	+ 7·8	−12·2	−4·4	− 8·3	+ 6·3	−2·0
2	+ 9·8	−13·2	−3·4	− 2·1	+ 6·7	+4·6
3	+ 9·9	−13·7	−3·8	− 5·4	+ 6·5	+1·1
6	+18·1	−17·6	+0·5	− 5·5	+ 4·5	−1·0
9	+13·4	−12·8	+0·6	− 1·2	0·0	−1·2
11	+10·1	−10·5	−0·4	+ 3·3	− 3·4	−0·1
25	− 1·7	− 1·0	−2·7	− 3·4	+ 2·4	−1·0
27	+ 2·4	− 6·6	−4·2	− 4·2	+ 3·1	−1·1
29	+ 5·9	−11·6	−5·7	− 6·1	+ 5·2	−0·9
March 2	+11·9	−12·6	−0·7	− 3·9	+ 6·9	+3·0
3	+ 8·3	−11·5	−3·2	− 5·8	+ 6·9	+1·1
4	+11·3	−13·5	−2·2	− 6·2	+ 6·1	−0·1
5	+13·0	−14·8	−1·8	− 3·9	+ 4·7	+0·8
6	+12·5	−15·8	−3·3	− 1·3	+ 3·1	+1·8
28	+ 4·2	−10·7	−6·5	− 5·2	+ 4·9	−0·3
30	+14·7	−13·1	+1·6	− 9·3	+ 7·7	−1·6
April 1	+ 8·3	−12·7	−4·4	− 6·3	+ 8·1	+1·8
2	+11·5	−12·3	−0·8	− 8·3	+ 7·0	−1·3
3	+11·0	−12·7	−1·7	− 4·8	+ 5·6	+0·8
4	+11·6	−12·9	−1·3	− 0·6	+ 3·8	+3·2
5	+10·8	−13·0	−2·2	+ 0·5	+ 2·1	+2·6
26	+ 4·2	−12·7	−8·5	− 7·6	+ 7·2	−0·4
27	+ 5·0	−14·2	−9·2	− 7·8	+ 8·0	+0·2

Day. 1852.	Longitude.			Ecliptic N.P.D.		
	B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
	″	″	″	″	″	″
May 1	+ 7·8	−12·0	−4·2	− 5·4	+ 5·4	0·0
2	+ 8·6	−11·7	−3·1	− 0·6	+ 3·8	+3·2
3	+10·7	−12·7	−2·0	− 0·0	+ 2·2	+2·2
4	+10·6	−13·8	−3·2	− 2·2	+ 1·0	−1·2
7	+ 6·1	− 7·3	−1·2	+ 1·7	− 1·5	+0·2
24	+ 7·1	−14·7	−7·6	− 6·0	+ 5·4	−0·6
31	+ 7·8	−11·0	−3·2	+ 0·8	− 1·5	−0·7
June 4	+ 7·9	− 8·8	−0·9	+ 0·5	− 3·0	−2·5
22	+11·8	−18·9	−7·1	− 6·2	+ 2·8	−3·4
26	+ 5·5	−12·4	−6·9	− 1·1	− 1·2	−2·3
27	+ 4·0	−11·4	−7·4	+ 2·2	− 2·8	−0·6
29	+ 5·2	−10·1	−4·9	+ 5·6	− 5·8	−0·2
July 1	+ 5·3	− 7·8	−2·5	+ 5·9	− 6·0	−0·1
2	+ 6·2	− 6·7	−0·5	+ 8·3	− 5·2	+3·1
3	+ 3·5	− 6·0	−2·5	+ 5·6	− 4·0	+1·6
4	+ 9·0	− 5·1	+3·9	+ 2·7	− 2·1	+0·6
6	+ 7·2	− 2·7	+4·5	− 1·2	+ 1·4	+0·2
8	+ 6·1	− 1·6	+4·5	− 3·4	+ 3·7	+0·3
9	+ 4·8	− 2·0	+2·8	− 5·9	+ 4·6	−1·3
10	+ 4·5	− 2·2	+2·3	− 4·2	+ 4·7	+0·5
23	+ 9·7	−14·1	−4·4	− 2·8	+ 1·7	−1·1
27	+ 8·9	−10·4	−1·5	+ 3·4	− 4·6	−1·2
30	+ 5·6	−10·1	−4·5	+ 5·3	− 5·9	−0·6
31	+ 9·9	−10·3	−0·4	+ 6·0	− 4·6	+1·4
Aug. 2	+11·7	− 8·2	+3·5	+ 0·2	− 1·2	−1·0
3	+ 7·9	− 4·7	+3·2	+ 1·0	+ 0·5	+1·5
4	+ 8·1	− 1·2	+6·9	− 0·1	+ 1·7	+1·6
7	+ 8·6	0·0	+8·6	− 8·3	+ 5·8	−4·5
9	+ 3·5	− 2·2	+1·3	− 5·4	+ 5·1	−0·3
22	+ 5·1	− 9·0	−3·9	− 2·9	+ 1·2	−1·7
26	+11·1	− 8·8	+2·3	+ 3·7	− 3·6	+0·1
28	+13·9	−13·8	+0·1	+ 4·2	− 2·9	+1·3
29	+13·8	−15·7	−1·9	+ 2·7	− 1·6	+1·1
30	+18·1	−15·6	+2·5	+ 0·7	− 0·1	+0·6
31	+15·0	−12·6	+2·4	− 0·8	+ 0·9	+0·1
Sept. 1	+10·9	− 7·8	+3·1	0·0	+ 1·7	+1·7
2	+ 7·3	− 3·3	+4·0	− 1·2	+ 2·2	−1·0

Day. 1852.	Longitude.			Ecliptic N.P.D.		
	B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
	″	″	″	″	″	″
Sept. 23	+ 4·9	— 5·0	—0·1	+ 1·7	— 3·0	—1·3
24	+ 5·2	— 6·6	—1·4	+ 0·6	— 2·1	—1·5
25	+ 8·0	— 9·0	—1·0	+ 4·1	— 1·0	+3·1
29	+18·2	—13·7	+4·5	— 2·0	+ 3·3	+1·3
Oct. 2	+ 5·4	— 3·7	+1·7	— 4·7	+ 3·9	—0·8
3	+ 7·1	— 2·7	+4·4	— 6·8	+ 4·3	—2·5
4	+ 5·6	— 4·2	+1·4	— 7·2	+ 5·3	—1·9
6	+13·5	—11·7	+1·8	—10·8	+ 8·4	—2·4
18	+10·9	—15·8	—4·9	+ 5·0	— 7·0	—2·0
19	+10·3	—11·2	—0·9	— 4·4	— 6·9	—2·5
20	+ 8·9	— 8·9	0·0	+ 3·4	— 6·1	—2·7
23	+ 7·9	— 7·0	+0·9	+ 1·9	— 1·0	+0·9
24	+ 7·6	— 6·9	+0·7	+ 0·7	+ 0·5	+1·2
25	+ 9·3	— 7·8	+1·5	— 2·1	+ 2·0	—0·1
26	+10·8	— 9·3	+1·5	— 0·7	+ 3·2	+2·5
28	+14·6	—12·0	+2·6	— 4·2	+ 4·1	—0·1
29	+13·9	—11·5	+2·4	— 3·8	+ 4·0	+0·2
31	+ 8·3	— 7·4	+0·9	— 5·6	+ 4·1	—1·5
Nov. 2	+ 5·4	— 6·3	—0·9	— 5·6	+ 5·3	—0·3
3	+ 9·3	— 9·4	—0·1	— 8·2	+ 6·0	—2·2
17	+ 4·1	—10·6	—6·5	+ 4·5	— 5·7	—1·2
18	+ 5·9	—10·0	—4·1	+ 5·2	— 4·0	+1·2
24	+ 7·9	— 6·5	+1·4	— 4·0	+ 5·0	+1·0
26	+ 9·5	— 8·6	+0·9	— 6·3	+ 4·9	—1·4
27	+10·1	— 9·3	+0·8	— 6·5	+ 4·6	—1·9
28	+ 9·7	— 9·3	+0·4	— 4·3	+ 4·3	0·0
29	+ 8·2	— 7·7	+0·5	— 3·0	+ 3·9	+0·9
30	+ 5·6	— 6·4	—0·8	— 4·1	+ 3·8	—0·3
Dec. 2	+ 8·9	— 7·4	+1·5	— 3·8	+ 4·3	+0·5
15	+10·2	— 7·6	+2·6	+ 2·9	+ 4·4	+7·3
17	+ 7·1	— 6·6	+0·5	+ 0·5	+ 0·2	+0·7
18	+ 6·4	— 7·7	—1·3	— 0·4	+ 2·2	+1·8
20	+ 6·3	— 9·6	—3·3	— 6·4	+ 5·9	—0·5
21	+10·4	— 9·4	+1·0	— 6·4	+ 6·6	+0·2
27	+ 7·7	— 8·5	—0·8	— 6·4	+ 5·6	—0·8
Sum of Squares	9467·1	...	1222·3	2402·0	...	321·2

For Observations with the Altazimuth.

Day. 1852.		Longitude.			Ecliptic N.P.D.		
		B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
		″	″	″	″	″	″
Jan.	1	− 3·6	− 7·3	−10·9	− 9·2	+ 4·7	− 4·5
	2	+ 6·3	− 8·5	− 2·2	− 4·8	+ 5·1	+ 0·3
	3	+10·8	− 9·9	+ 0·9	− 7·4	+ 5·5	− 1·9
	4	+ 9·1	−11·8	− 2·7	− 1·3	+ 5·5	+ 4·2
	5	+13·2	−13·0	+ 0·2	− 4·3	+ 5·8	+ 1·5
	6	+14·1	−14·3	− 0·2	− 3·9	+ 5·8	+ 1·9
	7	+11·3	−14·7	− 3·4	− 6·1	+ 5·9	− 0·2
	8	+16·3	−14·8	+ 1·5	− 2·6	+ 5·5	+ 2·9
	9	+12·0	−14·5	− 2·5	− 7·6	+ 5·3	− 2·3
	10	+ 9·4	−15·3	− 5·9	− 5·0	+ 4·7	− 0·3
	11	+16·9	−16·2	+ 0·7	− 3·9	+ 3·7	− 0·2
	13	+24·5	−18·3	+ 6·2	− 1·2	+ 0·7	− 0·5
	15	+13·6	−16·2	− 2·6	+ 3·1	− 3·9	− 0·8
	16	+ 7·9	−11·8	− 3·9	+ 5·7	− 6·2	− 0·5
	17	+ 0·3	− 5·6	− 5·3	+ 7·5	− 7·5	0·0
	18	+ 0·9	+ 0·3	+ 1·2	+11·5	− 8·1	+ 3·4
	23	+ 0·2	0·0	+ 0·2	+ 5·1	− 3·1	+ 2·0
	25	+ 2·5	+ 0·5	+ 3·0	+ 3·9	+ 0·5	+ 4·4
	26	+ 1·9	+ 0·5	+ 2·4	− 2·1	+ 2·1	0·0
	27	+ 0·2	− 0·6	− 0·4	− 1·2	+ 3·2	+ 2·0
	28	− 0·1	− 2·7	− 2·8	− 5·3	+ 3·5	− 1·8
	29	+ 5·4	− 5·5	− 0·1	− 5·2	+ 5·0	− 0·2
	30	+ 9·0	− 8·4	+ 0·6	− 5·5	+ 5·1	− 0·4
Feb.	1	+11·8	−12·2	− 0·4	− 9·7	+ 6·3	− 3·4
	2	+17·4	−13·2	+ 4·2	− 3·8	+ 6·7	+ 2·9
	3	+14·2	−13·7	+ 0·5	− 8·7	+ 6·5	− 2·2
	5	+17·0	−16·5	+ 0·5	− 7·4	+ 5·5	− 1·9
	6	+12·8	−17·6	− 4·8	− 8·8	+ 4·5	− 4·3
	7	+15·4	−17·1	− 1·7	− 4·0	+ 3·1	− 0·9
	8	+14·5	−15·6	− 1·1	+ 0·8	+ 1·5	+ 2·3
	9	+11·1	−12·8	− 1·7	+ 0·1	0·0	+ 0·1
	10	+ 8·7	−11·0	− 2·3	+ 1·2	− 1·5	− 0·3
	11	+ 8·7	−10·5	− 1·8	+ 4·5	− 3·4	+ 1·1
	22	− 6·8	+ 2·9	− 3·9	− 1·4	+ 0·2	− 1·2
	25	− 0·1	− 1·0	− 1·1	− 2·6	+ 2·4	− 0·2

Day. 1852.		Longitude.			Ecliptic N.P.D.		
		B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
		"	"	"	"	"	"
Feb.	26	− 0·3	− 3·6	− 3·9	− 3·9	+ 2·8	− 1·1
	27	+ 8·0	− 6·6	+ 1·4	− 3·4	+ 3·1	− 0·3
	28	+11·6	− 9·5	+ 2·1	− 2·7	+ 4·1	+ 1·4
	29	+ 9·8	−11·6	− 1·8	− 2·6	+ 5·2	+ 2·6
March	1	+ 8·1	−12·8	− 4·7	− 8·7	+ 6·3	− 2·4
	2	+ 9·3	−12·6	− 3·3	− 8·7	+ 6·9	− 1·8
	3	+ 8·3	−11·5	− 3·2	− 8·6	+ 6·9	− 1·7
	4	+10·1	−13·5	− 3·4	− 9·5	+ 6·1	− 3·4
	5	+13·6	−14·8	− 1·2	− 8·3	+ 4·7	− 3·6
	6	+12·9	−15·8	− 2·9	− 7·9	+ 3·1	− 4·8
	7	+11·0	−14·3	− 3·3	− 6·1	+ 1·2	− 4·9
	14	0·0	+ 5·0	+ 5·0	+ 5·8	− 7·1	− 1·5
	22	− 7·3	+ 4·0	− 3·3	+ 0·6	+ 1·6	+ 2·2
	23	+ 0·7	+ 0·4	+ 1·1	+ 2·1	+ 2·0	+ 4·1
	25	+ 6·6	− 4·5	+ 2·1	− 0·7	+ 2·5	− 1·8
	26	+ 4·9	− 7·2	− 2·3	− 1·3	+ 3·0	+ 1·7
	27	+11·3	− 9·0	+ 2·3	− 2·3	+ 3·9	+ 1·6
	28	+18·2	−10·7	+ 7·5	− 6·9	+ 4·9	− 2·0
	30	+14·5	−13·1	+ 1·4	− 4·6	+ 7·7	+ 3·1
April	1	+14·9	−12·7	+ 2·2	−11·6	+ 8·1	− 3·5
	2	+11·8	−12·3	− 0·5	− 9·1	+ 7·0	− 2·1
	3	+10·8	−12·7	− 1·9	− 4·2	+ 5·6	+ 1·4
	4	+ 8·2	−12·9	− 4·7	− 5·7	+ 3·8	− 1·9
	5	+ 8·6	−13·0	− 4·4	− 4·8	+ 2·1	− 2·7
	8	+ 4·2	− 4·4	− 0·2	− 0·8	− 1·9	− 2·7
	9	− 0·5	− 1·6	− 2·1	+ 4·1	− 3·4	+ 0·7
	10	− 2·4	+ 0·4	− 2·0	+ 5·3	− 4·4	+ 0·9
	13	− 2·4	+ 7·6	+ 5·2	+ 3·1	− 4·0	− 0·9
	21	− 2·6	− 3·3	− 5·9	− 1·7	+ 2·9	+ 1·2
	22	+11·5	− 7·6	+ 3·9	+ 2·8	+ 3·4	+ 6·2
	24	+12·6	−10·9	+ 1·7	− 4·0	+ 4·7	+ 0·7
	25	+12·0	−11·9	+ 0·1	− 6·5	+ 5·8	− 0·7
	26	+11·8	−12·7	− 0·9	−11·7	+ 7·2	− 4·5
	27	+14·9	−14·2	+ 0·7	− 7·8	+ 8·0	+ 0·2
	29	+ 6·4	−15·1	− 8·7	−11·7	+ 8·2	− 3·5
May	1	+15·7	−12·0	+ 3·7	− 6·3	+ 5·4	− 0·9
	2	+ 8·9	−11·7	− 2·8	− 3·1	+ 3·8	+ 0·7

Day. 1852.		Longitude.			Ecliptic N.P.D.		
		B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
		″	″	″	″	″	″
May	3	+11·5	−12·7	− 1·2	+ 0·6	+ 2·2	+ 2·8
	4	+ 7·6	−13·8	− 6·2	− 0·9	+ 1·0	+ 0·1
	5	+11·9	−13·2	− 1·3	− 3·7	− 0·2	− 3·9
	10	+ 5·8	− 2·0	+ 3·8	+ 2·4	− 2·4	− 0·0
	22	+ 9·9	−13·5	− 3·6	+ 0·8	+ 4·0	+ 4·8
	29	+ 6·2	−12·9	− 6·7	+ 1·3	+ 1·5	+ 2·8
	31	+ 5·8	−11·0	− 5·2	− 0·3	− 1·5	− 1·8
June	1	+12·8	−12·0	+ 0·8	+ 0·3	− 2·6	− 2·3
	2	+ 6·1	−12·1	− 6·0	− 1·4	− 3·2	− 4·6
	4	+ 8·5	− 8·8	− 0·3	− 1·0	− 3·0	− 4·0
	10	+ 4·8	− 3·6	+ 1·2	− 9·2	+ 3·3	− 5·9
	11	+ 3·6	− 1·9	+ 1·7	− 5·9	+ 3·9	− 2·0
	19	+14·5	−16·7	− 2·2	− 4·5	+ 2·6	− 1·9
	21	+16·1	−19·1	− 3·0	− 0·7	+ 3·0	+ 2·3
	22	+13·0	−18·9	− 5·9	− 1·7	+ 2·8	+ 1·1
	23	+13·5	−18·0	− 4·5	− 0·1	+ 2·5	+ 2·4
	24	+11·1	−16·2	− 5·1	− 0·6	+ 1·7	+ 1·1
	26	+10·6	−12·4	− 1·8	+ 1·5	− 1·2	+ 0·3
	27	+ 4·0	−11·4	− 7·4	+ 1·1	− 2·8	− 1·7
	29	+ 6·5	−10·1	− 3·6	+ 4·7	− 5·8	− 1·1
	30	+11·4	− 8·8	+ 2·6	+ 9·3	− 6·2	+ 3·1
July	1	+ 6·0	− 7·8	− 1·8	+ 7·6	− 6·0	+ 1·6
	2	+ 4·9	− 6·7	− 1·8	+ 4·7	− 5·2	− 0·5
	3	+ 6·2	− 6·0	+ 0·2	+ 3·8	− 4·0	− 0·2
	4	+ 8·5	− 5·1	+ 3·4	+ 0·1	− 2·1	− 2·0
	5	+ 7·9	− 4·0	+ 3·9	− 1·9	0·0	− 1·9
	6	+ 3·9	− 2·7	+ 1·2	− 5·6	+ 1·4	− 4·2
	7	+ 0·8	− 2·0	− 1·2	− 7·2	+ 2·6	− 4·6
	8	− 0·6	− 1·6	− 2·2	− 6·0	+ 3·7	− 2·3
	9	+ 5·0	− 2·0	+ 3·0	− 9·4	+ 4·6	− 4·8
	10	+ 6·2	− 2·2	+ 4·0	− 6·3	+ 4·7	− 1·6
	20	+25·4	−27·6	− 2·2	+ 1·3	+ 3·2	+ 4·5
	21	+19·2	−23·5	− 4·3	+ 0·6	+ 2·8	+ 3·4
	22	+11·9	−18·7	− 6·8	+ 5·5	+ 2·5	+ 8·0
	23	+ 6·8	−14·1	− 7·3	+ 4·8	+ 1·7	+ 6·5
	24	+ 5·2	−11·4	− 6·2	+ 1·3	+ 0·6	+ 1·9
	27	+ 7·4	−10·4	− 3·0	+ 4·1	− 4·6	− 0·5

Day. 1852.		Longitude.			Ecliptic N.P.D.		
		B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
July	29	+12″·2	− 9″·8	+ 2″·4	+ 5″·6	− 6″·1	− 0″·5
	30	+11·3	−10·1	+ 1·2	+ 3·5	− 5·9	− 2·4
	31	+ 9·9	−10·3	− 0·4	+ 6·4	− 4·6	+ 1·8
Aug.	1	+10·0	−10·0	0·0	− 2·8	− 3·0	− 5·8
	2	+11·8	− 8·2	+ 3·6	+ 0·2	− 1·2	− 1·0
	3	+ 5·9	− 4·7	+ 1·2	− 4·5	+ 0·5	− 4·0
	4	+ 3·9	− 1·2	+ 2·7	− 5·2	+ 1·7	− 3·5
	5	− 1·6	+ 0·8	− 0·8	− 6·4	+ 2·6	− 3·8
	6	+ 4·1	+ 1·0	+ 5·1	− 6·6	+ 3·1	− 3·5
	7	+ 8·9	0·0	+ 8·9	− 7·6	+ 3·8	− 3·8
	8	+ 6·3	− 1·5	+ 4·8	− 8·6	+ 4·3	− 4·3
	9	+ 0·6	− 2·2	− 1·6	− 9·4	+ 5·1	− 4·3
	10	+ 3·7	− 2·9	+ 0·8	− 7·1	+ 5·5	− 1·6
	18	+35·4	−35·4	0·0	− 5·1	+ 4·3	− 0·8
	19	+25·9	−27·8	− 1·9	− 3·3	+ 3·5	+ 0·2
	21	+ 7·9	−12·9	− 5·0	+ 0·3	+ 2·4	+ 2·7
	22	+10·4	− 9·0	+ 1·4	+ 0·1	+ 1·2	+ 1·3
	25	+ 7·0	− 7·4	− 0·4	+ 2·8	− 3·2	− 0·4
	26	+11·3	− 8·8	+ 2·5	+ 6·0	− 3·6	+ 2·4
	27	+14·3	−11·1	+ 3·2	+ 3·1	− 3·3	− 0·2
	28	+17·6	−13·8	+ 3·8	+ 3·9	− 2·9	+ 1·0
	29	+13·7	−15·7	− 2·0	− 0·4	− 1·6	− 2·0
	30	+16·1	−15·6	+ 0·5	− 3·4	− 0·1	− 3·5
	31	+ 8·2	−12·6	− 4·4	− 4·9	+ 0·9	− 4·0
Sept.	1	+10·3	− 7·8	+ 2·5	− 2·7	+ 1·7	− 1·0
	2	+ 4·3	− 3·3	+ 1·0	− 4·6	+ 2·2	− 2·4
	3	+ 1·9	− 0·2	+ 1·7	− 6·2	+ 2·5	− 3·7
	4	+ 7·2	+ 0·6	+ 7·8	− 6·3	+ 2·9	− 3·4
	5	+ 5·9	− 1·0	+ 4·9	− 6·2	+ 4·0	− 2·2
	7	+ 7·0	− 6·1	+ 0·9	−10·1	+ 6·7	− 3·4
	8	+ 6·3	− 8·2	− 1·9	−12·2	+ 8·0	− 4·2
	11	+13·5	−19·8	− 6·3	−10·9	+ 9·3	− 1·6
	16	+37·0	−40·7	− 3·7	− 3·5	+ 2·1	− 1·4
	17	+33·9	−32·2	+ 1·7	− 3·3	+ 0·8	− 2·5
	18	+25·2	−22·6	+ 2·6	− 3·4	− 0·4	− 3·8
	20	+ 9·3	− 9·2	+ 0·1	+ 5·5	− 2·5	+ 3·0
	21	+ 9·5	− 6·1	+ 3·4	+ 0·7	− 3·3	− 2·6

Day. 1852.		Longitude.			Ecliptic N.P.D.		
		B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
		"	"	"	"	"	"
Sept.	22	+ 7·8	— 4·9	+ 2·9	+ 1·9	— 3·4	— 1·5
	23	+ 6·4	— 5·0	+ 1·4	+ 7·5	— 3·0	+ 4·5
	24	+ 8·6	— 6·6	+ 2·0	+ 2·4	— 2·1	+ 0·3
	25	+12·5	— 9·0	+ 3·5	+ 1·8	— 1·0	+ 0·8
	26	+13·2	—12·1	+ 1·1	+ 1·2	+ 0·4	+ 1·6
	28	+15·0	—15·2	— 0·2	— 6·2	+ 2·7	— 3·5
	29	+13·1	—13·7	— 0·6	— 4·9	+ 3·3	— 1·6
	30	+10·3	—10·6	— 0·3	— 7·9	+ 3·5	— 4·4
Oct.	2	+ 1·0	— 3·7	— 2·7	— 9·6	+ 3·9	— 5·7
	3	+ 2·1	— 2·7	— 0·6	— 8·5	+ 4·3	— 4·2
	4	+ 9·7	— 4·2	+ 5·5	— 8·4	+ 5·3	— 3·1
	5	+ 8·6	— 7·1	+ 1·5	— 8·1	+ 6·7	— 1·4
	6	+ 4·3	—11·7	— 7·4	—11·4	+ 8·4	— 3·0
	8	+11·1	—18·2	— 7·1	—10·0	+ 9·5	— 0·5
	11	+26·2	—26·8	— 0·6	—14·5	+ 5·3	— 9·2
	15	+41·1	—37·9	+ 3·2	— 7·4	— 3·7	—11·1
	17	+23·3	—22·6	+ 0·7	+ 4·6	— 6·3	— 1·7
	18	+ 7·6	—15·8	— 8·2	+ 4·8	— 7·0	— 2·2
	19	+ 7·2	—11·2	— 4·0	+ 4·5	— 6·9	— 2·4
	20	+ 6·3	— 8·9	— 2·6	+ 5·3	— 6·1	— 0·8
	21	— 7·9	— 7·7	+ 0·2	+ 5·1	— 4·7	+ 0·4
	22	+ 8·1	— 7·1	+ 1·0	+ 0·9	— 2·9	— 2·0
	23	+ 8·2	— 7·0	+ 1·2	— 0·1	— 1·0	— 1·1
	24	+ 6·5	— 6·9	— 0·4	— 1·9	+ 0·5	— 1·4
	25	+11·1	— 7·8	+ 3·3	— 0·6	+ 2·0	+ 1·4
	26	+10·9	— 9·3	+ 1·6	— 6·1	+ 3·2	— 2·9
	27	+11·4	—11·0	+ 0·4	— 6·2	+ 3·9	— 2·3
	28	+17·3	—12·0	+ 5·3	— 3·5	+ 4·1	+ 0·6
	29	+13·3	—11·5	+ 1·8	— 3·7	+ 4·0	+ 0·3
	30	+13·8	— 9·7	+ 4·1	— 0·7	+ 3·9	+ 3·2
	31	+10·6	— 7·4	+ 3·2	— 7·0	+ 4·1	— 2·9
Nov.	2	+10·2	— 6·3	+ 3·9	—11·1	+ 5·3	— 5·8
	3	+ 2·6	— 9·4	— 6·8	—10·4	+ 6·0	— 4·4
	5	+15·8	—17·8	— 2·0	—13·0	+ 6·8	— 6·2
	7	+16·3	—18·7	— 2·4	—11·3	+ 4·4	— 6·9
	16	+12·0	—12·5	— 0·5	+ 9·0	+ 7·4	+16·4
	17	+ 8·2	—10·6	— 2·4	+ 7·7	— 5·7	+ 2·0

Day. 1852.		Longitude.			Ecliptic N.P.D.		
		B—O.	H—B.	H—O.	B—O.	H—B.	H—O.
		"	"	"	"	"	"
Nov.	18	+ 7·5	−10·0	− 2·5	+ 3·5	− 4·0	− 0·5
	19	+ 4·7	− 9·8	− 5·1	+ 2·2	− 1·7	+ 0·5
	20	+14·3	− 9·7	+ 4·6	+ 0·2	+ 0·5	+ 0·7
	24	+ 7·1	− 6·5	+ 0·6	− 6·5	+ 5·0	− 1·5
	26	+11·2	− 8·6	+ 2·6	− 7·5	+ 4·9	− 2·6
	27	+12·0	− 9·3	+ 2·7	− 6·3	+ 4·6	− 1·7
	28	+10·6	− 9·3	+ 1·3	− 6·1	+ 4·3	− 1·8
	29	+ 2·5	− 7·7	− 5·2	− 3·2	+ 3·9	+ 0·7
	30	− 4·5	− 6·4	−10·9	− 5·1	+ 3·8	− 1·3
Dec.	1	+12·7	− 5·8	+ 6·9	− 2·0	+ 3·8	+ 1·8
	2	+ 9·8	− 7·4	+ 2·4	− 4·6	+ 4·3	− 0·3
	3	+10·5	−10·7	− 0·2	− 6·0	+ 4·1	− 1·9
	15	+10·7	− 7·6	+ 3·1	+ 6·6	− 4·4	+ 2·2
	17	+10·6	− 6·6	+ 4·0	+ 1·9	+ 0·2	+ 2·1
	18	+ 5·7	− 7·7	− 2·0	− 3·5	+ 2·2	− 1·3
	19	+ 4·8	− 8·7	− 3·9	− 2·8	+ 4·1	+ 1·3
	20	+11·3	− 9·6	+ 1·7	− 5·7	+ 5·9	+ 0·2
	21	+16·2	− 9·4	+ 6·8	− 3·5	+ 6·6	+ 3·1
	22	+ 3·2	− 7·8	− 4·6	− 4·0	+ 7·3	+ 3·3
	24	+ 6·6	− 5·9	+ 0·7	− 9·2	+ 7·1	− 2·1
	25	+ 5·6	− 6·5	− 0·9	− 8·2	+ 6·5	− 1·7
	26	+ 7·7	− 7·7	0·0	− 5·6	+ 6·2	+ 0·6
	27	+10·1	− 8·5	+ 1·6	− 8·3	+ 5·6	− 2·7
Sum of Squares		28331·4	...	2647·0	7024·0	...	1994·4

"Thus it appears that, in the meridional observations, the sum of squares of residual errors in longitude is reduced by Hansen's tables to nearly $\frac{1}{8}$ of the sum of squares for Burckhardt's tables, and that for latitude is reduced to nearly $\frac{1}{8}$ part. And for the extra-meridional observations, the sum of squares of residual errors in longitude is reduced to nearly $\frac{1}{11}$ part; and that for latitude is reduced to nearly $\frac{1}{4}$ part. (All these denominators are a trifle too large.) Probably in no recorded instance has practical science ever advanced so far in accuracy by a single stride.

"It must, however, be borne in mind that no similar comparison has been made either of Damoiseau's tables or of tables formed from Plana's theory, adapted to corrected values of the elliptic constants, and that it is not improbable that they might

be found to occupy a very high place as regards accuracy. The comparisons in the *Greenwich Lunar Reductions* of Plana's theory, based on Damoiseau's and Plana's elliptic constants, with observations, show that it is greatly superior to Burckhardt's.

"Besides the conclusion as to the general accuracy of Hansen's tables, two or three other conclusions will, perhaps, properly follow from this *exposé*.

"1. The policy of the establishment and vigorous employment of the altazimuth at the Royal Observatory is well supported, I conceive, by these results. In almost all cases, the largest errors of Burckhardt's places are obtained in these parts of the moon's orbit which could not be touched by the meridional instrument, and they became the crucial points for any new theory. These errors are faithfully corrected by Hansen's tables.

"2. This comparison generally furnishes a remarkable proof, if any were wanted, of the enormous value of the system of keeping up the reduction of observations to the greatest accuracy that is permitted by existing elements.

"3. So long as observations are confined to the meridional half of the moon's orbit, the variation and the parallactic equation are always combined in the same way; but in that part which is touched only by the altazimuth, they are combined in the opposite way. The equally effective correction in both parts of the orbit exhibited by Hansen's tables proves (among other things) that his parallactic equation must be very correct, and therefore that his assumption as to the sun's distance must be nearly accurate.

"It was generally understood between Mr. Hind and myself that the comparison of tables should extend from the introduction of the altazimuth in 1847 to the latest convenient year. I need not say that I wait with intense anxiety the results of the comparison for other years.

"*Royal Observatory, Greenwich, April* 29, 1859."

On the Cause of Irregularity in the Rate of a Chronometer.

By A. Wackerbarth, Esq.

"A cause of faulty performance in a chronometer has lately been made evident to me, which it had never previously occurred to me to think of; and, as it is possible it may have escaped the observation of others also, I think it may perhaps be worth while to trouble the Society with a notice of it. Some years ago I purchased a gold pocket chronometer; it had been originally made by the elder Frodsham, and was put in order for me by the present Mr. Frodsham, who, however,

stated that to go quite satisfactorily it would require some further repairs, but it would be well to try it first. The performance was, however, very unsatisfactory, and Mr. Frodsham thought that repairs to the amount of about 15*l.* would be necessary in order to get it to keep time satisfactorily. Söderburg, of Stockholm, made a higher estimate for repairs. Not being able to afford conveniently to repair the chronometer, it lay unemployed in its box till a watchmaker in this town, named Sedström, called on me and asked to see it. On looking at it, he told me that the instrument was to all appearance very beautifully made; that he thought there was not very much the matter with it, and asked whether he might take it home and look at it; adding that he had made three chronometers in his lifetime, of which no complaint had been made. I consented, and in a few days he called on me again, stating that he had discovered sufficient to account for considerable irregularity of rate; for, on taking out the balance-wheel, which, as usual, was loaded with steel screws, he laid it down upon his working board just where there happened at the moment to be some very fine steel filings, and observed that the steel-dust immediately jumped up and placed itself upon the screws, thus indicating that they had become magnetic, and strongly so; and this, of course, was a sufficient cause of perturbation. He offered to put in new screws, exactly weighed, and remedy some few other slight effects of wear-and-tear, and said that the whole would be but a trifling expense. He accordingly had the chronometer during the summer and autumn, while I was at the sea-side; and I have now had it home some two or three months, and it now appears to keep its rate pretty steadily, and the whole expense was no more than thirty rix-dollars (= 33*s.* 4*d.* English money). I have thought this worth mentioning, for it may occasionally happen that one goes to heavy expense upon a chronometer when there is really nothing more the matter with it than there was in this instance, when all was set to rights for not very many shillings.

"*Upsala, March 20th*, 1859."

The Astronomer Royal, while expressing his entire confidence in the carefulness and accuracy of Mr. Wackerbarth as an observer, remarked that the circumstance to which that gentleman's communication referred had not previously escaped attention. It has been found that, with very rare exceptions, the effect of the earth's magnetism, which is much greater than that produced by local attraction, is wholly insensible.

In a letter from Mr. Bond to Mr. Carrington, dated April 4, there occur the following notes and remarks :—

"We obtained a photograph of the comet (Donati's) on September 28th. On October 5th it was visible in the daytime.

"In the list of comets at page 155 of the Annual Report of your Council, the one discovered by Mr. H. P. Tuttle at this observatory on September 5, 1858, has been omitted ;* and on page 142 it should have been stated that the same comet-hunter found Donati's independently on June 28, the day before Parkhurst.

"A nebula, which is to be found neither in Sir J. Herschel's Catalogue nor D'Arrest's, was detected by Mr. Tuttle on the 5th of February in R.A. $3^h\,21^m$, Decl. $+\,30°\,55'$. It follows a star of the 9·10 mag. by 6 seconds, and is 2′ north of it. It is barely visible in a telescope of 3 inches aperture."

After the conclusion of the reading of the papers presented to the Society, the President requested that the Astronomer Royal would occupy the chair for a short time while he offered to the meeting, according to preceding announcement, some remarks on Donati's comet in connexion with the theories that had been devised to account for the physical peculiarities of such bodies, and with especial reference to Bessel's theory of opposite polarities.

He produced diagrams of some of the most celebrated comets, such as those of 1744, 1769, 1811, 1835 (Halley's), and 1858 (Donati's), and showed that the appearances which they exhibited when approaching their perihelion were so similar that generally, in any attempt to form an adequate theory for the phenomena exhibited, any one of them might be taken, but that the excellent drawings and elaborate descriptions of Donati's comet rendered it a much more interesting subject of study than any which preceded it. The phenomena consist chiefly of the disturbance of the surface of the nucleus, and the violent emission of jets of luminous matter in the direction of the sun, followed by the rapid turning round of these jets in the opposite direction to form the tail, and afterwards of the formation of a succession of luminous envelopes, which are evidently the result of a more general and quietly-developed emission from that portion of the whole surface of the nucleus which is turned towards the sun.

He then gave a short account of the theory first propounded

* For an account of the discovery of this comet, and a statement of the elements of its orbit, see *Monthly Notices*, vol. xviii. p. 303.—Ed.

by Olbers in the twenty-fifth volume of the *Monatliche Correspondenz*, and of the attempt of Brandc to give a mathematical development to the theory, under certain restrictions, contained in the twenty-ninth volume of that work. It was not, however, till Bessel had had the opportunity of studying minutely the phenomena of Halley's comet, on its return in 1835, that this theory received anything like an adequate development. This comet, from the comparative simplicity of the phenomena exhibited, was better adapted than some of the other large comets for the discovery of their general principle; and Bessel, who was fortunate enough to observe the complete oscillation of one of the jets, of which he had witnessed the original emission, assumed that this could arise only from the action of some directive force or polarity, similar to that which actuated the magnetic needle, and directed to the sun as the origin or centre of force; and since the existence of one kind of polarity always implies that of the opposite kind, he was enabled to show how the sun, which originally attracts with great violence towards himself the particles streaming from the nucleus, afterwards, when they have moved through a certain space filled with matter of opposite polarity, might be as violently repelled by him, and driven out in an hyperbolic orbit, according to the usual law of central forces varying inversely as the square of the distance.

The President, in alluding to Bessel's labours, took the opportunity to impress upon the meeting the necessity of *continuous* observations of the phenomena of cometary bodies as absolutely necessary for the deduction of the law by which they are governed. Thus Bessel, on the night of October 12, 1835, watched the comet from sunset to sunrise, and was rewarded by seeing the luminous jet in that time describe an arc of 36°. In like manner Chacornac, one of the most successful observers of the phenomena of Donati's comet, watched it frequently from the time of its attaining a sufficient height above the horizon in the early morning till daylight rendered it invisible.

Before concluding, the President drew attention to the extreme importance of drawing at the time, and with all possible accuracy, the most striking of the appearances of the comet observed. This has been done by several astronomers in the case of Donati's comet, but was done very imperfectly indeed (as is complained of by Olbers) for the greater number of comets preceding that of 1811, with the remarkable exception of that of 1744, which was admirably described and graphically represented by Heinsius in a distinct account drawn up by him, and which, unfortunately, appears to be very scarce in this country.

RECENT PUBLICATIONS.

Sur l'Intégration des Equations Différentielles dans le Problèmes de Mécanique : — Application de la Méthode de M. Hamilton au Calcul des Perturbations de Jupiter. Par M. Houel, Professeur de Mathématiques au Lycée à Alençon. Paris, 1855.

In the first of these papers the author has developed the general method of dynamics proposed by Sir William Hamilton in two papers published in the *Philosophical Transactions* for 1834 and 1835, taking into consideration the subsequent extensions by Jacobi. In the course of his investigation the author has demonstrated several general theorems of Celestial Mechanics, including the theorem of the invariability of the mean distances, taking into account all the terms of the order of the cube of the disturbing force.

The second paper contains an application of Sir William Hamilton's method to the computation of the mutual perturbations of *Jupiter* and *Saturn*. In the course of his labours the author has obtained several important corrections to the numerical results relative to the same subject, given by M. Pontécoulant in his *Théorie Analytique du Système du Monde.*

Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the Year 1857. Under the Superintendence of Manuel J. Johnson, M.A., Radcliffe Observer, vol. xviii., Oxford, 1859.

This volume, the sheets of which were passing through the press at the time of the death of its lamented author, contains the usual observations with the transit instrument and meridian circle. These are followed by a catalogue of the stars observed in the year 1857, showing the result of every observation. The number of stars in the catalogue is 1398. The volume closes with a very full account of the meteorological operations executed at the observatory in the course of the year.

Almanaque Nautico para 1860, *Calculado de orden de S. M. en el Observatorio de Marina de la Ciudad de San Fernando.* Cadiz, 1858.

This volume contains two useful graphic representations of the path of the moon's shadow on the occasion of the total

eclipse of the sun which is to occur on the 18th of July, 1860. The first of these relates to the track of the shadow on the surface of the earth in general; the second is specially designed to exhibit the circumstances of its passage over the Spanish territory.

Note sur la Comète de Donati. Par M. le Professeur E. Plantamour. (Tiré des Archives des Sciences de la Bibliothèque Universelle de Genève.)

This pamphlet, embodying Professor Plantamour's observations of Donati's comet, forms a useful accession to the materials of a similar character which have already been published. The determinations of the position of the comet extend from August 28 to October 18. There are some valuable drawings of the physical phenomena exhibited by the comet during the period of its greatest splendour.

Professor Mädler has recently published a pamphlet containing some details relative to the total eclipse of the sun which is to occur on the 18th of July, 1860. The circumstances of the eclipse are rigorously calculated for 112 positions in Spain, Portugal, and Algiers, the places of the sun and moon being derived from an ephemeris computed by Prof. Wolfers from the Solar Tables of Hansen and Olufsen, and the Lunar Tables of Hansen. The author has given a graphic representation of the same results, and in another figure he has exhibited the aspect which the heavens may be expected to offer at the city of Saragossa during the totality of the eclipse. Besides the planets *Mercury*, *Venus*, *Jupiter*, and *Saturn*, it contains all the stars of the first two magnitudes, and several of the third magnitude which will then be above the horizon. The four planets will be situate very near to the sun in the form of a rhomboid. The stars *Regulus*, *Procyon*, *Sirius*, α and β *Orionis*, *Castor*, *Pollux*, *Capella*, *Aldebaran*, *Vega*, *Arcturus*, and *Spica Virginis*, will all be above the horizon. Professor Mädler's paper contains some useful suggestions for observing the eclipse.

MISCELLANEOUS INTELLIGENCE.

New Comet.

A new comet was discovered on the 2nd of April at Venice, by M. Tempel. It is a faint telescopic object. The following elements of its orbit, founded on observations made at Vienna, April 10; Berlin, April 14; and Altona, April 18; have been computed by M. Pape,—

T	.....	May 29,1830, M.B.T.
π	.....	77° 38′ 8″ } App. Eq., April 14.
Ω	.....	359 16 1 }
i	.....	87 20 51
Log q	.....	9·314042

Motion Retrograde.

These elements do not offer a resemblance to those of any former comet.

M. Pape has also computed an ephemeris from the foregoing results, extending from April 18 to May 16, with two additional places for June 1 and June 30. The following are the places from May 6:—

1830.	R.A.	Decl.	log r	log Δ
May 6	90° 30′·0	+37° 20′·4		
7	89 45·8	35 35·4		
8	89 2·4	33 51·9	9·8337	9·8824
9	88 18·9	32 9·8		
10	87 35·3	30 29·6		
11	86 51·4	28 51·1		
12	86 7·0	27 14·5	9·7645	9·9036
13	85 21·7	25 39·9		
14	84 34·9	24 7·4		
15	83 46·7	22 37·1		
16	82 56·6	21 9·2	9·6785	9·9281
June 1	60 20·8	13 34·4	9·3811	0·0444
30	50 19·3	51 54·4	9·9789	0·1102

The increase in the intensity of light shows the following results:—

April 18 = 1; April 30 = 1·8; May 16 = 4·2; June 1 = 9·7; June 30 = 0·5.

Biela's Comet.

In No. 120 of Gould's *Astronomical Journal* Professor Hubbard has determined the elements of the two nuclei of this comet for the perihelion passage of the present year, and has computed an ephemeris of their positions for every fourth day, extending from 1859, March 23, to July 21. Mr. Hind, as Superintendent of the *Nautical Almanac*, has published an extension of this ephemeris, giving the places for every day from April 20 to July 24. Copies of Mr. Hind's circular have been forwarded to the various private observatories throughout the country.

The following letter from Mr. Maclear to Admiral Smyth contains some further intelligence respecting Donati's comet:—

"At page 67 of No. 2, vol. xix. of the Royal Astronomical Society's *Notices*, it is stated that Donati's Comet was first seen at Rio de Janeiro, on October 7; Buenos Ayres, 7; Loando (W. Africa), 7; Cape of Good Hope, 11.

"Omitting Loando, for the geographical reason of that station being some 24 degrees to the north of the Cape, the Comet should have been seen at the other stations about the same date, nearly, weather permitting. I give from our Meteorological Journal the state of the sky at the evening Göttingen mean times of observation, viz. at Cape mean times $5^h\ 34^m$ and $9^h\ 34^m$ P.M.; as follows:—

1858.	$5^h\ 34^m$	$9^h\ 34^m$	
October 7	9	7	
8	9	7	Light rain.
9	3	4	Light rain.
10	6	3	Light rain.
11	1	1	

"The number 9 implies that nine-tenths of the visible hemisphere were clouded at the time of observation; 7, that seven-tenths were clouded, &c. &c.; from which the state of the sky between 5^h and 9^h may be inferred.

"It is said to have been seen at Green Point for a few minutes on the 9th, through a break in the clouds.

"On the 10th I saw the head while pointing the telescope, but it was covered by a cloud immediately after; and I remained at the telescope for about an hour, hoping for an opening. The weather cleared up on the 11th.

"The numbers for the 7th and 8th will explain why it could not be seen on those evenings.

"We have pursued it through a considerable segment of the orbit, having hauled off only a fortnight back!

"The number of stars compared with it amount to about 120.

"*Cape of Good Hope, 1859, March 19.*"

Sir William Keith Murray has recently forwarded to the Society a printed description of the observatory erected by him a few years ago at Ochtertyre, Perthshire. He has also sent along with this description a set of plans, which serve to give a clear idea of the different parts of the building. It is generally known to astronomers that Sir W. Keith Murray possesses an equatoreally mounted refractor of 9 inches clear aperture, by Messrs. Cooke and Sons of York. Results indicative of the favourable performance of this instrument have been published from time to time in the *Monthly Notices*.

Captain Shea continues to record his observations of the solar spots, and to transmit the results to the Society; he states that his observations now extend over a period of twelve years.

In No. 1182 of the *Astronomische Nachrichten* M. Schwabe has given the following results of his observations of the solar spots in the year 1858:—

Month.	Number of Groups.	Days on which no Spots were seen.	Days of Observation.
January	13	0	24
February	11	0	27
March	13	0	30
April	15	0	30
May	12	0	30
June	15	0	30
July	14	0	29
August	22	0	31
September	20	0	30
October	16	0	30
November	21	0	26
December	16	0	18

Five of the principal groups were visible to the naked eye, namely, those of June 4, August 28, October 16, November 9, and November 29. The group of November 9 had a diameter of 65″·7 — 68″·3. The diameter of the group observed on the 29th of the same month amounted to 205″·8. The greater number of spots appeared in the southern hemisphere. The two most extensive groups were those of March 17 (diameter from east to west 243″·8) and September 30 (diameter 321″·3, also from east to west).

Equatoreal Instrument for Sale.

The object-glass was the property of the late Dr. Ritchie, and was examined and an account of its performance given by the late Rev. Samuel King, in vol. v. of the *Monthly Notices* of the Society. The aperture of the object-glass is about $7\frac{3}{4}$ inches; it is furnished with several astronomical and one terrestrial eyepiece, a finder, illuminating apparatus, and parallel line micrometer.

The mounting is of the Frauenhofer kind, with circles reading by verniers to one second of time and ten seconds of declination, and horary motion by clockwork. The entire mounting was prepared by Troughton and Simms.

This instrument may be seen at the observatory of the late A. K. Killmister, Esq., at Leek, Staffordshire; and any further information obtained either from Messrs. Troughton and Simms, or G. R. Killmister, Esq., West Bank, near Macclesfield.

The price asked is 250*l.*, less than half its original cost.

Mr. Cayley sends the following Errata in his paper "On the Development of the Disturbing Function in the Lunar Theory," (Roy. Ast. Soc. Mem. vol. xxvii.)

Page 75	*in*	Ω_9	*for*	$C_c^{i'} + C_s^{i'}$	*read*	$C_c^{i'} - C_s^{i'}$	
— 76	—	Ω_1	,,	$\frac{231}{48}$	,,	$\frac{77}{16}$ [same in least terms].	
— —	—	—	,,	$\frac{9}{4}e'^2 + \frac{7}{4}e'^3$	,,	$\frac{9}{4}e'^2 + \frac{7}{4}e'^4$	
— 77	—	Ω_2	,,	correction	,,	evection	
— —	—	Ω_2	,,	$-3e + \frac{13}{8}e^3 - \frac{5}{192}e^5$	,,	$-\frac{3}{2} + \frac{13}{8}e^2 + \frac{5}{192}e^4$	
— 78	—	Ω_3	,,	$-3e + \frac{13}{8}e^3 - \frac{5}{192}e^5$	,,	$-\frac{3}{2} + \frac{13}{8}e^2 + \frac{5}{192}e^4$	
— 80	—	Ω_5	,,	$-3e + \frac{13}{8}e^3 - \frac{5}{192}e^5$	,,	$-\frac{3}{2} + \frac{13}{8}e^2 + \frac{5}{192}e^4$	
— 78	—	Ω_3	,,	$1 + \frac{3}{2}e'^2 + \frac{15}{8}e'^4 - \frac{35}{16}e'^6$	,,	$1 + \frac{3}{2}e'^2 + \frac{15}{8}e'^4 + \frac{35}{16}e'^6$	
— 79	—	Ω_4	,,	$-\frac{25}{385}e'^4$	,,	$-\frac{25}{384}e'^4$	
— 89 arg^t. L. 24			,,	$2\tau - \xi - \xi'$	,,	$2\tau - 2\xi - \xi'$	
— — — P. 45			,,	$2\xi - \varphi - \varphi'$	,,	$2\tau - 2\varphi - \varphi'$	
— 91	—	L. 87	,,	$2\tau + \xi + \xi' + 2\pi$	,,	$2\tau + \xi + \xi' - 2\pi$	
— 93	—	L. 128	Bracket should extend over $3g - g'$ viz., in effect *for*	$3g - g'$	*read*	$3g - g' + 3\mho - 3\mho'$.	

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *May* 13, 1859. No. 7.

Rev. R. MAIN, President, in the Chair.

Conrad Pinches, Esq., Ph. D., Westminster Road, Kennington;
Wm. Davies, Esq., 3 Corbet Court, Gracechurch Street;
Wm. Thomas, Esq., Trinity College, Dublin;
Thos. Gaunt, Esq., Wellington Place, Stoke Newington; and
Samuel Kinns, Esq., Shaftesbury House, Highbury New Park,

were balloted for and duly elected Fellows of the Society.

Note from the Astronomer Royal.

"The Superintendant of the *Nautical Almanac*, always anxious for the accuracy of the numbers which issue from his department, has pointed out that one of the numbers in the comparison of Burckhardt's and Hansen's Tables, *Monthly Notices*, 1859, April 8, has been taken with a wrong sign. Page 220, November 16, last column but one (H — B, Ecliptic N.P.D.) *for* + 7·4 *read* — 7·4.

"This reduces the corresponding value of H — O from + 16·4 to + 1·6. And thus the largest apparent error of Hansen's Tables is removed.

"The sum of squares of errors, Ecliptic N.P.D., H — O, is reduced from 1994·4 to 1728·0.

"*Royal Observatory, Greenwich, 1859, May 31.*"

Notice of Traces of Eruptive Action in the Moon.

By the Rev. T. W. Webb.

The inquiry as to the continuance of volcanic or explosive action on the surface of the moon must be admitted to be a very interesting one. Astronomers are generally agreed as to its entire cessation on any conspicuous scale; but this would not necessarily infer the impossibility, or even improbability, of minor eruptions, which might still continue to result from a diminished but not wholly extinguished force. Till the publication of the labours of Beer and Mädler the necessary data for the determination of the question were very imperfect; and since that time the general impression would seem to be adverse to the idea of any physical change. Before, however, it is entirely acquiesced in, it may be well to see whether any evidence of an opposite nature exists. Want of leisure has hitherto prevented me from entering upon the subject in any other than the most incidental manner; but I would request permission to direct attention to one or two regions where an accurate investigation might be desirable.

One of these is the spot named *Cichus*, near the south extremity of the *Mare Nubium*. Here, many years ago, in comparing Schröter's drawings with the moon, I was struck with the apparent enlargement of the small crater which has defaced one side of the ring. On procuring the map of Beer and Mädler I found that they had also seen it enlarged. Could we, in this instance, depend upon the older drawings, we might reasonably infer the probability of a change since the year 1792. Schröter was, undoubtedly, a coarse draftsman, but still he was faithful and careful; nor does there seem any appearance, but the reverse, that his designs were copied from one another to save trouble; if not, the agreement of three separate figures seems fair evidence that this little crater was not then of its present magnitude.

The second case seems more satisfactory. It is that of the spot *Messier*, in the *Mare Fœcunditatis*, where two small craters lie side by side at the end of two parallel white streaks, resembling the divided tail of a comet. In consequence of an observation of Schröter, Beer and Mädler directed their attention especially to this region, and examined it more than 300 times between the years 1829 and 1837, with the result that the two craters were perfectly and singularly alike in size, shape, height of ring, depth of cavity, and even the position of some peaks upon the rings. I cannot say that I have ever been able to obtain a perfectly distinct view of this spot, from atmospheric causes; but every sight I have had of it has strengthened my impression that this remarkable similarity no longer exists. I first noticed a difference in the aspect of the two craters, 1855, Nov. 14. Since that time, in the occasional use of four instruments of very different size and power (one, an

object-glass of 5½ inches by Alvan Clark, now in my possession), I have invariably felt convinced that neither in size, form, or depth, are those two craters any longer precisely similar, but that, on the contrary, under suitable angles of illumination, such as must have repeatedly occurred during the 300 examinations by Beer and Mädler, the difference between them is obviously so great as to indicate some permanent alteration in the surface during the space of twenty years.

May 12, 1859.

Sur la Variation Séculaire du Moyen Mouvement de la Lune. Par M. de Pontécoulant.

(*Communicated by the President.*)

"Dans la théorie de la Lune qui forme le quatrième volume de ma *Théorie Analytique du Système du Monde*,* j'avais adopté pour l'expression analytique du coefficient de *l'inégalité séculaire* de la longitude de la Lune, la formule trouvée par M. Plana dans son important ouvrage sur la même Théorie, mais depuis cette époque j'ai refait moi-même le calcul des différents termes qui composent cette expression, et j'ai trouvé, en négligeant les quantités dépendantes de l'excentricité et de l'inclinaison, pour déterminer l'inégalité dont il s'agit, l'équation suivante:—

$$\frac{dn}{n\,dt} = \frac{e'\,de'}{dt}\left(-3m^2+\frac{2187}{64}m^4+\frac{4455}{32}m^5+\frac{480481}{768}m^6+\frac{10244539}{576}m^7\right).$$

"Cette valeur coincide avec celle de M. Plana jusqu'aux termes de l'ordre m^5 inclusivement et les différences que l'on remarque dans les coefficients des termes dépendants des puissances de m^6 et m^7, tiennent sans doute à quelques incorrections qui se seront glissées dans la suite de ces longs calculs et qu'il sera facile de faire disparaître en les soumettant à une nouvelle révision. Mais si l'on compare l'expression précédente à celle donnée par M. Adams dans le No. du Compte-Rendu Mensuel des Travaux de la *Société Royale Astronomique* du 8 Avril, 1859 (p. 207), on voit que la discordance se fait sentir dès les termes de l'ordre m^4, ce qui provient de la considération de nouveaux termes que M. Adams a cru devoir introduire dans les formules analytiques d'où cette expression est déduite et auxquels on n'avait point en égard jusqu'ici. Or l'influence de ces termes est considérable puis qu'elle ne va à rien moins qu'à réduire à 5″·7, l'accélération du moyen mouvement lunaire dans un siècle, c'est à dire, *à la moitié* de la valeur à-peu-près

* *Théorie Analytique du Système du Monde*, vol. iv. p. 645.

qu'on était habitué à lui attribuer jusqu'à présent et à détruire, par conséquent, l'admirable accord qui semblait exister sur ce point entre la théorie et l'observation. Cette conséquence seule paraîtrait déjà rendre *très problématique* l'existence des nouveaux termes introduits par M. Adams, et il est, en effet, facile de démontrer, par une analyse très simple, que l'intégration des formules différentielles du mouvement lunaire ne produit dans l'expression de *l'inégalité séculaire* du mouvement moyen aucun terme différent de ceux qu'on a été accoutumé à y considérer jusqu'ici. J'aurai l'honneur d'adresser prochainement à l'Académie une note détaillée sur cet objet, et j'ose espérer qu'elle voudra bien, attendu l'importance de la question, lui donner une place dans le précieux recueil de ses mémoires, mais j'ai cru qu'il était indispensable de ne pas laisser passer, sans en faire l'objet d'une réclamation immédiate, une assertion qui aurait pour conséquence de jeter du doute sur l'un des plus remarquables résultats de la théorie du système du monde.

"*Paris, May* 28, 1859."

Extract of a Letter from Prof. Hansen to the Astronomer Royal, dated Gotha, May 31, 1859.

"Delaunay's Säcularänderung der mittleren Mondlänge muss ich entschieden für unrichtig halten. Ich habe folgende drei Resultate durch die Theorie erhalten:—

(1) + 11″·93, Ast. Nach. No. 443,
(2) + 11″·47, ... No. 597,
(3) + 12″·120, in den Mondtafeln angewandt.

"Die Bestimmungen (1) und (3) sind durch zwei *total von einander verschiedene* Methoden erlangt, die ich in der Explication der Berechnung der Mondstörungen, die ich jetzt in Arbeit habe, erklären werde. Die nahe Uebereinstimmung verbürgt die Richtigkeit meiner Methoden und Rechnung. Die Differenz ist nur 0″·25.

"Die Bestimmung (2) ist nach derselben Methode wie (3) berechnet, aber ich hatte bei der Berechnung von (2) zu wenige Glieder hinzugezogen, daher rührt die grössere Differenz.

"Worin der Fehler von Delaunay liegt, kann ich in diesem Augenblick nicht sagen, aber man kann als nicht unwahrscheinlich annehmen, dass bei der Entwickelung der Mondstörungen nach den Potenzen von m, Glieder, die mit *sehr hohen* Potenzen multiplicirt sind merklich werden können.

"Ueberhaupt hat man ja gar keinen Beweis von der Convergenz dieser Reihen, und sie müssen nothwendig bei ver-

grössertem Werthe von m divergiren. Ich habe bekanntlich diese Art der Entwickelung gar nicht angewandt, sondern von Annäherung zu Annäherung die erhaltenen Störungen in die Gleichungen substituirt. Dieses Verfahren habe ich so lange fortgesetzt, bis das Resultat der letzten Annäherung bis auf kleine Bruchtheile von Secunden dem Resultat der vorletzten Annäherung gleich wurde. Ich habe dazu nur 12 oder 13 Annäherungen nöthig gehabt."

Results of the Observations of Small Planets, made at the Royal Observatory, Greenwich, in the month of April, 1859.

(*Communicated by the Astronomer Royal.*)

Astræa (5).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, April 15	10 0 30·0	11 34 36·27	80 29 56·18
21	9 35 27·7	11 33 9·18	80 20 52·40

Flora (8).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, April 21	14 1 42·3	16 0 7·54	102 18 27·00

Metis (9).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, April 4	13 56 19·9	14 47 42·79	100 46 57·58
6	13 47 0·7	14 46 15·17	100 41 21·96
13	13 13 46·2	14 40 31·16	100 20 22·62
15	13 4 7·3	14 38 43·79	100 14 3·47

Irene (14).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, April 5	11 40 36·5	12 35 33·65	.....
6	11 35 50·4	12 34 43·41	76 3 15·56

Calliope (22).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, April 6	11 12 50·3	12 11 39·49	71 35 47·99
9	10 58 43·7	12 9 20·21	71 34 43·92

Themis (24).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, April 6	12 50 55·6	101 5 6·93
7	12 46 15·8	101 1 12·28

Bellona (28).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, April 6	13 54 21·5	92 46 14·98
21	12 44 53·1	91 13 46·98

Europa (52).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, April 21	14 3 41·3	100 27 35·69

Observations of the Occultation of Saturn by the Moon, made at the Royal Observatory, Greenwich, 1859, May 8.

(*Communicated by the Astronomer Royal.*)

Phenomenon.	Greenwich Mean Solar Time of Obs.						
	Hour and Min.	Seconds observed by					
		M.	G.	D.	E.	C.	L.
	h m	s	s	s	s	s	s
First Contact with Ring	8 18	55·3	59·0		55·6	58·9	
First Contact with Ball	8 18				59·6		
Total Disapp. of Ball	8 19	33·2		27·6	33·5	32·4	
Total Disapp. of Ring	8 19	42·4	41·5	42·6	42·9	42·2	
First Reapp. of Ring	9 17	14·0					
First Reapp. of Ball	9 17	32·9		30·3			
Last Contact with Ball	9 17			55·3		52·3	
Last Contact with Ring	9 18	21·8	21·0	22·2	20·9	20·8	24·8

Notes by Mr. Main [M.].—The instrument used was the Sheepshanks' equatoreal; for the disappearance the eye-piece used was No. 4 with a high power, probably about 300; for the reappearance No. 1 was used, power about 100. The only points of interest noticed were, first, that at the disappearance

when the moon's edge (unenlightened) was passing over the shadows lying on the apparently upper portion of the ring behind the ball, a curious flickering kind of light was produced as if disappearing and reappearing. Secondly, that at the reappearance of the planet, the faintness of its light as compared with that of the moon was very striking, and this might perhaps produce a trifling error in the observation of the first reappearance, which was however considered to be tolerably accurate. Mr. Main also observed the disappearance of a star of the 8th magnitude, which took place accurately at $9^h\ 11^m\ 36^s{\cdot}9$ Greenwich Mean Solar Time; it was afterwards ascertained to be identical with one which is numbered 17237–8 in Lalande's Catalogue.

By Mr. Glaisher [G.].—With the 39-inch achromatic telescope and a solar chronometer. The power was very low (only about 30) and the planet very faint; little confidence was felt in the observation of the first disappearance, and it was considered likely that it was fully a second too late. It was impossible with so low a power to detect the first reappearance at the bright limb. The observations of final disappearance and reappearance were considered good. The interval of time after emersion before the planet was distingnished with the naked eye was remarkable, and amounted to at least ten minutes.

By Mr. Dunkin [D].—With the 46-inch achromatic telescope in the Octagon Room, and a sidereal chronometer. At the disappearance the moon and *Saturn* were exceedingly tremulous, and it was found impossible to use more than a very low power (about 40); the observations were considered doubtful, particularly that of the disappearance of the ball, in consequence of the indistinctness of the planet. At the reappearance, *Saturn* was very faint, and the images were still unsteady; they were, however, much better than at the disappearance, so that it was found practicable to use a considerably higher power (about 150), the observations being all considered satisfactory.

By Mr. Ellis [E].—With the altazimuth, and a power of about 100. The observations were all considered satisfactory.

By Mr. Criswick [C].—With the telescope formerly attached to the west equatoreal, placed in the Octagon Room, and a solar chronometer. The observations of the disappearance were considered satisfactory, but those of the reappearance rough, in consequence of the extreme faintness of the planet. The power used was about 70.

By Mr. Lynn [L].—With the north equatoreal, and a power of about 60. It unfortunately appeared that at the disappearance the moon was hidden behind one of the turrets of the observatory, and no observation therefore could be made. The planet at the reappearance was very faint, and the first reappearance was therefore not caught, nor could satisfactory

observations of the reappearance of the ball be obtained; that of the final reappearance was, however, considered accurate.

Further Observations of the Occultation of Saturn by the Moon on the 8th of May.

Observed by T. W. Burr, Esq., at Highbury.

The observations as to time on which I can place reliance were the last contacts of the ring both at immersion and emersion.

	Local Sidereal Time. h m s	
Immersion, last contact of ring	11 23 37·1	Dark limb.
Emersion, ,, ,,	12 22 26	Bright limb.

No projection, illumination of the moon's limb or other abnormal phenomena were observed, but the contrast between the brilliancy of the moon and the dull leaden blue colour of the planet at the emersion was very striking. Telescope 4 ft. 4 in. achromatic, aperture 3⅛ in., power 173, longitude 23ˢ S.W.

Observed by William Simms, Jun., Esq.

The telescope employed was of 4 inches' aperture and 57 inches focal length; power 200.

The dark limb of the moon, which was clearly seen previously with a low power, passed steadily over the planet, without the slightest distortion appearing to either body, the unobscured portion of the planet retaining its usual light up to the disappearance of the edge of the ring.

The reappearance, which was carefully looked for, took place at a very rugged part of the moon's bright limb, and escaped observation until a considerable part of one of the ansæ of the rings was projected; so feeble was the light that I could not, for three or four seconds, believe it to be the planet. When the body was advanced about two-thirds of its diameter, I saw the band of dark shade over the planet, accompanying the edge of the moon, similar to the appearance noticed by myself and several observers at the last occultation of *Jupiter;* and which I now believe to be caused by the violet fringe of the secondary spectrum, seen round all very bright objects in refracting telescopes.

May not the projection of stars upon the moon's limb, frequently seen at occultations, be caused by the ray from the star having to pass through this violet fringe in its passage to the eye at the time of contact? Telescopes differing greatly in the amount of this colour might account for the irregularity of the observation of this phenomenon; and which, as far as I am aware, has not been seen in reflecting telescopes.

Observed by J. W. Jeans, Esq., at Grantham.

The time of first contact is uncertain to one or two seconds, also that of first reappearance; but it is, I think, more accurate than the former; the times of total immersion and emersion are correct within very narrow limits. The only particular appearances which I noted were, that, upon immersion, the last portion of the ring seemed to linger for the fraction of a second, and then disappeared suddenly, and that, upon emersion, the colour of the planet was very ashy, but, I believe, entirely from contrast. I observed nothing noticeable which could be attributed to a lunar atmosphere, but certainly the conditions were highly unfavourable.

The instrument was a refractor, by Cary, of 4 inches aperture, equatoreally mounted; and, after several trials, I fixed upon a power of 120, as giving, *cæteris paribus*, the best view. The time was corrected by transits of *Regulus*, *β Leonis*, *ι Corvi*, and *α Virginis*; and the times of contact, &c., are given in corrected local sidereal time.

Barom. at 9^h P.M., 30^{in},100, rising. Therm. 48°.

Corrected Local Sidereal Time.

	h m s		h m s
First contact ...	11 17 37?	Total immersion	11 18 20·55
First reappearance	12 16 43?	Last contact ...	12 17 42·08

Lat. N. 52° 54′ 42″. Long. W. 0° 39″.

Observed by the Rev. W. R. Dawes, at Haddenham, Thame.

The lunar occultation of *Saturn* was well seen here on the evening of the 8th. I observed it with a power of 235 on the excellent 7¼-inch object-glass I have now in use.

At the disappearance, the dark edge of the moon was sharply defined on the rings and ball of the planet, without the slightest distortion of their figure. There was no extension of light along the moon's limb. Even the satellites disappeared without the slightest warning, and precisely at the edge, which was faintly visible.

At the reappearance, I could not perceive any dark shading contiguous to the moon's bright edge, such as was seen by myself and several other observers on *Jupiter*, on 2d January, 1858. The dark belt, south of the planet's equator, was clearly defined up to the very edge; and there was no distortion of any kind, either of the rings or ball.

The very pale greenish hue of *Saturn* contrasted strikingly with the brilliant yellowish light of the moon.

No attempt was made to note the time.

Observed by S. Whitbread, Esq., at Cardington.

	h m s		h m s
First contact	11 20 20	Reappearance	12 19 13
Disappearance	11 21 4	Last Contact	12 20 58

Local Sidereal Time, May 8, 0 . 0 . 0 = $3^h\ 3^m\ 8^s$. $1^m\ 39^s$ W. G.

Observed by Sir W. K. Murray, Bart., at Ochtertyre.

I observed the occultation of *Saturn* under favourable atmospheric circumstances on the 8th; but, unfortunately, just before the moon's limb approached the ball of the planet, the clockwork suddenly stopped, which caused a most untoward interruption, though only for an instant. I could not detect the slightest distortion or change in the image of the planet, either at disappearance or reappearance.

The instrument used was a 13-feet equatoreal, of 9 inches clear aperture, by Mr. Cooke, of York; power 120.

Mr. Airy's paper on Occultations of Stars by the Moon reminds me that, in observing the occultations of a double star last year, one of which was of a reddish tinge, it appeared to pass on to the disk of the moon, whereas the white star disappeared instantaneously.

Observed by Captain Noble, at Forest Lodge, Maresfield.

Previously to the occultation *Saturn* was examined in the twilight (from $7^h\ 45^m$ to 8^h M.T.). The detail of the planet was remarkably distinct. The banding on the body of *Saturn*, the nebulous ring (C), the sharp black shadow of the ball upon the rings, &c., being beautifully shown.

The first contact of the moon's limb with the preceding edge of the ring, took place about $11^h\ 24^m\ 44^s$ L.S.T. The disappearance of the preceding limb of *Saturn* at $11^h\ 24^m\ 50^s{\cdot}7$, and the final disappearance at $11^h\ 25^m\ 32^s{\cdot}2$.

	h m s
The preceding limb of the planet emerged at	12 23 10·8
The following limb of the planet emerged at	12 23 40·7
And the final total emersion took place at	12 24 1·3

The limb of the moon advanced over the planet with exceeding steadiness, every feature of the detail remaining perfectly distinct up to the instant of its disappearance. The most noticeable effect was exhibited on the emersion of *Saturn* from behind the bright limb of the moon. Owing to the effect of contrast he appeared of a faint greenish grey, or greyish green colour (such as might be produced by mixing yellow ochre and

bistre with a great deal of water). Notwithstanding which the detail was extremely distinct, the planet presenting the appearance of a water-colour drawing which had been washed out, so as just to leave the outlines undisturbed. He apparently brightened rapidly on his recession from the moon. Neither the slightest distortion of figure, nor anything approaching to a shake or a wave, was seen, either during the immersion or emersion. I so entirely devoted my attention to the physical detail of the planet that the times given above must (with reference to the respective limbs of *Saturn*) be received with some caution. The observations of final disappearance and of total reappearance are, however, very good, and perfectly to be relied on. The telescope employed was my Ross equatoreal, of 61 inches focal length, and 4·2 inches aperture, with a power of 255 throughout.

Observed by W. R. Grove, Esq.

I observed the occultation with a 6-foot telescope, 4·3 inches aperture, power 165. I imagined that, upon the moment of first contact, the ring was a little drawn out into a point, succeeded by a knob or apparent excrescence; but on thinking it over since, it has occurred to me that, as I could not see the edge of the moon's dark limb, and had no clock or means to advise me of the moment of impact, the effects I noticed may well have been due to the cutting off, or, as it were, sharpening the point of the ring by the oblique incidence. I mention the apparent effect, however; as, if better observers have noticed it, there may be something in it as a corroboration.

From the impact until the total disappearance, I could not notice the slightest distortion.

On emergence, the extremely faint light of *Saturn*, contrasted with the moon, was very remarkable. *Saturn* was a mere ghost of himself, a faint grey-blue shadow. The line of the moon's edge was perfect, and no dark band was perceptible, as I and others noticed in the occultation of *Jupiter*. The part of the ring last emergent appeared flattened, or shorter than the opposite portion, doubtless from the greater contrast of light, or from irradiation. I think the former was the cause, as some disproportion continued after *Saturn* was clear of the moon, and distant from the edge about the diameter of its own orb. The atmosphere was better than the average for the early time of night, and I doubt not some good observations will have been made.

Observed by W. Lassell, Esq., at Bradstones, near Liverpool.

I observed the occultation of *Saturn* on the 8th instant with the 20-foot equatoreal, under as favourable circumstances as I usually obtain.

The power used was 316 and the aperture 22 inches. At the immersion the moon's edge was remarkably sharp, black, and smooth, while the planet, though sharp, quivered greatly about the middle of the immersion, which occupied about 51 seconds. The immersion was very oblique to the transverse axis of the ring, while at the emersion the planet protruded the end of its ring almost in the direction of a radius of the moon. At the emersion the planet was very steady and the moon's limb disturbed — a remarkable contrast to the phenomena of the immersion.

The colour of *Saturn* on emerging was certainly pale and of a greenish hue, in comparison with the moon's lustre and colour; yet, calling to mind the late occultation of *Jupiter*, it was not so pale and dull as I expected.

The time occupied by emersion was somewhere about 80 seconds, but I did not think it important to notice very exactly the times of the phenomena, my attention being engaged in closely watching for any remarkable appearance which might present itself. Nothing, however, did present itself worthy of remark; the planet underwent no distortion nor apparent alteration whatever during either transit across the moon's limb.

Right Ascension and North Polar Distance of the Minor Planets, from Observations made at the Madras Observatory in the Years 1853–1857. By Captain W. S. Jacob, late Director of the Madras Observatory.

⑤ *Astræa.*

	Mean Solar Time of Observation.	R.A. from Observation.	N.P.D. from Observation.
1853.	h m s	h m s	° ′ ″
Dec. 12	8 20 0·9	1 44 54·20	86 57 43·38
19	7 51 39·4	1 44 3·81	86 44 21·97
1856.			
Aug. 7	10 45 39·8	19 51 19·21	
22	9 36 34·7	19 41 11·46	

⑥ *Hebe.*

1857.			
Feb. 14	12 8 44·4	9 47 40·26	74 0 30·52
Mar. 2	....	....	71 34 11·67
3	10 47 14·8	9 32 58·59	71 27 59·35
	10 47 53·0	9 33 36·82	
5	10 38 53·8	9 32 29·29	
6	10 33 18·0	9 30 49·12	71 3 45·58
16	9 48 15·9	9 25 5·44	
19	....	....	69 47 16·45

⑦ *Iris.*

	Mean Solar Time of Observation.	R.A. from Observation.	N.P.D. from Observation.
1854.	h m s	h m s	° ′ ″
Aug. 7	7 49 40·0	16 52 53·00	110 43 27·45
1856.			
Jan. 5	....	....	69 15 47·34
7	10 25 9·0	5 30 58·51	69 23 11·16
8	10 22 36·4	5 32 22·30	69 26 41·21
9	10 16 59·6	5 30 40·34	69 30 9·51
	10 18 46·1	5 32 27·20	
11	....	....	69 36 49·80
Feb. 12	8 2 42·5	5 30 5·26	70 31 17·84

⑧ *Flora.*

1853.			
Oct. 12	....	....	114 38 25·06
13	6 51 42·4	20 19 47·52	

⑨ *Metis.*

1853.			
Oct. 11	11 46 46·0	1 7 47·48	91 31 12·25
12	11 41 52·1	1 6 48·49	91 34 50·88
13	11 36 58·2	1 5 50·25	91 38 16·92
14	11 32 4·0	1 4 51·91	91 41 38·41
15	11 27 10·3	1 3 53·97	91 44 53·77
25	10 38 43·0	0 54 44·59	92 8 10·95
27	10 29 12·5	0 53 4·71	92 10 37·74
Nov. 15	9 3 51·6	0 42 24·26	
16	8 59 39·7	0 42 8·37	91 48 50·35
17	8 55 29·5	0 41 54·24	91 45 30·45
18	8 51 21·2	0 41 41·54	91 42 58·75
22	8 35 7·3	0 41 11·65	91 25 51·95
23	8 31 8·9	0 41 9·11	91 17 11·51
24	8 27 11·8	0 41 7·80	91 16 48·46
26	8 19 24·9	0 41 12·36	
30	8 4 11·0	0 41 42·69	90 44 57·77
Dec. 2	....	....	90 32 15·67
1856.			
Aug. 7	9 39 4·6	18 44 33·12	119 4 8·25
	9 39 55·0	18 45 23·64	
8	9 33 22·2	18 42 46·31	

⑩ *Hygeia.*

1856.			
Aug. 22	11 26 32·5	21 31 27·33	101 17 2·97
	11 26 57·4	21 31 52·31	

⑪ *Parthenope.*

	Mean Solar Time of Observation.	R.A. from Observation.	N.P.D. from Observation.
1854.	h m s	h m s	° ′ ″
July 21	8 27 2·1	16 23 15·21	107 23 19·25
1855.			
Nov. 19	11 16 35·5	3 9 22·58	79 57 23·18
21	11 6 50·9	3 7 29·06	80 2 41·27
1856.			
Jan. 3	7 58 11·5	2 47 40·51	79 37 38·48
5	....	....	79 27 40·73
8	....	....	79 14 58·86
17	7 8 14·5	2 52 57·80	
18	7 5 5·4	2 53 44·63	78 29 45·76
1857.			
Mar. 4	11 32 11·3	10 21 58·98	
16	10 35 4·4	10 12 1·64	
	10 35 5·8	10 12 3·04	
17	10 30 28·2	10 11 21·25	74 37 0·86
19	10 21 14·9	10 9 59·53	
April 16	8 21 44·4	10 0 32·57	

⑲ *Victoria.*

1854.			
Dec. 6	11 2 8·6	4 2 51·95	71 59 57·26
7	10 57 40·7	4 2 19·28	71 59 43·76
1857.			
Oct. 8	8 12 33·5	21 21 17·65	

⑫ *Egeria.*

1854.			
Oct. 24	10 58 6·6	1 9 17·31	
1856.			
Mar. 17	10 41 5·9	10 22 56·90	54 16 35·45
	10 42 26·1	10 24 17·38	
April 2	9 29 49·4	10 14 33·85	

⑭ *Irene.*

1853.			
Dec. 1	11 27 12·6	4 9 14·60	74 18 29·56
	11 27 57·2	4 9 59·40	
3	11 16 58·6	4 6 51·93	74 17 28·75
	11 17 16·3	4 7 9·66	
6	11 1 45·1	4 3 24·85	74 16 8·00
	11 2 25·1	4 4 4·95	

⑮ *Eunomia.*

1854.			
Mar. 21	11 33 43·7	11 29 27·39	103 51 40·26
April 1	10 41 22·4	11 20 19·39	102 59 54·86
7	10 13 30·2	11 16 2·18	102 24 45·28
8	10 8 55·2	11 15 23·05	

(16) *Psyche.*

	Mean Solar Time of Observation.	R.A. from Observation	N.P.D. from Observation.
1854.	h m s	h m s	° ′ ″
July 27	11 45 31·6	20 5 56·05	107 53 49·40
1856.			
Jan. 3	8 49 39·3	3 39 16·77	74 42 33·80
4	8 46 42·3 8 47 23·1	3 40 26·22 3 41 7·10	74 41 40·55
5	8 43 18·7	3 40 58·47	74 40 28·87
7	8 35 13·6	3 40 45·06	74 38 4·42
8	8 31 13·9	3 40 41·54	74 36 36·94
9	....	....	74 35 22·94
10	....	....	74 33 46·30
24	7 30 44·3	3 43 6·66	74 2 37·39
25	....	....	74 0 8·46
28	7 16 37·4	3 44 43·83	73 51 0·36
1857.			
Mar. 2	11 24 2·5	10 5 55·83	78 6 34·61
4	11 16 1·4	10 5 46·42	

(17) *Thetis.*

1853.			
Oct. 11	9 41 7·9	23 1 47·71	103 24 38·73
15	9 22 55·2	22 59 18·43	103 30 58·02

(18) *Melpomene.*

1856.			
Oct. 31	11 47 28·9	2 28 26·24	

(19) *Fortuna.*

1854.			
Mar. 10	10 6 9·3	9 18 16·90	76 28 34·56

(20) *Massilia.*

1854.			
April 1	10 55 37·4 10 56 25·2	11 34 36·70 11 35 24·64	87 56 33·84
7	10 28 39·6 10 29 25·0	11 31 13·99 11 31 59·61	87 24 12·86
1856.			
Nov. 1	11 54 18·2	2 39 13·08	74 47 41·89

(21) *Lutetia.*

1854.			
Mar. 22	9 38 35·8	9 37 57·18	71 26 24·19
23	9 34 39·9	9 37 57·05	71 26 24·45
24	9 30 43·8	9 37 56·93	71 26 24·45

(21) *Lutetia* (*continued.*)

	Mean Solar Time of Observation.	R.A. from Observation.	N.P.D. from Observation.
1857.	h m s	h m s	° ′ ″
Jan. 20	8 10 24·1	4 10 6·79	68 44 58·79
21	8 6 28·4	4 10 6·98	68 44 57·02
23	7 58 59·1	4 10 29·50	68 37 53·52
24	7 55 3·4	4 10 29·69	
Feb. 9	6 56 37·9	4 14 59·68	
	6 57 2·6	4 15 24·46	

(22) *Calliope.*

1854.			
April 6	11 1 13·0	11 59 56·40	
1856.			
Sept. 10	11 0 53·5	22 20 37·66	122 14 13·60

(23) *Thalia.*

1854.			
July 8	7 42 16·7	14 47 6·26	104 44 42·73
	7 42 40·5	14 47 30·15	

(25) *Phocea.*

1856.			
Feb. 13	11 48 37·7	9 20 33·70	
Mar. 1	10 27 2·4	9 5 46·98	
4	10 13 57·8	9 4 29·43	102 59 7·79
5	10 10 48·5	9 5 15·78	102 49 13·28
	10 10 53·3	9 5 20·55	
6	10 5 49·3	9 4 12·81	
	10 6 1·6	9 4 25·17	
7	10 0 6·3	9 2 25·23	102 31 4·55
	10 0 8·7	9 2 27·64	
15	9 24 20·9	8 58 6·62	101 16 10·40
1857.			
Oct. 8	6 30 4·4	19 38 31·70	84 28 13·76
	6 30 27·3	19 38 54·62	

(26) *Proserpine.*

1854.			
Sept. 19	10 43 30·5	21 36 39·13	
Oct. 7	9 24 25·7	22 28 19·58	
1857.			
Mar. 16	11 49 47·2	11 26 56·71	
17	11 45 5·7	11 26 11·00	
	11 45 30·2	11 26 35·59	
19	11 35 53·8	11 24 50·73	

(97) *Euterpe.*

1856.	Mean Solar Time of Observation. h m s	R.A. from Observation. h m s	N.P.D. from Observation. ° ′ ″
Aug. 22	11 15 26·7	21 20 19·70	
	11 15 44·5	21 20 37·58	107 35 3·57
	11 16 43·3	21 21 36·48	

(29) *Amphitrite.*

1857.			
Jan. 20	7 31 12·4	3 30 48·67	
21	7 26 58·1	3 30 30·19	
23	7 20 28·7	3 31 52·74	

(30) *Urania.*

1856.			
Feb. 13	10 49 3·9	8 20 50·14	
14	10 44 17·0	8 19 59·12	70 38 37·13
15	10 39 31·1	8 19 8·91	70 37 38·46
16	....	....	70 35 46·22

(31) *Euphrosyne.*

1856.			
Mar. 17	10 38 25·1	10 20 15·74	44 8 9·06

(32) *Pomona.*

1856.			
Mar. 4	10 41 19·8	9 31 55·98	84 16 3·13
5	10 36 33·2	9 31 4·65	84 4 39·20
7	10 28 40·1	9 31 3·73	
	10 28 50·9	9 31 14·57	
1857.			
July 23	10 33 12·8	18 38 44·84	
Aug. 4	9 38 20·1	18 31 2·00	

(33) *Polyhymnia.*

1856.			
Feb. 11	11 14 36·7	8 38 33·93	
12	11 8 54·0	8 36 47·25	69 5 5·07
	11 9 18·6	8 37 11·93	
1857.			
Mar. 19	11 48 43·8	11 37 42·82	86 51 17·58

(36) *Atalanta.*

1857.	Mean Solar Time of Observation. h m s	R.A. from Observation. h m s	N.P.D. from Observation. ° ′ ″
Mar. 17	11 13 35·6	10 54 35·70	
19	11 3 12·2	10 52 3·70	
24	10 39 42·4	10 48 13·21	
25	10 35 7·0	10 47 33·26	
April 16	8 55 14·5	10 34 8·26	76 22 10·56

(37) *Fides.*

1857.			
Mar. 16	11 1 2·9	10 38 4·44	
17	10 56 46·2	10 37 43·59	
26	10 17 2·3 10 17 23·8 10 17 39·5	10 33 21·90 10 33 53·53 10 33 59·17	78 55 15·18
April 16	8 47 39·4	10 26 31·83	78 51 34·19

N.B.—The north polar distances of these minor planets are reduced without the application of the horizontal parallax corrections, as they were not given in the *Nautical Almanac*.

RECENT PUBLICATIONS.

Account of the Construction of the New National Standard of Length, and of its principal Copies. By G. B. Airy, Esq., Astronomer Royal.* 4to. London, 1858.

This paper was drawn up by the author in consequence of the lamented death of his valued friend Mr. Sheepshanks, upon whom the preparation of such an account for the press would have naturally devolved. It is divided into nine Sections.

Section I. *History of the British and of some Foreign Standards, and of the Methods of using them in Base-Measures and Pendulum-Measures, anterior to the Legalisation of the Imperial Standards by the Act of Parliament of* 1824; *definition of the Standard of Length by that Act; and Provision for its Restoration in Case of Loss.* — In this section the author refers to various papers in the *Transactions of the*

* *Phil. Trans.* vol. cxlvii.

Royal Society down to the year 1824, containing accounts of standard measures and their comparisons with each other. Among these may be mentioned a standard yard which Bird executed for the House of Commons in 1760, and which was eventually adopted as the legal measure of the country. In the comparisons of standards effected during the earlier part of last century the comparing instrument was a beam compass. Troughton, however, in executing a scale for Sir George Shuckburgh from a scale of Bird's (*Phil. Trans.* 1798), employed for that purpose a bar carrying adjustible micrometer-microscopes. Captain Kater subsequently compared Shuckburgh's scale with the pendulum vibrating seconds in the latitude of London and also with the mètre (*Phil. Trans.* 1821); and having moreover found that it closely agreed with Bird's Standard of 1760, he proposed that the latter should represent the Imperial Standard of length. Accordingly, in 1824, an Act of Parliament was passed, by which the distance between the centres of the two points in the gold studs, inserted in the brass rod known as Bird's Standard of 1760, was declared to be "the original and genuine standard of that measure of length or lineal extension called a Yard;" and the distance between the said points when the temperature of the brass rod stood at 62° of Fahrenheit, was denominated "the Imperial Standard Yard." It was further enacted that in case of the standard being destroyed, defaced, or otherwise injured, its restoration should be effected by means of its known relation to the length of a pendulum vibrating seconds in the latitude of London in a vacuum at the level of the sea.

Section II. *Comparisons of Standards between the passing of the Act of* 1824 *and the Appointment of a Commission for Consideration of Standards after the Destruction of the Imperial Standard in* 1834*; with Remarks suggested by the Advance of Collateral Theory and Experiment in that interval.* — Kater had found, in the course of his comparisons of secondary standards, that discordances to the amount of $\frac{1}{500}$th of an inch were produced by almost undiscoverable inequalities in the surface of the table on which they were laid. In the new secondary standards this defect was cured by removing the upper half of the thickness of the bar, in the parts where the defining points are planted, so that the defining points are in reality at the middle of the depth of the bar. Again, Baily, while engaged in the comparison of the tubular scale of the Royal Astronomical Society with several other standards, found that an injurious effect was produced by placing the tube upon fixed supports. He was, therefore, led to employ rollers for that purpose. In connexion with this subject we may refer to a paper by the Astronomer Royal in vol. xv. of the *Memoirs* of the Society, the results of which were used in subsequent observations. It relates to the value of the intervals (supposed equal) which ought to exist between different supports of a

bar, each support exerting the same vertical pressure upwards, in order that the interval between two points upon the surface of the bar may not be altered by the flexure.

Another important fact connected with standard measures was Bessel's discovery that the correction for reducing the oscillations of a pendulum to a vacuum was sensibly erroneous. It was also strongly suspected that the reduction to the level of the sea was not unexceptionably accurate. The author thus sums up the advances which had been made in the science of standards from 1824 to 1834, the year of the destruction by fire of the Houses of Parliament:—

"It had been shown that it was imprudent in any case to trust to points or lines traced on the surface of a bar, and therefore for delicate purposes, supposing the standard to be a line measure, only two lines or points ought to be used, sunk to the middle of the bar's thickness. It had been shown that it was imprudent to lay the bar upon a table or upon fixed supports of any kind; and, therefore, the bar must be stiff enough to bear to be supported upon a few points at which rollers could be conveniently applied. It had been shown that the physical reference provided in the Act of Parliament of 1824 was erroneous in one particular and doubtful in another; and as it seemed likely that similar uncertainties might be found in any other physical reference, the conviction was gradually rising that it would be better to trust for restoration to attested copies of the standard. The question of the propriety of adopting line measure or end measure for the National Standard, which, in this country, had been practically decided (without a single opposing instance as regarded accurate standards) in favour of line measure, had again been raised by Bessel's adoption of end measure."

On the 16th of October, 1834, the Standards of weight and measure fell a prey to the fire which destroyed both Houses of Parliament. The bar of 1760 was, indeed, recovered, but one of its gold pins bearing a point was melted out, and the bar was otherwise injured.

Section III. *Appointment of the Treasury Commission of* 1838; *its Proceedings and Report. Appointment of the Treasury Commission of* 1843; *its Proceedings to the Death of Mr. Baily in* 1844.—In 1838 a commission was appointed by the Government to take into consideration the proper steps for determining new standards of weight and measure, to replace those which were destroyed by the burning of the Houses of Parliament. The members of this commission consisted of the Astronomer Royal, Mr. F. Baily, Mr. J. D. Bethune, Mr. Davies Gilbert, Mr. J. S. Lefevre, Mr. J. W. Lubbock, Rev. G. Peacock, and the Rev. R. Sheepshanks. In their Report to the Government they stated that the definition of the standard yard, according to which it is declared to be *a certain brass* rod, as specified in the Act 5 Geo. IV. cap. 74,

sec. 1, appeared to them to be the best which it was possible to adopt. With respect to the provisions for restoration, they were not prepared to recommend the adoption of that prescribed in section 3 of the same Act, whereby it is directed that in case of the loss of the metallic standard, the yard should be restored by taking the length which should bear a certain proportion to the length of the pendulum, vibrating seconds of mean time, in the latitude of London, in a vacuum at the level of the sea. They remarked that several of the elements of reduction of pendulum experiments had been found to be erroneous or doubtful, and they pointed out other sources of inaccuracy. They then proceeded to state that several measures exist which were most accurately compared with the former standard yard, and that by means of them the value of the latter could be restored without sensible error; and they expressed their full conviction that, with reasonable precautions, it will always be possible to provide for the accurate restoration of standards, by means of material copies which have been carefully compared with them, more securely than by reference to any experiments referring to natural constants. The committee accordingly recommend a course of procedure, of which the following are some of the more important features:—

"1. That so much of the Act 5 Geo. IV. cap. 74, as provides for the restoration of the standards in the manner therein mentioned be repealed, and that the standard of length be defined, in subsequent enactments of the Legislature, either by the whole length of a certain piece of metal or other durable substance, supported in a certain manner, at a certain temperature (to the construction and dimensions of which further allusion will be made in this Report), or by the distance between two points or lines engraved upon the surface of a certain piece of metal or other durable substance, supported in a certain manner, at a certain temperature; but that the standard be in no way defined by reference to any natural basis, such as the length of a degree of meridian on the earth's surface in an assigned latitude, or the length of the pendulum vibrating seconds in a specified place.

"5. That four sets of copies (to be denominated the Parliamentary Copies) of the standards of length and weight be made in all respects similar to the legal standards of length and weight (which are hereinafter denominated the Parliamentary Standards), and as nearly as possible equivalent to them, their difference from the Parliamentary Standards, if appreciable, being carefully ascertained; and that the several Parliamentary Copies be distinguished by proper marks.

"6. That one set of the Parliamentary Copies be enclosed in a case hermetically sealed, and embedded within the masonry of some public building; and that the place in which it is enclosed be pointed out by a conspicuous inscription on the

outside, and that this set of copies be not disturbed without the sanction of an Act of Parliament.

"17. That the superintendence of the construction of the new Parliamentary Standards be entrusted to a committee of scientific men, to be named by her Majesty, subject to the general instructions which follow.

"18. That the Superintending Committee be instructed to combine, in the way which may appear most advantageous, the evidence afforded by the Royal Astronomical Society's scale, by the Royal Society's scale, No. 46, and by the three-feet bars of the Ordnance Survey;* and by the use of these to construct a new standard of length, representing as nearly as possible the same measure as the lost standard.

"19. That the whole length of the bar, or the distance between two points or lines marked upon it, be adopted as the parliamentary standard of length; and that the bar bear no other divisions.

"20. That the question of adopting the whole length or the distance between two points or lines, as well as the choice of material, and the general construction of the standard bar, be left to the decision of the Superintending Committee.

"21. That the length of the new Parliamentary Standard be one yard; there appearing to us no sufficient reason for departing from the length hitherto adopted for the standard.

"22. That the thermometrical expansion of the standard bar be determined by actual experiment on it.

"23. That the temperature at which the length defined by the standard bar represents the yard, be fixed by the Superintending Committee."

A committee was now appointed to superintend the measures necessary for carrying into effect the recommendations of the original committee. It consisted of the following individuals: the Astronomer Royal, the Marquis of Northampton, the Lord Wrottesley, F. Baily, Esq., J. E. D. Bethune, Esq., J. G. S. Lefevre, Esq., Sir J. W. Lubbock, Bart., the Rev. G. Peacock, Dean of Ely, the Rev. R. Sheepshanks, Sir J. F. W. Herschel, Bart., and Professor Miller. The name of the Earl of Rosse was subsequently added. Mr. Baily undertook to carry out the views of the committee in regard to the standard of measure; Professor Miller's services were engaged for the standard of weight.

The first meeting of the committee was held on the 11th of July 1843, when it was reported that the following measures were collected :—

The scale of the Royal Society, No. 46.†

* These four standard measures had been compared directly with the imperial standard previous to the year 1834.

† There existed in the hands of a private individual another scale exactly similar to the Royal Society's, but an application for the loan of it on the part of the Committee was refused.

The scale of the Royal Astronomical Society.

The two 3-foot bars of the Ordnance Survey.

It was resolved (provisionally) that the parliamentary standard of length be a measure *à traits*, [subsequently confirmed;] that the material be gun-metal (including any variations in the proportion of tin, &c.) or steel, as Mr. Baily should think best; that the standard be about an inch square; that its ends be notched away to half its thickness, and that the marks defining the standard length be made upon pins (if necessary) inserted in the surface parallel to the length of the bar thus exposed; that the marks be adjusted to represent the length of one yard at the temperature of 61° or 62°, as nearly as possible; and that the bar be supported by a series of supports distributed over many points through its whole length, so as to destroy the effect of gravity in bending the bar as completely as possible.

Mr. Baily proceeded to make experiments on different materials, with the view of ascertaining the substance most suitable for the construction of standard bars. In accordance with the Astronomer Royal he entertained the opinion that if a bell-metal or gun-metal mixture could be found possessing the properties most essential to the present use of a standard bar, it would be greatly preferable to iron or steel for the construction of a standard intended to last through many ages, by reason of its almost perfect immunity from rust.

The results of Mr. Baily's experiments were communicated to the committee by the Astronomer Royal on the 30th of November, 1844. The gun-metal, No. 4, was adopted in all the standard bars subsequently made (except where a difference is specially mentioned), and appears to the author to be an admirable alloy for all similar purposes. Its exact composition is,

Copper	16
Tin	2½
Zinc	1

Mr. Baily, having concluded his experiments on the suitableness of metals for standard bars, proceeded to make a series of experiments on the influence produced by small inequalities of the surface supporting a bar. About the same time experiments on thermometric expansion were made by Mr. Simms at 138 Fleet Street, under the general instruction of Mr. Baily, and with some suggestions from the Astronomer Royal.

Mr. Baily finally proceeded to make comparisons of the standards of superior construction which were known to have been compared with the Imperial Standard, with the view of ascertaining how far they might prove available for the restoration of the standard yard. The results were of a negative character. It was shown that in no instance did there exist a trustworthy representative of the Imperial Standard. Mr. Baily

died on the 30th of August, 1844. The state in which the problem was left to his successor is thus enounced by the Astronomer Royal:—

"The formation of a new standard must be an operation *de novo;* the length must be confined within certain limits (wide in the scientific, narrow in the commercial sense), but within these it might have any definite value; and when that definite value was fixed, it must be in no way referred again to the old standards or scales, whether original or intermediary. The principal object now was to ensure constancy and definiteness to the new standard and its copies, and means of reducing without sensible error the comparisons which might be made with them. As far as depended on the standard itself, it was hoped that the construction adopted gave sufficient security. As regarded the means of making comparisons, a far firmer apparatus than had been hitherto used was requisite. As regarded the effects of temperature, it was necessary to create an entirely new system of thermometers, founded upon the natural constants, to be determined by appropriate physical experiments, and to use them in new determinations of thermometric expansion."

Section IV. *Proceedings of the Committee and of Mr. Sheepshanks to* 1847, *June; Construction of new Thermometers and erecting of Comparing Apparatus; Description of the Apparatus and of Mr. Sheepshanks' method of Comparing.* —The committee at their meeting on the 30th of November, 1844, received with pleasure and thankfully accepted Mr. Sheepshanks' offer to continue the work commenced by Mr. Baily. A request to that effect was in consequence addressed by the Lords Commissioners of the Treasury to Mr. Sheepshanks, which was acceded to by the latter, who, like Mr. Baily, declined to accept any pecuniary remuneration for his services. On the 6th of June, 1845, Mr. Sheepshanks communicated a Report to the Committee in which he further explained his proposed plan of operations.

"Mr. Sheepshanks," says the author, "now turned his attention energetically to the practical preparation of the various apparatus to which his Report alludes. Of the details of the incessant labour of the next two years, few traces remain except in Mr. Sheepshanks' correspondence with myself. But to these labours there was little interruption. Old apparatus was to be tested, and new to be planned and carried into execution; instrument-makers, much occupied with other business, were to be kept closely to the work; manipulations of various kinds were to be learned; and all was done under the disadvantage of Mr. Sheepshanks' domestic residence being at Reading, while the work of every kind was executed in London. The most tedious of the operations actually carried on appears to have been the preparation of original thermometers (or rather the acquisition of the experience necessary for their preparation,

for I do not think that a thermometer was really finished before the end of 1847). It would seem that at this time there was no workman in London who could be trusted without the closest superintendence even in the minutest points, to make a thermometer."

Mr. Sheepshanks' experiments were conducted at the Apartments of the Royal Astronomical Society, Somerset House, in one of the cellars two storeys below the ground-floor. Upon a solid platform of masonry were erected two stone pillars, upon which rested a horizontal slab, carrying two cross-stones or transoms, to which the micrometer-microscopes were attached. Upon a movable platform was placed a large trough of cast-iron nearly filled with water, for adjustment of temperature, and within this trough was a drop-box, into which the bars intended for comparison were put. During the comparison of a bar it was made to float *very nearly* in quicksilver. For this purpose a quicksilver box was inserted in the drop-box. The bars, before being placed in the quicksilver, were all covered with gold-beater's skin. The defining points of the bars were illuminated by means of two lamps, whose light passed through perforations in the horizontal slab towards the reflectors of the microscopes. In all the bars ultimately constructed by Mr. Sheepshanks, the defining mark is a fine line, transversal to the bar; accompanied by two lines parallel to it, one on each side at the distance of $\frac{1}{100}$th of an inch, which were intended to give the means of ascertaining the value of the divisions of the micrometer. For further details we must refer to the paper. The views of Mr. Sheepshanks met with the cordial approval of the Committee.

Section V. *Proceedings of Mr. Sheepshanks to the middle of* 1850; *Preparation of Thermometers, &c.*—This section contains an account of the measures made by Mr. Sheepshanks with the various apparatus described in the preceding section. Various bars were compared in succession with the best primary representatives of the lost Imperial Standard. In the course of these observations he was finally induced to adopt a bar denominated "Bronze 28," as the type of his subsequent measures. He remarked that the length of this bar was as nearly as possible that of the Imperial Standard, that the bar floated evenly in the quicksilver, and that it was nicely divided. Bronze 12, which had also engaged his attention for the same purpose, was not nicely divided and did not float evenly; and one of its gold dots was tarnished while the other was quite bright; and the tarnished dot was much disfigured by a mercurial stain; it was, moreover, found to be peculiarly liable to personal equation.

Section VI. *Operations of important character to the end of* 1853; *Comparisons of a small number of Bars with Bronze* 28; *Investigation of Personal Equation, &c.*—This section contains results for determining the influence of personal equation.

The persons who assisted Mr. Sheepshanks in these experiments were, Mr. Henderson and Mr. Dunkin, of the Royal Observatory; Mr. Simms, Mr. W. Simms, jun., and Mr. Warren De La Rue. Five bars were compared with the bar denominated Bronze 28, and two other bars were similarly compared together. The author has rigorously treated the aggregate observations by the principles of the theory of probabilities. The following are the results of a comparison of Bronze 10 with Bronze 28. The letter *d* indicates a division of the micrometer screw or a hundredth part of a revolution, or $0^{in}{\cdot}0000358$:—

Excess of the length of Bronze 10 above the length of Bronze 28

Observer.	Excess.	Probable Error.
	d	d
Sheepshanks	−1·28	0·042
Henderson	−1·78	·088
Dunkin	−1·41	·101
Simms	−1·39	·079
W. Simms, Jun.	−1·83	·074
De La Rue	−1·66	·117

The foregoing numbers, as well as those relative to the other bars, indicate the existence of a personal equation of considerable magnitude. The final results for each case are obtained by taking the simple arithmetical mean of the results of the different observers. Experiments were also made for ascertaining the relative expansions of bars of several metals.

Section VII. *Comparisons of numerous Bars, from* 1851 *to* 1855: *Suspicion of Change in Bronze* 28; *Removal of the Suspicion; Close of the Operations for determining the Yard by Line Measure.*—In addition to a large number of bronze bars Mr. Sheepshanks made experiments upon bars of brass, copper, iron, and steel. The present section contains the results of accurate comparisons of bars of all these different metals. Bronze 28 is employed as the standard of reference. The list contains comparisons of between forty and fifty bronze bars, and of about thirty bars of other metals. In April, 1855, an unexpected anomaly presented itself to Mr. Sheepshanks. The difference between the standard-bar denominated "Bronze 28" and a bar of cast steel, when determined by the intermediation of comparisons with four iron bars, was found to be different from the result obtained by direct comparison of the two bars with each other. Finding it impossible to account for the whole of the discordance he was reluctantly led to suspect that Bronze 28 had sensibly shortened. And so deeply and so painfully was this impression fixed on his mind that he actually contemplated the rejection of all the results which had cost so many years of labour, and the commencing of the work *de novo*.

Further experiments, however, showed that his suspicion was unfounded, and established beyond doubt the general trustworthiness of the results already obtained by him.

"I have now," says the author, "to give the melancholy termination of this part of the work. To 1855, July 14 inclusive, the whole of Mr. Sheepshanks' observations, so far as I can discover, are transferred from the memorandum-book to the reduction-papers, and the means are taken. The final series of observations are the last two sets of comparisons of Bronze 17 with Bronze 28. After the observations of July 14, the following note is placed in the memorandum-book:

"'1855, July 25. I went to Reading on the 14th, and felt my head so deranged that I stayed there till Friday 20th, when I came up to arrange my apparatus. I did only a little in the latter affair, but I left pretty full instructions for W. S. [William Simms, junr.], who has, I believe, carried them out with his usual skill. I told him that the microscopes had not the same scale, and that D would require to be set a trifle lower. He preferred taking a bit off A, and so get them nearer to the same length, and to have the focus of distinct vision in the same horizontal line; but it will alter the scale of A.'

"Then follow a few observations (one page only, with a few subtractions on the next page) which I am not able to interpret with certainty. They are not dated.

"Mr. Sheepshanks had returned to London on some day in the week commencing with Monday, July 23, and had, as I conceive, made some trials of the apparatus, which were registered (for the moment only) in the unintelligible entries to which I have alluded. I understand that he was engaged on the apparatus on Saturday, July 28; but several persons had remarked that he was evidently in great distress. On the afternoon of that day he went to his residence at Reading, carrying with him a large travelling-bag full of papers relating to the Standards. In the afternoon of Sunday, July 29, while sitting in family society, he fell speechless from his chair, struck with apoplexy, and expired on August 4.

"Thus died—almost in the scene of his labours, and with his thoughts still intent on them—a man, whose equal in talent and perseverance, in disinterestedness, in love of justice and truth, I have scarcely known. He had, however, brought to a satisfactory termination the great division of the Standard-work which best suited his taste, having well overcome the last of the difficulties which had presented themselves, and leaving the work in such a state that not a single additional comparison of line measures was required."

Section VIII. *Formation of End-measure Bars.*—The form given to the end-bars depended in some degree upon the process adopted for comparing end-measure bars with line-measure bars, which originated in a suggestion of the author's. Suppose that we have *two* end-bars, each nearly equal in length

(as measured between its extreme ends) to the length of the line-bars (as measured from line to line). Suppose, also, that each of the end-bars has a well sunk to the middle of its depth, with a transverse line cut upon a pin (as in the line-measures); the line being not necessarily at the middle of the length of the bar, though it is essential that its distance from the centre of the bar's length be nearly the same in both end-bars. As, generally, one segment of the bar, from the transverse line to the end, will be longer than the other segment, let the longer segment be denoted by + and the shorter by —. And suppose that the two bars are placed end to end, the + end of one being opposed to the — end of the other. The distance between their transverse lines will evidently be the sum of the + segment of the first bar and the — segment of the second bar; and the conditions stated, as to the similar division of the lengths of the two bars, will make that sum so nearly equal to the length of the line-measure, that the difference may be measured by the micrometer-apparatus. Then interchanging the positions of the end-bars, so that their other ends shall be in contact, the distance under observation will be the sum of the — segment of the first bar and the + segment of the second bar. Adding together the two measured distances, we have a comparison of the sum of the two end-bars with double the length of the line-bar.

In order to make this determination available, it is necessary further to determine the difference between the whole lengths of the two end-bars. A felicitous suggestion by Mr. W. Simms, jun. gave the means of doing this. Mr. W. Simms's proposal was, to apply the process above described to *three* end-bars, determining the measure of the sum of each pair of end-bars in reference to double the length of the line-bar; then there are given three equations to find three unknown quantities. The length of each of the three end-bars is thus found without difficulty.

Mr. Sheepshanks had commenced the preparation of some end-bars, but no efficient progress was made in their completion up to the time of his decease. The Astronomer Royal, on then revising the condition of the work, requested Mr. Simms to undertake the whole operation of testing and adjusting the bars in his own workshops, and, finally, of making the definitive comparisons with Bronze 28 on Mr. Sheepshanks' apparatus at Somerset House.

Section IX. *Closing Proceedings of Official Character; Extracts from the Final Report of the Commission; Extracts from the Act of Parliament legalising the new Standard; Standard Temperatures for the Compared Bars; Disposal of the Bars.* —The final Report of the Committee, addressed to the Lords Commissioners of Her Majesty's Treasury, was signed on the 28th of March, 1854. We can only afford room for the following extracts:—

"20. The expansions of these bars corresponding to a given change of temperature had been sufficiently determined in the course of the experiments; and it was then judged expedient, instead of stating the difference in the length of the selected bars at the same temperature, to infer the difference of temperature which would cause all to represent the same length, by the application of which it would be possible to assign the specific temperature at which each bar represents precisely the length of one yard. Thus it was found that the length of one yard, as given by the lost Imperial Standard, is represented with no sensible uncertainty, except in the measures of the Imperial Standard itself, by the following bars, at the temperatures placed opposite to them:—

Bronze 19, or No. 1, at 62·00 Fahrenheit.
Bronze 20, or No. 2, at 61·94 Fahrenheit.
Bronze 2, or No. 3, at 62·10 Fahrenheit.
Bronze 7, or No. 4, at 61·98 Fahrenheit.
Bronze 10, or No. 5, at 62·16 Fahrenheit.
Bronze 28, or No. 6, at 62·00 Fahrenheit.

"21. The degrees of temperature for the use of these standards are defined as proportional to the corresponding apparent increase of volume of quicksilver in the thermometer-tube; the degree 32° representing the freezing-point of water; and the degree 212° representing the temperature of steam under Laplace's standard atmospheric pressure, or the atmospheric pressure corresponding to the following number of inches in the barometric reading reduced to 32° Fahrenheit:—

$$29{\cdot}9218 + 0{\cdot}0766 \times \text{cosine}(2.\,\text{latitude}) + 0{\cdot}00000179 \times \text{height in feet above the sea};$$

and the degree 62° denoting the temperature which produces in the quicksilver an apparent expansion equal to $\frac{30}{180}$ of the expansion between 32° and 212°; and so in proportion for other degrees.

"23. We propose that the bar No. 1 be adopted by the Legislature as the Parliamentary Standard of one yard; that Nos. 2, 3, 4, 5, be adopted as Parliamentary Copies, and that No. 6 be retained by some officer of the Government for the comparison of other bars, or for other scientific purposes in which reference to the Standard may soon be required. We will advert shortly to the proposed places of deposit of the Standards Nos. 1, 2, 3, 4, 5.*

"33. Remarking this probable intention of the Legislature in the year 1760, and remarking also the evident propriety of

* Reference is here made to a Bill which was introduced into Parliament in 1760, containing provisions for the safe custody of the standards of weight and measure.

placing the National Standards under the care of the Executive, we recommend that the Parliamentary Standards of one yard and one pound be deposited in the office of the Exchequer, there to be preserved under such regulations as the Parliament may affirm fitting.

"34. In selecting places for the preservation of the authentic copies of the Parliamentary Standards, we have been guided by a consideration of the general fitness of the offices named; by an appreciation of the careful and accurate habits of the persons employed in them; by the limited accessibility to standards preserved in such offices, which it may be presumed will be made available for their legitimate purposes only; and by the occasional utility of accurate standards in verification of the operations to which these offices are devoted. . . . We adhere to the recommendation in Article 6 of the Report of 1841, that one set of copies should be imbedded in the masonry of a public building. In the distribution of the different copies, we have been guided by trifling peculiarities in the copies themselves.

"35. After careful consideration, we recommend,—

"That the copy of Length Standard No. 2, and the copy of Weight Standard, PC, No. 1, be deposited in the Royal Mint.

"That the copy of Length Standard, No. 3, and the copy of Weight Standard, PC, No. 2, be transferred to the Royal Society.

"That the copy of Length Standard, No. 5, and the copy of Weight Standard, PC, No. 3, be deposited in the Royal Observatory of Greenwich.

"That the copy of Length Standard, No. 4, and the copy of Weight Standard, PC, No. 4, be immured in the cill of the recess on the east side of the Lower Waiting Hall in the New Palace at Westminster."

On the question of referring the values of the measure and weight represented by the standards to natural elements.

"40. After due consideration of this question, referring to the reasons explained in Chapter II. of the Report of 1841, December 21, we adhere to the recommendation contained in that chapter, and embodied in articles 1 and 2 of the same Report, that no reference be made to natural elements for the values represented by the standards.

"41. We consider the ascertaining of the earth's dimensions and of the length of the seconds pendulum in terms of the standard of length, and of the weight of a certain volume of water in terms of the standard of weight, as philosophical determinations of the highest importance, to the prosecution of which we trust that Her Majesty's Government will always give their most liberal assistance; but we do not urge them on the Government at present as connected with the conservation *of standards.*"

The Act of Parliament for legalising the Standards of Weight and Measure received the Royal Assent on the 30th of July, 1855, the day succeeding that on which Mr. Sheepshanks was attacked by the illness of which he died.

The Astronomer Royal closes his paper with the new nomenclature of the bars, and the temperatures at which they represent the British yard. He has also given a statement of their distribution. Bar, No. 6, has been retained at the Royal Observatory, as accessible representative of the National Standard.

A Treatise on Navigation and Nautical Astronomy. By John Riddle, F.R.A.S., Head Master of the Nautical School, Royal Hospital, Greenwich. 8vo. Seventh Edition. London, 1859.

This excellent standard work, in which theory and practice are so judiciously blended together, is too well known to require any detailed statement of its contents. The present edition is the result of a careful revision, by which the work is adapted to the best modern practice of the art of navigation.

ERRATA.

Page 198, *for* $c = 280{\cdot}4 \pm 83$, *read* $c = 280{\cdot}4 \pm 8{\cdot}3$.

— — *for* $\delta r = (\ldots) \sin^2 \lambda$, *read* $\delta r = (\quad) \sin^2 2\lambda$.

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London:—Printed by G. Barclay, Castle St. Leicester Sq. and Published at the Apartments of the Society, June 8, 1859.

ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *June* 10, 1859. No. 8.

Rev. R. MAIN, President, in the Chair.

Rev. Dr. Booth, Incumbent of Stone, Bucks;
Christopher George, Esq., 5 Barnes Street, Limehouse;
F. H. Elliott, Esq., 56 Strand;
Thomas Cooke, Esq., York;
Rev. Francis Redford, Silloth by Carlisle; and
J. E. Richard, Esq., West Hill, Wandsworth,

were balloted for and duly elected Fellows of the Society.

Occultation of Saturn by the Moon on May 8, as observed at the Cambridge Observatory. By Professor Challis.

The observations were taken by myself and my senior assistant, Mr. Bowden. I employed the Northumberland telescope, with a magnifying power of 215, the junior assistant, Mr. Christy, counting the seconds from a sidereal clock; and Mr. Bowden observed with the telescope of the 5-feet equatoreal, using a magnifying power of 120. The atmospheric circumstances were favourable. The following are the Greenwich mean times of observation, the letters C and B denoting the observers.

Disappearances.

	C.	B.	
	h m s	s	
Contact of Saturn's limb.....	8 17 29·58	.....	(*a*)
Complete immersion of Saturn ...	8 17 63·09	59·84	(*b*)
,, ,, of outer ring	8 18 13·66	12·70	(*c*)

Reappearances.

	C.	B.	
	h m s	s	
First contact of outer ring	9 16 2·01(?)	.....	(*d*)
,, ,, of Saturn.....	9 16 23·95	23·79	(*e*)
Complete emersion of Saturn	9 16 53·47	60·19	(*f*)
,, ,, of outer ring...	9 17 13·81	11·66(?)	(*g*)

(*a*.) The occultation occurring earlier than was expected, the first contacts of the planet and ring were lost. The time noted by C is that of coincidence of the moon's limb with that point of *Saturn's* limb which was judged to be on the axis major of the ring. (*b*) and (*c*.) These times were considered to be "good."

(*d*.) This time is very doubtful. The limb of the ring was partially obscured at emergence by the micrometer wires set to indicate where to look for it, and had evidently projected a few seconds of arc from the moon's limb when the time was noted. (*e*) and (*f*.) C's times were considered "pretty certain:" those of B were "extremely unsatisfactory." (*g*.) The limb of the ring appeared to C to separate suddenly from the moon's limb, and the recorded time was thought to be late: B considered his observation of no value, the edge of the moon being badly defined, from an effect of the telescope, it was supposed, and not from atmospheric causes.

The time of disappearance taking me by surprise, I had scarcely an opportunity of making physical observations in addition to notes of time. I remarked, however, that the moon's limb, where it crossed the middle of *Saturn* and his rings, appeared to be unusually curved, and that both the ball and the ring were reduced before disappearance to a very narrow strip of light. The disappearance seemed to linger. I noted also that the moon's limb was fringed by a darkish border; but I have reason to say that this was merely an effect of the Ramsden eyepiece that was used, the planet, in the hurry, not being placed in the middle of the field. Mr. Bowden remarked as follows: "I did not perceive the slightest change in the appearance of *Saturn*, except just before the total disappearance, when the edge of the ring lost its sharpness of definition, and I thought I saw it wave to and fro. I also thought that just before the total disappearance the edge seemed to hang on the moon's limb, and that the final disappearance was retarded."

The circumstances of the reappearance were very favourable for observing physical phenomena. The moon's limb, as seen in the Northumberland telescope, was steady, and the irregularities were sharply defined. I looked particularly to see whether *Saturn's* disk was crossed by a dark band like that noticed at the occultation of *Jupiter* on Jan. 2, 1857. Nothing of the kind was visible. At the place of reappearance the moon's limb was neither depressed nor elevated, but accorded with the general contour. When the planet was a little separated from the moon's limb, a regular increase of its brightness from the parts nearest the limb to the parts more remote was very observable, and was probably the effect of the dispersed light, which in a telescope is usually seen near the moon's bright limb. The colour of the planet, which very much resembled that exhibited by *Jupiter* at the occultation above mentioned,

was in remarkable contrast with the whiteness of the moon's light. Mr. Bowden described it as "a dull earthy colour."

From calculations of the occultation made according to the formulæ given in the appendix to the *Nautical Almanac* for 1854, I have obtained results as follows. Let x and y be the corrections of the tabular R.A. (in arc) and tabular N.P.D. of the moon's centre; e and f the like quantities for *Saturn's* centre; $S \times (1 + 0{\cdot}001\,n)$ the moon's true apparent semi-diameter; c the distance of *Saturn's* centre from the moon's limb; D the distance between the centres. Then the tabular quantities being calculated from the *Nautical Almanac* for the Greenwich mean time, $8^h\ 18^m\ 3^s{\cdot}09 + t$, and the latitude and longitude of the observatory, and the moon's parallax being assumed to require no correction, the following equation of condition was obtained,

$$S + e - D + 0{\cdot}465\,t = -0{\cdot}612\,x - 0{\cdot}774\,y + 0{\cdot}609\,e + 0{\cdot}764\,f - 0{\cdot}985\,n$$

The values of c for the contacts of *Saturn* were calculated from the equatoreal diameter adopted in the *Nautical Almanac*, and an assumed ellipticity of $\frac{1}{13}$; and the value for the contact of the ring was calculated from the data in p. 510 of the *Nautical Almanac* for 1859. The three values were found to be $+4''{\cdot}72$, $-8''{\cdot}30$, and $-12''{\cdot}24$. Hence the numerical value of the left-hand side of the above equation as given by C's three observations is $-2''{\cdot}04$, $+0''{\cdot}61$, and $+1''{\cdot}58$; and as given by B's two observations, $-0''{\cdot}90$ and $+1''{\cdot}13$.

The analogous equation of condition for the reappearance, calculated for the Greenwich mean time $9^h\ 16^m\ 38^s{\cdot}3 + t$ is the following,—

$$S + c - D - 0{\cdot}494\,t = +0{\cdot}944\,x + 0{\cdot}098\,y - 0{\cdot}939\,e - 0{\cdot}092\,f - 0{\cdot}983\,n$$

The values c calculated for the four contacts from the same data were found to be $\mp 19''{\cdot}75$ and $\mp 8''{\cdot}71$; and accordingly the numerical value of the left-hand side of the equation, as given by C's four observations is, $-4''{\cdot}35$, $-3''{\cdot}50$, $+0''{\cdot}21$, $+1''{\cdot}80$; and as given by B's three observations, $-3''{\cdot}37$, $-3''{\cdot}11$, $+2''{\cdot}86$.

These results are not all entitled to the same weight. For instance, C's first time, from the nature of the observation, was liable to error; and B's time for the complete immersion of *Saturn* is plainly too early, probably by an error in recording. Omitting these, the mean result for the other three times is $+1''{\cdot}11$. Again, the first of C's times of reappearance, and the last of B's, may be rejected, as being considered at the time of no weight; and the remainder will then give the mean result $-1''{\cdot}59$. Thus the two equations of condition become,

$$+1'\cdot11 = -0\cdot612\,x - 0\cdot774\,y + 0\cdot609\,e + 0\cdot764\,f - 0\cdot985\,n$$
$$-1\cdot59 = +0\cdot944\,x + 0\cdot098\,y - 0\cdot939\,e - 0\cdot092\,f - 0\cdot983\,n$$

These equations, which assume the correctness of the dimensions of *Saturn* and his ring given in the *Nautical Almanac*, might be used to find n when x, y, e and f are known.

On the Present State of the Controversy respecting the Amount of the Acceleration of the Moon's Mean Motion. By the Rev. R. Main, President.

There is probably no astronomical subject in which the advance or improvement within the last twenty-five years has been so great as in the lunar theory, and in the means taken for increasing the number of observations necessary for bringing the theory to perfection. To begin with observations: during the whole of this period the moon has been observed at Greenwich on the meridian with inflexible rigour, and with an amount of precision which was never previously attempted; and during a portion of it, that is, since the year 1847, a series of observations have been made by an instrument especially designed for the purpose, by which the gaps in the meridian observations, necessarily occurring for about three hours before and three hours after each conjunction, have been in a great measure filled up. These results of observation are, of course, independent of those obtained at other great observatories, which are, on the whole, not inconsiderable, though of very inferior importance.

But, however remarkable be the quantity of materials for the correction of the lunar orbit during the latter portion of the present century, the use which has been made of them is still more so. The gigantic work undertaken by the Astronomer Royal, of reducing, on a uniform plan and with the best modern elements, the whole of the lunar observations made by his predecessors, for a period of eighty years, namely, from 1750 to 1830, comes first in order, as more nearly related to the observations, and is, perhaps, absolutely first in order of importance. The first result from the publication of these observations was the discovery, by Professor Hansen, of the long inequality produced by *Venus*, and the second has been the completion of his invaluable Tables. It is too early to predict that the errors of these tables in all parts of the orbit are so small that all future improvements may be considered to belong to speculative rather than practical science; but this is known already, that, in a rigorous comparison of the *Greenwich Observations* for the year 1852, the errors rarely exceed those which might be due in a great measure to faulty observations; and the scientific public will look with great interest and anxiety for the results of the completion of Mr. Hind's

laborious and self-imposed task of comparing all the *Greenwich Observations* from the year 1847 to the present time with the Tables.

In all the improvements of the lunar theory yet mentioned, observation has a very great share; but there is one development of a purely theoretical character of great importance in itself, but of still greater interest on account of the controversy to which the conflicting results arrived at by most eminent analysts have given rise. This inequality is the secular acceleration of the moon's mean motion, of which the fact has been known for nearly two centuries, but concerning the amount of which there is even at the present time a doubt of so interesting a character, whether it be regarded practically or theoretically, that a statement of the leading features of the state of the controversy seems to be not only desirable, but absolutely necessary.

Dr. Halley appears to have been the first astronomer who was acquainted with the fact of the acceleration of the moon's mean motion, though it is not likely that he could, from the elements of the lunar orbit which had been obtained from observation in his day, give any guess as to its probable amount. His note on this occurs in a paper containing "Some Account of the Ancient State of the City of Palmyra, &c. &c." (*Phil. Trans.* for 1695, No. 218.) "If any curious traveller," he says, "or merchant residing there, would please to observe, with due care, the phases of the moon's eclipses at Bagdat, Aleppo, and Alexandria, thereby to determine their magnitudes, they would do the science of astronomy a greater service; for in and near these places were made all the observations whereby the middle motions of the sun and moon are limited; and I could then pronounce in what proportion the moon's motion does accelerate, and shall (God willing) one day make it appear to the public."

The next paper of importance, as proving the fact, and with tolerable approximation determining the amount of the acceleration, is that of the Rev. Richard Dunthorne, given in the *Phil. Trans.* for 1749.

Comparisons of the Tables with four well-known eclipses observed by Albategnius gave no certain result, on account of the want of a knowledge of the position of the place of observation; but there are, fortunately, observations of two ancient eclipses which are very favourable for the determination: these are, first, the eclipse of the moon said by Hipparchus to have been observed at Babylon in the 366th year of Nabonassar, or in B.C. 383, Dec. 22; and, secondly, the eclipse of the moon which was observed at Alexandria, B.C. 201, Sept. 22. In each of these cases it is found that a considerable correction must be made to the place of the moon calculated with a uniform motion, to represent the circumstances of the eclipse. The very ancient eclipse of the moon, observed at Babylon

B.C. 721, March 19, as related by Ptolemy, confirms the acceleration, as, for this eclipse, the true place of the moon precedes the compared place by a quantity equivalent to fifty minutes of time. From a careful discussion of these eclipses, Mr. Dunthorne thinks that the coefficient of the acceleration is not greatly different from 10″.*

The paper of Mr. Dunthorne here quoted is, at the present time, valuable only as showing that the lunar tables, even so long ago as the middle of the preceding century, were sufficiently precise to put in evidence the fact of the acceleration, and to enable its amount to be assigned with tolerable accuracy.

Recently Mr. Airy has had occasion to discuss three of the most remarkable eclipses of antiquity, namely, the eclipse of Agathocles, the eclipse of Larissa, and the eclipse of Thales,† with the advantage of the use of very much better solar and lunar tables than any which were previously employed, namely, the solar tables of Hansen and Olufsen, and the lunar tables of Hansen. This paper is so well known that any remarks upon it are at present superfluous, and we are only concerned with the results as bearing on the amount of the lunar acceleration.

These results the Astronomer Royal states as follows :—

1. The eclipse at Larissa, — 556, May 19, is established as a real eclipse at a well-defined point, and may be adopted for critical reference in deciding on the value of lunar tables, as applicable to distant places of the moon.

2. Professor Hansen's Tables very well represent the phenomena of the three eclipses of Agathocles, Larissa, and Thales, &c.

3. If any change is permitted in the two elements, of secular acceleration of longitude, and change of the argument of latitude, it must be of the nature of increasing the acceleration, and increasing the argument of latitude in the distant ages.

In an addendum to this paper it is announced that the eclipse of Stiklastad confirms the preceding result, namely, that if any correction is made to the coefficient of the acceleration, it must be by *way of increase, and not of decrease.*

It may, then, be taken for granted that the coefficient of the acceleration must be *at least as great* as that used by Professor Hansen in his Tables, namely, 12″·12.

We now come to the consideration of the controversy

* In addition to this paper by Dunthorne on the existence of the secular acceleration of the moon's mean motion, as exhibited by ancient eclipses, a very interesting paper by La Place, printed in the *Connaissance des Tems* for 1800, "On the Secular Equations of the Motions of the Apogee and Nodes of the Lunar Orbit," may be consulted. In this paper La Place announces his discovery of the secular equation of the lunar apogee, and gives the results of comparison with twenty-seven ancient eclipses, compared with the tables, at his request, by M. Bouvard.

† "On the Eclipse of Agathocles, the Eclipse at Larissa, and the Eclipse of Thales, &c." By G. B. Airy, Esq., Astronomer Royal. (*Mem. Ast. Soc.*, vol. xxvi.)

respecting the amount of this coefficient, as derived from theory, — a controversy vitally interesting and important; both as regards the great names that are ranged on different sides, that is, in support of the commonly received coefficient on the one side, and of that recently insisted on by Mr. Adams on the other; and as regards the accuracy of those analytical processes employed in the different investigations which have given rise to the present dispute. In support of the old coefficient are the great names of Plana and Pontécoulant, who deduce a coefficient very nearly equal to that required to satisfy the ancient eclipses; while Professor Hansen, by using an essentially different method, arrives at nearly the same result. On the other side are Adams and Delaunay, who, pursuing processes totally different, and carrying their expansions to a degree never before attempted, arrive at a result smaller by one-half than that commonly received and contended for by their opponents. That which renders the present controversy still more interesting is the fact that the astronomers named above are, with very few exceptions, the only *savans* in the world at the present time who have such complete mastery of the complicated and difficult analysis necessary for the solution of the problem as to enable them to judge of the merits or defects of the various processes employed, which, from the discordant results, cannot all be correct.

In endeavouring to lay before the Society the present state of the question, it is necessary to refer to two former short articles on this subject, written by me, and contained in the sixteenth volume of the *Monthly Notices* of this Society, and a very brief recapitulation of their contents may be useful. M. Plana had been induced by Mr. Adams's paper, published in the *Phil. Trans.* for 1853,* to review his theory of the secular acceleration given in the first volume of the *Théorie du Mouvement de la Lune*, and imagined that he had completely reconciled his own value of the coefficient with that given by Mr. Adams, by the detection of an error at pages 60 and 61 of the *Théorie*. He investigates at great length the terms which had been omitted, and arrives at a result identical with that of Mr. Adams, as far as the latter had carried his development. He also intimated his intention of carrying the expansion to terms of the seventh order. This memoir is dated April 16, 1856. In June 1856, however, M. Plana published another memoir, *Sur l'Equation Séculaire du moyen Mouvement de la Lune*,† in which he gives the additions to be made to his formula for the acceleration given in the first volume of the *Théorie* on account of the error before adverted to, but in which he seems to deny, what he had before admitted, the

* "On the Secular Variation of the Moon's Mean Motion." By J. C. Adams, Esq., M.A., F.R.A.S.

† Both papers of Plana are extracted from the *Turin Memoirs*.

correctness of the reasonings and the results of Mr. Adams. This whole paper is so obscure, and the references to his own former researches and to the results of Mr. Adams are so indistinct, that nothing can be gained from it as to the probable correctness of his own decision; and all that is learnt from the paper is, that he assumes that Mr. Adams has taken into his computations some terms which do not properly belong to the secular equation of acceleration. It was imagined by astronomers at the time of the writing of this memoir, that is, in the year 1856, that M. Plana would attempt to confirm his views by an elaborate discussion of the whole subject; for, from some hints which occur in his previous memoirs, it was plainly his intention to carry the approximation to terms of the seventh order. Nothing, however, has since appeared from his pen (at least I am not aware of any publication) bearing on this subject; and it only remains to show what has been the nature of the researches and the results of his opponents, Adams and Delaunay.

The paper in which Mr. Adams first announced his discovery that the usually received coefficient of the acceleration was incorrect, has been before quoted; and it is due to its author to state, in proof of the clearness of his views, that he indicates in a geometrical and very simple form the mistake which he assumes to exist in the ordinary theory. This theory, though it has been much developed by Plana, is that of La Place without essential alteration;* and therefore, whatever objection is fatal to La Place's theory, is fatal to the correctness of the deductions of that astronomer, to whatever degree of development they may be carried.

For the sake of such members of the Society as may not be familiar with the cause of the acceleration of the moon's mean motion, it may be worth while to state the leading features of La Place's theory.

The time of revolution of the moon round the earth depends upon the amount of her gravitation towards the earth; and, if her orbit were not disturbed by the sun and planets, it would be absolutely constant. The sun's disturbing force, however, on the whole, during a revolution, diminishes the central or radial force, and her orbit is, therefore, larger and her period greater than it would be if she were undisturbed. The period will, therefore, depend upon the sun's distance from the earth, and will be different for the different months of the year, but will have a mean value which will be pretty nearly constant for each year. On account, however, of the decrease of the excentricity of the earth's orbit, the sun's average distance in any year is slightly greater than it was in the preceding, and his disturbing power diminishing the moon's gravity towards the earth is less, and therefore the moon's

* See Adams's Paper on the Secular Acceleration, referred to above, *Phil. Trans.* for 1853, p. 398.

gravitation towards the earth is greater, and her motion becomes accelerated.

La Place, with his usual sagacity and power of analysis, deduced the effect of this secular disturbance in the mean motion; but he concluded, Mr. Adams thinks erroneously,* that the *radial force* only is concerned in producing this effect, and that the equal description of areas would not be affected by it, the tangential force producing no effect. It is a curious fact that in the infancy of the lunar theory a similar mistake was made with respect to the motion of the moon's apogee. It was assumed correctly that only the radial disturbing force was concerned *directly* in producing the motion of the apse, but it was not taken into account that the tangential force, by diminishing or increasing the velocity, caused the position of the moon to be at a given time different from what it would otherwise have been, and therefore *indirectly* altered the radial force, and tended to produce a motion of the apse: and it is remarkable that this indirect effect of the tangential disturbing force was in this case of the motion of the apogee equal to the direct effect of the radial disturbing force; and that Clairaut by this discovery relieved men's minds from one of those periodical panics, which, ever since the publication of the *Principia*, have at intervals seized the minds of astronomers with regard to the absolute truth of the theory of gravitation.

It is then with this radical defect in omitting the effect of the action of the tangential part of the secular disturbing force arising from the diminution of the earth's excentricity, that Mr. Adams charges the theory of La Place, and therefore the processes of Plana and Pontécoulant, and he proceeds in a very simple and satisfactory manner to show how he introduces into his analysis the additional terms which will thence arise.

By a correspondence with the Astronomer Royal, to which I have had access, it appears that ever since the publication of his paper he has at intervals been steadily employed in developing his results to terms of higher order, and he has recently published the result of his researches by giving the value of his coefficient of acceleration correctly, as he believes, to terms of the seventh order. This result was first announced in a letter to M. Delaunay, which is printed in the *Comptes Rendus* (vol. xlviii. p. 247).

He there gives for his expression, from which the coefficient of acceleration is deduced:—

$$\frac{dn}{n\,dt} = \frac{e'\,de'}{dt}\left\{-3m^2 + \frac{3771}{32}m^4 + \frac{34047}{32}m^5 + \frac{30685}{48}m^6 + \frac{17053741}{576}m^7\right\}$$

* It was evidently the intention of La Place to carry this development of the function expressing the acceleration only to the second power of m, within which limit the equality of the areal velocity is not affected; and, therefore, it cannot be assumed that any mistake was committed by him in his investigation; but the same apology will not apply to his followers.

and hence, he says, the coefficient of the acceleration is $10''{\cdot}66 - 2''{\cdot}34 - 1''{\cdot}58 - 0''{\cdot}71 - 0''{\cdot}25 = 5''{\cdot}78$.

On this result Mr. Adams remarks that "the convergence of this series, though slow up to this point, converges more rapidly afterwards, and there is no fear that the sum of the terms neglected should exceed $0''{\cdot}08$."

This remark is very important, because, if the convergence of the series be admitted, there can be very little doubt of the correctness of the result, since, as will shortly be seen, Delaunay's expression is identical with that of Adams, and Professor Hansen can imagine only the failure of convergence as producing what he calls, perhaps prematurely, the error of Delaunay.

Before proceeding to the consideration of Delaunay's method and results, it will be well to give the substance of a letter from Mr. Adams to the Astronomer Royal, which contains one of the most curious and interesting specimens extant of the nature of the errors which will, when all pains have been used to avoid them, creep into computations of this kind, and will at the same time show the absolute identity of the results of the two astronomers.

On receiving from M. Delaunay his expression for the secular acceleration, Mr. Adams found a discrepancy between it and his own value in the coefficient of m^7, of which he was enabled to trace the origin, and which we give in his own words, premising that M. Delaunay's term was $\frac{17103741}{576}\,m^7$, and that of Mr. Adams $\frac{17053741}{576}\,m^7$.

"The simplicity of the difference," he says, "between these two quantities, when put under the form $\frac{100000}{1152}$, shows that it is caused by a mistake in a figure in the numerator of one of the fractions which go to form the coefficient. Accordingly, yesterday I looked over my old calculations, and soon found that there was an error of this nature, and that when this was corrected, my result perfectly agreed with M. Delaunay's. The error took place, as it not unfrequently happens, in one of the simplest parts of the calculation, namely, in dividing by $2 - 2m$, in order to obtain the coefficient of the term in the moon's longitude involving $\frac{e'\,dc'}{n\,dt}\cos 2\xi$ (Pontécoulant's notation). The last term in this coefficient I had inadvertently written as $\frac{3372845}{10368}\,m^5$ instead of $\frac{3572845}{10368}\,m^5$. The correction to be applied is, therefore, $\frac{100000}{5184}\,m^5$; and the corresponding correction to be applied to the multiplier of $e'\frac{de'}{dt}$ in the value of $\frac{dn}{n\,dt}$ is the above correction multiplied by $\frac{9}{2}\,m^4$. Hence this correction $=\frac{100000}{1152}$, which is just the difference between M. Delaunay's result and mine. This agree-

ment between the results found by quite different methods is very satisfactory."

In the *Monthly Notice* for April last will be found a short paper by Mr. Adams, giving the results at which he had arrived, including the erroneous coefficient alluded to above; and giving also the secular variations of the excentricity and inclination.

We will now turn to the investigations of M. Delaunay.

The papers in which he gives his accounts of the methods employed in his new lunar theory are contained in the 46th, 47th, and 48th volumes of the *Comptes Rendus*. His work appears, from the account which he has given of it, to be one of enormous magnitude, and to have occupied him already for eighteen years. It is now so nearly completed that he is able to give hopes of the speedy printing and publication of it. In the meanwhile he has given a sufficiently explicit account of it to enable us to judge of the chief points of difference between his methods and those of his predecessors in lunar investigations. These are, 1st, that he expresses immediately the true longitude, radius vector, &c. in terms of the time in conformity with the methods of Lubbock, Poisson, and Hansen, but in opposition to those of Plana and Damoiseau; and this mode of proceeding he looks upon as the most natural and manageable. 2dly, He expresses the coefficients of all the lunar inequalities in a literal shape expanded in series, in opposition to Hansen, who, in his *Fundamenta Nova Investigationis Orbitæ veræ quam Luna perlustrat*, insists on the necessity of expressing them numerically. 3dly, He has simplified the processes of the analytical development by replacing the large and complicated operations which occur by a much larger number of smaller and much easier operations.*

It has been stated that Mr. Adams' results were exactly corroborated by those of M. Delaunay, and that a correspondence had been commenced between those astronomers: it has also been mentioned how a singular error made by Mr. Adams was detected and corrected; and it now only remains to be stated that, in the *Comptes Rendus* for 1859, April 25, M. Delaunay has given his final result, and accompanied it with a very lucid popular explanation of the whole subject. The reader, who desires to see the exact expression for the acceleration arrived at by M. Delaunay, must consult the work referred to, as it is too long for insertion here, consisting, as it does, of *forty-two* terms. Thus much may be said of it, that it takes in every possible term as far as the eighth order, including those terms depending on the inclination and excentricity of the lunar orbit; and that all the terms calculated by Mr. Adams are in exact accordance with it.

* See *Comptes Rendus* for 1858, May 17 and May 24, vol. xlvi., pp. 912 and 953; also for 1858, Nov. 22, vol. xlvii. p. 813; and for 1859, Jan. 17, Jan. 31, and April 25; vol. xlviii. pp. 137, 247, and 817, respectively.

I would finally draw attention to two letters of M. Pontécoulant and Professor Hansen, printed in the *Monthly Notices* for May last, which were addressed, the one to myself as President of the Society, and the other to the Astronomer Royal, both contending for the accuracy of their own methods, and assuming the incorrectness of the processes of M. Delaunay and Mr. Adams.

The question becomes, therefore, one of extreme interest, and astronomers will, perhaps, have to watch patiently for some time for additional elucidations of the discrepancy. At present we may say that, as far as we can judge from what is before us of the theoretical investigations of all concerned, Adams and Delaunay seem to have right on their side.

Mr. Adams, with his usual clearness, pointed out in a geometrical shape the error which had been committed in all previous investigations, and M. Delaunay, using a totally different method, has arrived at precisely the same result. It is, therefore, incumbent on the opponents, in the first place, to exhibit the incorrectness of the reasoning of Mr. Adams,—that is, to show clearly that the tangential force, producing alterations of the areal velocity, can produce no effect, before proceeding farther to establish their own results. Thus far the field of battle becomes very much limited, and we shall look with anxiety for the paper which M. Pontécoulant has promised to publish in support of his *réclamation*.

But a still greater difficulty awaits us if we accept Mr. Adams' value of the coefficient, and the principles of his investigation be established beyond controversy, as, in my own mind, I have very little doubt they will be. This value is far too small to satisfy the ancient eclipses, and therefore some other cause (such as a resisting medium) totally different from the disturbing influences of the sun and planets, must be resorted to, or we must hope, from a hint dropped by M. Delaunay at the end of his paper, that he has some means at present kept out of sight for laying the ghost which he has helped to raise. "En effet," he says, "l'accélération séculaire de la lune n'est pas le seul élément du mouvement de cet astre dont la valeur influe directement sur l'explication complète d'une éclipse de soleil anciennement observée: le mouvement du nœud de l'orbite de la lune joue un rôle important dans cette explication, et sa valeur n'est pas tellement fixée, qu'elle ne soit pas susceptible de recevoir une certaine modification; le moyen mouvement de l'astre lui-même, tel qu'on le déduit des observations modernes, peut être rendu inexact par suite de l'existence de certaines inégalités à longue periode, dont la grandeur n'est pas encore parfaitement connue. Avant d'aller plus loin, il est nécessaire d'examiner complètement chacun de ces deux points importants de la question: c'est ce que je me propose de faire dans de prochaines communications."

Results of Observations of Small Planets, made with the Transit Circle at the Royal Observatory, Greenwich, during the month of May, 1859.

(*Communicated by the Astronomer Royal.*)

Astræa (5).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 4	8 44 34·1	11 33 22·50	80 29 5·86

Flora (8).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 6	12 49 51·6	15 47 13·42	101 31 28·05
12	12 20 7·4	15 41 3·65	101 13 58·92
14	12 10 8·6	15 38 56·26	101 8 32·96
23	11 25 12·9	15 29 22·23	100 47 34·96
25	11 15 16·7	15 27 17·47	100 43 54·96
30	10 50 36·7	15 22 16·21	100 36 25·91

Metis (9).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 4	11 30 57·5	14 20 13·22	99 15 10·33
6	11 21 10·2	14 18 17·39	99 9 50·13
9	11 6 33·6	14 15 28·08	99 2 24·11
11	10 56 52·8	14 13 38·80	98 57 49·03
12	10 52 3·8	14 12 45·58	98 55 44·47
13	10 47 15·4	14 11 52·97	98 53 39·01
14	10 42 28·3	14 11 1·56	98 51 48·02
23	10 0 12·0	14 4 7·36	98 39 21·49
25	9 51 2·4	14 2 49·35	98 37 59·08

Irene (14).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 5	9 25 37·8	12 18 29·46	77 8 28·96
6	9 21 31·0	12 18 18·50	77 14 38·26
11	9 1 24·9	12 17 51·93	77 48 23·55
13	8 53 34·4	12 17 53·25	78 3 15·43
14	8 49 41·8	12 17 56·52	78 10 57·68

Fortuna (19).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 6	14 15 51·0	17 13 26·87	111 40 7·80
26	12 41 10·8	16 57 22·25	111 4 59·75

Calliope (22).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 4	9 6 54·6	11 55 46·65	72 33 55·22
5	9 2 42·0	11 55 29·94	72 38 58·55
6	8 58 29·9	11 55 13·68	72 43 23·52

Themis (24).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 4	10 40 17·3	13 29 24·71	99 14 46·19
6	10 31 10·6	13 28 9·56	99 8 10·46
11	10 8 40·8	13 25 18·90	98 52 41·99
12	10 4 13·5	13 24 47·42	.....

Bellona (28).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 6	11 33 48·2	14 30 57·51	90 2 46·85
11	11 10 17·4	14 27 5·56	89 46 46·62
12	11 5 37·1	14 26 21·09	89 44 3·95
25	10 6 8·2	14 17 57·66	89 25 56·86

Europa (52).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, May 6	12 55 1·3	15 52 23·94	99 43 26·63
11	12 31 37·2	15 48 38·79	99 30 18·84
12	12 26 55·9	15 47 53·20	99 27 53·67
14	12 17 31·6	15 46 20·49	99 23 2·53
21	11 44 34·6	15 40 53·96	99 8 25·40
23	11 35 10·1	15 39 21·10	99 4 53·46
25	11 25 47·3	15 37 49·85	99 1 37·50

Occultation of Saturn, as observed at Hartwell, 1859, May 8. By Norman Pogson, Esq.

(*Communicated by Dr. Lee.*)

	Greenwich Mean Time. Immersion.	Emersion.
	h m s	h m s
Saturn, western limb	8 18 0·1	9 16 26·2
Saturn, eastern limb	8 18 33·4	9 16 58·2
Eastern extremity of ring ...	8 18 42·1	9 17 16·1
Titan	8 21 21·7	.....
Star (about 8 mag.)	8 22 37·8	.....

The times of immersion and emersion of the western extremity of the ring were not recorded, but the other phenomena are considered pretty exact. *Titan* and the star following both disappeared instantaneously, but were neither of them visible until some distance from the moon's bright limb at their reappearance. No distortion of moon or planet was at any time perceived, though carefully looked for. The chief division of the rings was quite as distinct even up to the moon's limb as on any ordinary occasion. The disk of *Saturn* assumed a dull sea-green tint at emersion, much deeper and less luminous than that of *Jupiter* when similarly situated on Jan. 2d, 1857. On that occasion *Jupiter* was estimated to be a little brighter than the central part of *Mare Crisium*, while *Saturn* compared with the same spot was decidedly fainter. Five minutes after emersion, however, the unusual tint had considerably diminished, and the planet was fast recovering its general appearance. The evening was clear and calm as could be desired. The observations were made with the Hartwell equatoreal, charged with a power of 118.

Occultation of Saturn by the Moon, May 8, 1859. Observed by F. Morton, Esq., at Wrottesley Observatory.

(*Communicated by Lord Wrottesley.*)

The telescope employed was the 11-foot equatoreal, power 190. The definition of the planet was very fair, and no distortion was perceived during the occultation. Satellite I. did not disappear suddenly, as did the other, but its light had sensibly diminished before its final disappearance.

The satellites are designated in the order of their proximity to the planet.

The first reappearance of the ring was not caught, owing to the faintness of the planet as compared with the moon. The ring seemed to cling slightly to the edge of the moon before they separated.

Phenomenon.	Local Sidereal Time. h m s	Local Mean Solar Time. h m s
First contact with ring	11 11 10·4	8 6 40·6
Total disappearance of ring	11 11 55·9	8 7 25·9
Disappearance of Satellite I.	11 14 34·4	8 10 4·0
,, Satellite II.	11 15 52·9	8 11 22·3
First reappearance of ball	12 9 55·4	9 5 15·9
Last contact with ball	12 10 27·4	9 5 47·8
Last contact with ring	12 10 54·4	9 6 14·8

On the Successive Illuminations of the Lunar Crater Geminus.

By W. R. Birt, Esq.

A few years since a Committee of the British Association for the Advancement of Science was appointed to examine into the physical character of the moon's surface. Among the suggestions and recommendations offered by the Committee is that of the examination of any particular part of the lunar disk under at least three aspects, viz., a morning or forenoon, a midday, and an evening illumination. Having lately directed my attention to the crater *Geminus*, and observed it during *two* lunations under the three aspects recommended, I have been so struck with a somewhat remarkable feature that has presented itself in the midday illumination, that in the hope of a more powerful examination being made of it than I am able to effect, I am induced at once to lay the details before the Society rather than wait for fuller details resulting from any future observations I may make, which delay may possibly occasion an omission of a notice of it entirely.

The crater is situated in the north-west quadrant of the moon *as seen from the earth*, and northward of the *Mare Crisium*. It has to the westward of it a smaller crater named *Bernouilli*. Under a morning illumination it presents an elliptical outline, the western wall being in shadow, the eastern being more or less strongly illuminated by the sun's rays according to their obliquity. A bright spot exists between it and *Bernouilli*. The floor of the crater *Bernouilli* is very much darker than that of *Geminus* at a period of the lunar day corresponding to 7 days of the moon's age, while the space between *Geminus* and *Bernouilli* presents about the same reflective power as the floor of *Geminus*. A dark space of the same intensity as the floor of *Bernouilli* extends northward of this crater. These features are exhibited on the drawing* representing the forenoon illumination on May 9, 1859, between 8 and 10 hours, G.M.T., at 9 P.M. of that day the moon's age was 6·96 days.

Three days afterwards, viz. on May 12, between 7·30 and

* This drawing and the other, to which he refers were exhibited at the meeting.

12·30 P.M., moon's age at 10 P.M. equal 10·00 days. The remarkable feature which forms the subject of this communication was fully brought out. In the drawing illustrative of this feature the crater *Burckhardt* between *Geminus* and *Cleomedes* is introduced. Only a portion of the outline of *Geminus* at this period of the moon's age and under the angle of a midday illumination is discernible: the southern part of the crater appears to be cut off by a dusky patch extending from about the centre to a forked rillen emanating from the bright spot between *Geminus* and *Bernouilli* and branching towards *Burckhardt*. This dusky patch is not confined to *Geminus*, but extends to the westward of it nearly to the dark space northward of *Bernouilli*, while on the north of the nearly straight line bounding it a much lighter space is seen in which the outline of the northern part of the crater is very perceptible. The partial obliteration of the outline of the crater by a darker space, as if something had covered it, and the persistency of the remaining part in a lucid space apparently connected with a bright streak of the same character as those emanating from the bright crater *Proclus*, are phenomena in themselves sufficiently interesting to induce a closer observation of them, with the view of ascertaining if they are connected with any physical peculiarity of this part of the lunar surface.

Under the evening illumination these peculiar features disappear, the outline of *Geminus* is again complete, *Burckhardt* and *Bernouilli* exhibit striking contrasts of light and shadow, the rillen observable at midday are no longer to be seen, although the spots with which they appear to be connected are mostly recogniseable; the only features that appear to be common to the three aspects is the dark space northward of *Bernouilli*, a small rillen north of the crater is seen in the midday illumination, with this exception this feature is the most persistent.

The drawing representing the evening illumination is for the epoch April 18, 1859, between 12 and 16 hours G.M.T., 41 hours after full moon, the moon's age being 15·7 days. The evening illumination during the May lunation was not visible on account of clouds.

Desiderata.—The three views suggest various desiderata. It would appear from an inspection which I made of *Geminus* on the evening of Tuesday the 10th of May, about 24 hours after the epoch of the drawing representing the forenoon illumination, that the crater underwent a kind of *transition* from the elliptical form seen on May 9 to the partially obliterated outline seen on May 12. In my note of May 10, 9^h, I have the following remarks:

"*Geminus.*—The outline of this crater *distinct*, a part of the floor southwards appears to be *darker* than on last evening, but not of that character as observed at a later period last moon; the internal northern wall very bright; the darker part of the crater appears as if somewhat *terraced*, it is clearly

approaching the appearance it presented at a later period last moon."

It would be important to ascertain by *observation* the exact age of the moon, or still better the time elapsed from the initial illumination of the crater to the completion of the obliteration (between 8 and 10 days of the moon's age), also the phenomena of the *transition* from the perfect to the imperfect outline of the crater, with the various epochs of the visibility of the rillen; and in addition, as the sun declines on *Geminus*, the epochs of the re-completion of the outline and the commencement of the evening shadows.

Appendix.

As the peculiar feature alluded to in the foregoing paper appears to be connected with the system of luminous rays or streaks emanating from the bright crater *Proclus*, I have appended an ephemeris of *their* phenomena as well as some appertaining to *Geminus* and *Bernouilli* for the lunations in July and August next, extracted from a much more extensive one constructed for my private use.

July 5, 10·0 ☽'s age 5·31 Aug. 3, 10·0 ☽'s age 5·02	One of the luminous streaks from *Proclus* over the *Mare Crisium*, between the craters A and B (Beer and Madler's map) very faintly perceptible. *Geminus* and *Bernouilli* perfectly free from streaks or the appearance of rillen.
July 6, 10·0 ☽'s age 6·31 Aug. 4, 10·0 ☽'s age 6·02	Three luminous streaks of a somewhat forked character radiating from *Proclus* over the *Mare Crisium*. *Geminus* and *Bernouilli* perfectly free.
July 7, 10·0 ☽'s age 7·31 Aug. 5, 10·0 ☽'s age 7·02	The three streaks over *Mare Crisium* from *Proclus* more strongly marked. The streak skirting the north-west border of the *Palus Somnii* beginning to be apparent.
July 7, 10·0 ☽'s age 7·31 Aug. 5, 10·0 ☽'s age 7·02	About these ages of the moon an occasional gleaming of the rays from *Proclus* to the crater A (?), and towards *Geminus* may be detected also a bright luminosity about the crater A; this crater is considerably to the eastward of *Geminus* and north-east of *Cleomedes*. *Geminus*.—Perfectly free from luminous streaks, outline elliptical, *western wall* in shadow, with a somewhat dark spur extending a little *inwards*. *Eastern wall*, the south-

east portion not so dark as the north-eastern, the north-west wall brightest.*

Bernouilli perfectly free from luminous streaks.

July 8, 10·0 ☽'s age 8·31 Aug. 6, 10·0 ☽'s age 8·02 — The bright streak from A towards *Geminus*, which appears to be connected with the separation of the crater *Geminus* into a luminous and dusky portion, is discernible about these ages.

July 9, 10·0 ☽'s age 9·31 Aug. 7, 10·0 ☽'s age 9·02 — *Geminus* separated into two portions; a dusky and lucid outline of the northern portion distinct, a well-marked dark spur projecting outwards from the north-west part of the outline margin of the crater, edged with a somewhat dark fringe.

Bernouilli.—Floor very much darker than that of *Geminus*, north-eastern part of the margin of the crater bright.

July 9, 10·0 ☽'s age 9·31 Aug. 7, 10·0 ☽'s age 9·02 — *Rillen.*—Several rillen, or narrow light streaks, become discernible in the neighbourhood of *Geminus*, at or about these ages of the moon:—

First.—A light streak eastward of *Geminus* to the north-eastern border of the crater, near the dark spot on its eastern margin.

Second.—A forked rillen from the eastern border of *Geminus;* only a portion of this rillen is discernible at these ages, it is directed towards the spot whence emanate the first rillen noticed above. The branches are well seen a day later.

Third.—A forked rillen, emanating from the bright spot between *Geminus* and *Bernouilli*, one branch is directed towards *Burckhardt*, the other, which is fainter, towards the forked rillen (second).

* Between these epochs 7·31 and 8·31 of the moon's age one main feature apparently connected with the separation of the crater into two portions, comes into view; it is a rillen crossing the crater transversely and apparently connected with the rillen (second) of the list noticed under date July 9, 10·0. The continuity of the rillen appears to be interrupted by the dark fringe on the eastern margin of *Geminus*, and the rillen itself, within the crater, forms the southern border of the luminous portion of the crater when separated into two.

I observed this rillen very distinctly on the evening of June 8, 1859, between the hours of 9 and 10, moon's age about 7·60, the southern part of the crater was then very little, if at all, inferior in brightness to the northern.

Fourth.—A well-marked curved rillen from the spots northward of *Bernouilli.*
Fifth.—A straight rillen from *Bernouilli* northward.

N.B. This ephemeris will be available for any subsequent lunations by just altering the dates to suit the ages of the moon in September and following months, and by careful observation the times of the first appearance of the rillen and other features may be detected.

Description of Various Processes made use of for Finding out the Configuration of Optical Surfaces. By M. Leon Foucault.

(*Communicated by Mr. De La Rue.*)

"One of the principal advantages of the reflecting telescope consists in having, instead of an object-glass, a mirror which acts by means of a single surface; which surface being concave lends itself with remarkable facility to the application of the processes of examination which permit the appreciation of its figure even to the least details.

"I conjointly make use of three different processes. The first consists in placing very near to the centre of curvature a very small object, such as the point of a pin, in order to obtain an image of the same size as, and very near to the object itself; this image can then be observed by means of a microscope and compared directly with the object. We are thus enabled to judge with certainty by means of the amount of definition, of the effect which the mirror mounted as a telescope will give.

"If the image is imperfect, it is better then, with the view of ascertaining the faults of figure, to take a luminous point and observe the appearance of the pencil of light within and without the focus. It will be seen to decompose itself in partial images, the discussion of which gives at once some information respecting the configuration of the surface of the mirror.

"But the two processes I am about to point out are still preferable and give this information with greater precision.

"An object having parallel sides, such as the edge of a thin piece of steel, having one millimètre ($0^{in}.03937$) in thickness is placed in the neighbourhood of the centre of curvature, and in order that the object may project itself in outline it is enlightened from the side opposite to the mirror, and the image which is formed by it, being on a luminous ground, becomes very apparent, and presents in its aspect some peculiarities which depend upon the figure of the mirror. This image being observed with the naked eye at the distance of distinct vision is seen in each of its parts, but, by means of rays passing through the

pupil after having been reflected from a very small part of the surface of the mirror, so that in these circumstances the mirror only acts but partially, that is, by more or less restricted portions of its surface, which contribute severally to the formation of different parts of the image. Thus, if the curvature be not uniform, the different parts of the image will not form themselves in the same plane, and the angles subtended at the eye by the different parts will not be proportional to the corresponding part of the object; in short, the image will be deformed, contractions and dilatations will be seen, showing a decrease or an increase of the radius of curvature of the corresponding element of the mirror. The method becomes more correct and more conclusive, if instead of allowing the edge to have an indeterminate position in space, we fix on the eye-piece of a small telescope, magnifying but little, and provided with a narrow diaphragm comparable with the pupil.

"It is very rare that an image observed in such way will show itself free from any distortion, and such as a perfectly spherical mirror would give it, the edges instead of remaining rectilinear are bent most frequently in the shape of an hyperbola, and turn their concavity towards the interior of the field of the telescope; in presenting themselves under that shape they indicate that in every meridional section of the mirror the curvature goes on gradually diminishing from the centre towards the edge, which tends to correct to a certain degree the spherical aberration.

"Lastly, it remains now to describe a process which allows of the examination concave surfaces, so as to recognise directly, by an effect of relievo, the elevated and depressed parts which affect the figure.

"A thin piece of metal, pierced with a small hole having $\frac{1}{12}$th of a millimètre ($0^{\text{ln}}\cdot00328$, or about $\frac{1}{303}$th of an inch) and enlightened by an artificial light, is placed within the centre of curvature, through that little hole the diverging pencil of rays falls on the surface which is being examined and comes back converging to form an image which is situated a little beyond the centre.

"By placing the eye in the cone of rays which diverge anew, and in bringing it towards the image, we at last receive the whole of the pencil of rays which passes freely through the pupil, and at the same time the surface of the mirror will be seen entirely illuminated. Now, if an opaque and rectilinear edge is brought near the image of the hole and be made to infringe on it by degrees, the mirror will also lose by degrees its brilliancy, and when all the light is about disappearing, the whole of the irregularities of the surface of the mirror will be plainly seen.

"In fact, supposing the surface to be correctly spherical, the image is perfectly well defined, and when allowed to emerge a little from the opaque screen which is brought forward to in-

tercept it, the rays which are not stopped off come from the whole surface of the mirror which presents to the last a uniform brilliancy; if, on the contrary, this surface shows here and there some parts which are situated above or below the spherical level, the elements which do not present themselves under the proper incidence, these cause certain rays to deviate instead of converging with their fellow-rays (*congenères*) towards the different parts of the image and to be dispersed in all directions.

"It results from this that every point of the image, and in particular each of those which emerge from the opaque screen, receives rays which do not belong to it, and does not receive those which do belong to it.

"The missing rays will leave in their corresponding places on the surface of the mirror a deficiency of light, the accumulated rays will produce an increase of intensity in others, and from the contrast of these different degrees of brilliancy and on account of an effect *clair-obscur*, an enormously exaggerated manifestation of the inequalities which really exist on the surface submitted to this sort of test will result.

"As the observation can only be made by means of a single eye, it happens that under a moral influence independent of the the will of the observer, the valleys and hills seem to overlay each other; but with a little attention, what is a mere illusion will soon be distinguished from reality. In considering that the true rising parts are necessarily contained between two opposite declivities, one of which being turned towards the screen which hides the image appears dark, whilst the other presents an increase of brilliancy. In other words, the true figure of the mirror is analogous to that which would correspond to the apparent relievo interpreted in that hypothesis, in which an oblique light would come from that side opposite to the screen.

"This process offers, particularly, the advantage of being at the same time expeditious and direct; viz., a surface can be judged of in a few seconds, and, when it is defective, the faults of it are known immediately with great precision.

"Generally, all that has any influence on the direction of the rays, or anything which prevents them from converging simultaneously towards a point of common convergency, becomes visible and is found out. By means of this sort of observations, among several causes which affect the result, that one which exercises a predominant influence is easily distinguished from the rest. Thus the permanent irregularities of a surface will not be mistaken for the varying alteration due to the flexure of the mass of glass; and we can perceive the inequalities in the density of the ambient air which passes in whirling before the mirror, or which shows itself like stratified layers in the interior of the tube of the telescope. By taking the necessary precautions, all causes capable of producing an *accidental* disturbance are successively avoided, and the intrin-

sical faults or defects of the surface are the only ones which subsist.

"But, guided by the observation itself which causes them to be discovered, one succeeds, as I have demonstrated before, in correcting locally these defects; and lastly they can be reduced to such proportions as to exercise no sensible influence on the quality of the images.

"The mechanical processes by which the working of glass surfaces is usually effected seem to lose of their efficacy as soon as they are applied to pieces of unusual dimensions; the results which they then furnish are but an approximation, a sort of trial which is far from being satisfactory; but where mechanism becomes powerless, the hand of man can do something more: assisted by the resources which optics put at his disposal, and guided by a system of observations the power of which increases with that of the instrument which is to be constructed, the human hand is then enabled to proceed with the work, and to carry it out to the greatest degree of precision.

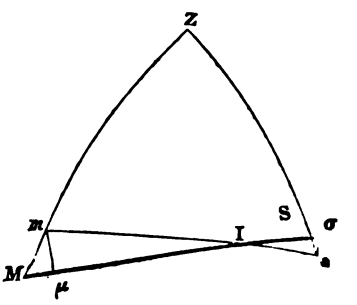
"If it be required to verify by optical means any one of the ellipsoidal surfaces which lead from the sphere to the paraboloid, the same process can be applied identically, on condition, that the object which is used as a mark is placed at one of the foci, and the image examined at the other focus. The information which is got thereby, and the corrections which must be deduced therefrom, are based on the same appearances; only they have reference especially to the sort of surface given by the position of the foci, and the inequalities which are found out show deviations which are reckoned in the same way as for the spherical figure, viz., from the level of the correct surface which it is desired to obtain."

On a New Method of Clearing Lunars.

By Lieut.-Col. R. Shortrede.

The following method of clearing a lunar distance gives a result always true to within a few hundredths of a second, and, requiring few logarithms, and these too from five to three places, is shorter and easier than any other I am acquainted with:—

m, M, and s, S, being the true and apparent places of the moon and star, and perpendiculars $m\mu$, $s\sigma$, being drawn to M S, the apparent distance $ms = MS - M\mu + S\sigma + Im - I\mu + Is - I\sigma$.



For the principal corrections $M\mu$, $S\sigma$, the proper expressions are

$$\tan M\mu = \tan Mm \cos M$$

and $$\tan S\sigma = \tan Ss \cos S$$

but as the triangles differ little from plane ones, we may without much error assume

$$M\mu = Mm \cos M = Mm - Mm \cdot 2 \sin^2 \tfrac{1}{2} M,$$

and $$S\sigma = Ss \cos S = Ss - Ss \cdot 2 \sin^2 \tfrac{1}{2} S.$$

Applying these corrections we have the curtate or reduced distance $\mu\sigma$.

With a given base $I\mu$, the excess $Im - I\mu$ is as μm^2, and as μm is not directly given, it is usual to take $Mm^2 - M\mu^2 = \mu m^2$; and then, by means of a small table, to take the difference of the corrections for Mm and for $M\mu$.

When the lunar distance is small and the corrections considerable, the tabular quantities vary rapidly; a small table of double entry is then inconvenient; and if the table be extended to include all cases, it becomes inconveniently large, and instead of finding the corrections for Im and Is separately, as they ought to be, it is usual to make one general correction for MS; the result so found may be m error by 4″ or 5″.

Legendre and Delambre, using a different method with the angle I, gave formulæ for finding the secondary correction; but as Delambre admits that his process of calculation is long, and that Legendre's process, though, as regards the logarithmic work, shorter than his own, requires a long and tedious preparation.

In the method here proposed we have to find Is, and then the secondary corrections.

To find Is we have

$$\text{Sin I} = \frac{\sin Ss \cdot \sin S}{\sin Is} = \frac{\sin Mm \cdot \sin M}{\sin Im}$$

$$\therefore \sin Is = \frac{\sin Ss}{\sin Mm} \cdot \frac{\sin S}{\sin M} \sin Im = \frac{\sin Ss}{\sin Mm} \cdot \frac{\sin ZM}{\sin Zs} \cdot \sin Im$$

$$= \frac{Ss}{Mm} \frac{\cos \text{Alt.} \leftmoon}{\cos \text{Alt.} \ast} \sin Im$$

Im is properly the difference between Is and the corrected distance sm. Instead of Im we may, for a first approximation, use the apparent distance SM, or the reduced distance $\sigma\mu$, and then subtracting the value thus found from $\sigma\mu$, we may, by repeating the process, find the true value of Is as near as we please. This is abundantly easy as logarithms, to 3 places, are sufficient, all of which, excepting that for the corrected value of $I\sigma$, are already given in the calculation of the principal corrections.

The triangle $I\mu m$ being right-angled, putting $\mu m = p$ for the difference d between the hypothenuse h and the base b, we have the expression $\sin d = 2 \sin \frac{p-d}{2} \cdot \sin \frac{p+d}{2} \cdot \cot b$, *which, as* p is always less than 1° and d less than 1′, may con-

veniently be reduced to $d'' = \frac{\sin 1''}{2} \cdot p''^2 \cdot \cot b$: for $p^2 = \mu m^2$ we have $(M m - M \mu)(M m + M \mu)$, whereof the first term is given in finding the principal correction, and the second term by adding together the parts already found, $\log \frac{\sin 1''}{2} = 4{\cdot}38454$ or simply $4{\cdot}385$; so that log cot $I \mu$ is the only trigonometrical term requiring to be found. It may be remarked that $I s$ being always a small arc, its cotangent will differ little from its cosecant or the arithmetical complement of log sin $I s$.

For the moon's correction logs to 5 places should be used, and for the star correction 3 or 4 places may be used according as the star may be high or low.

All these corrections are easily found true to a hundredth of a second. If extreme precision were required, a small correction $= -\frac{M m^3}{3 R^2} \cos M \sin^2 M$ should be applied on account of the difference between arc and tangent in the triangle $M m \mu$.

The true expression tan $M \mu$ = tan $M m$ cos M reduced to arc gives $M \mu + \frac{M \mu^3}{3 R^2} = M m^3 \cos M + \frac{M m^3}{3 R^2} \cos M \therefore M \mu = M m \cos M + \frac{M m^3}{3 R^2} \cos M - \frac{M \mu^3}{3 R^2}$; the principal term being subtractive, the spherical correction becomes $-\frac{M m^3}{3 R^2} \cos M + \frac{M m^3}{3 R^2} \cos^3 M = -\frac{M m^3}{3 R^2} \cos M \sin^2 M$.

The greatest possible value of this correction can never exceed eleven hundredths of a second.

Note on the recent Occultation of Saturn by the Moon, and on Experiments for ascertaining the Polarisation of the Moon's Light. By Professor Secchi.

(*Extract of a Letter to Admiral Manners, dated Rome, May* 9, 1859).

"We observed the occultation of *Saturn* on Sunday last with a very fine evening and steady image. We used for the purpose two different telescopes, the large refractor, power 600, and a smaller $3\frac{1}{2}$-inch Fraunhofer, but we could not observe any distortion at all attributable to a lunar atmosphere. At the immersion the dark limb of the moon was very visible and distinct, so that any variation in the form of the limb of the planet would be very sensible. I noticed only the irregular outline of the limb of the moon, the planet happening to disappear between two mountains. In consequence of this circumstance the last segment of the ring, as observed at the immersion, acquired the form of an oblong bead. At the reappearance the planet was so faint in comparison with the luminous edge of the moon that it could scarcely be perceived until it had already emerged a little. It looked like *Saturn*

seen by daytime half-an-hour before sunset. In this case also no sensible distortion was witnessed, although the terrestrial atmosphere was not so still as at the immersion.

"I have made some researches on the polarisation of the light reflected by the moon, and have been surprised to see the enormous difference between the mountains and the seas. It is very curious to see, for instance, the *Mare Crisium*, and other craters still smaller, polarised completely in the inner part, and only in a small degree in the outlines and ridges of the mountains. The maximum polarisation of the moon is at the seventh day of her age. The full moon is not at all polarised, *Jupiter*, *Saturn*, and *Mars*, are not more polarised than our clouds; in other words, the polarisation is insensible."

On the Deduction of the Latitude from Transits over the Prime Vertical. By Captain Tennant, B.E.

Let $\varphi_{(1)}$ denote the co-latitude cleared of the effects of instrumental error, $\varphi'_{(1)}$ the uncorrected value of the co-latitude, $\delta_{(1)}$ the polar distance of the star observed, θ its hour-angle at observation. Also let κ'' be the collimation-error, or the amount by which the small circle described by the mean of the wires differs from the great circle perpendicular to the axis of motion, κ being positive when the small circle is north of the other. Let η be the angle between the north point and the place where the instrument free from collimation would cut the horizon, and $\eta''_{(1)}$ its complement in seconds, which is the error in azimuth, and is negative when the direction of the axis of motion lies between the true meridian and the star's azimuth at transit, or when η is greater than 90°. Also let μ'' be the level-error, positive when the south end of the axis is too high, and negative if the north end be highest. The author then investigates the different corrections, and finally obtains the following expression:—

$$\varphi_{(1)} = \varphi'_{(1)} + \mu'' + \kappa'' \cos \varphi'_{(1)} \sec \delta_{(1)} + \eta''_{(1)} \sin \varphi'_{(1)} \tan \theta.$$

In a subsequent communication the author has given formulæ for reducing the transits to the mean of the wires. Both papers will be printed *in extenso* in the volume of *Memoirs*.

On a Method of Determining the Latitude by Transits. By Captain J. F. Tennant, B.E.

In this paper the author has investigated the formulæ for the effects of instrumental error in the case wherein the stars observed are not situate in the prime vertical. His object is, *by means of* the expressions thus obtained, to determine the

latitude when it is of small value; the method of prime vertical transits being, in his opinion, not so conveniently applicable in that case.

The President called the attention of the meeting to some communications which he had received from Mr. Slater, 136 Euston Road, relative to an object which had recently come under his observation, while examining a portion of the heavens with his large refracting telescope, of $14^{in}\cdot78$ aperture. In a letter, dated May 21, Mr. Slater states that in February last he was engaged in sweeping the region from *ξ Bootis* westward, with a power of 180, when in the position $11^h\ 48^m$ R.A., or $2^h\ 56^m$ west of the star just mentioned, the declination remaining unaltered, he saw something which looked very like a comet. In the field of view of the telescope, which subtended an angle of 30′, there were visible at the same time with the object three small stars of about the eighth magnitude, which formed almost a right angle. The declination of the object was found to exceed that of *ξ Boötis* by 8′. Mr. Slater watched the object during the evening, but could not detect any alteration in its position relatively to the three stars. After a month's unfavourable weather he again directed the telescope to the object, when he suspected that an alteration of its position had occurred. During the first week in May, before the interference of moonlight, he carefully examined the position of the object, comparing it with the earliest observation, and the alteration was now so manifest that the micrometer was not needed to detect it. In the month of February he could not see the object with a less aperture than 10 inches, and in May an aperture of $11\cdot5$ inches was necessary to render it visible.

Mr. Slater used several diaphragms for the purpose of ascertaining what size of telescope was required to see it. The best effect was with the whole aperture of $14^{in}\cdot78$.

In a second letter, dated May 23, Mr. Slater states that he again examined the object on the previous evening at $10^h\ 30^m$ G.M.T., when he succeeded in seeing it, but with difficulty. The nebulous appearance was not so decided as when first seen; it now looked more like two faint stars about 10″ apart. It appeared to have altered in position since he last saw it, on the evening of the 7th of May. According to the sketches which accompanied Mr. Slater's letters the object would seem to be travelling towards the north.

Captain Jacob has requested the Editor to announce that the observations of the minor planets printed in the *Monthly Notices* for May, though communicated by him, were chiefly made by the native *assistants in* the Madras Observatory.

Note on ρ Scorpii. By Capt. Noble.

"In sweeping over the constellation of *Scorpii* on the night of the 5th instant, I discovered that the star ρ (14) *Scorpii* (B.A.C. 5382) which is put down by Smyth in his *Celestial Cycle* as a double one, is in reality a triple, Smyth's B or ρ^2 being itself exquisitely double. At page 354 of the first edition of the *Celestial Cycle* it is described as 'a neat double star.' Pos. 338°·5, Distance 40″, Epoch 1831·50. The persistence of great atmospheric tremor, coupled with the small altitude of the star, has hitherto prevented me from obtaining micrometrical measurements, but at a guess I should say that the position of B*b* was somewhere about 40°·8, dist. 2″. I do not know whether this has ever been noticed before. At all events the discovery is original so far as I am concerned. The mean R.A. and Dec. of ρ *Scorpii* for this year are,—

R.A. $16^h\ 3^m\ 48^s{\cdot}34$

Decl. S. $19°\ 5'\ 26''{\cdot}65$

"*Forest Lodge, Maresfield, June* 9, 1859."

Observatory at Sydney.—First Annual Report of the Astronomer (W. Scott, Esq.) to the Observatory Board, December 22d, 1858.

The following are a few extracts from this Report: —

"In submitting my first Annual Report to the Observatory Board, I take the opportunity of making a brief statement of the principal circumstances connected with the establishment of the Observatory, from my own appointment to the present time. In the year 1855 the sum of 7000*l.* was set apart by the Government of New South Wales for the purpose of establishing an Astronomical Observatory; further sums were also voted for the salaries of an astronomer and one computer, and for the purchase of meteorological instruments to be used at various stations in connexion with the proposed observatory.

"As it was thought desirable that the Astronomer should himself superintend the building of the Observatory, the Astronomer Royal of England was requested to select a suitable person for that office.

"The Astronomer Royal having decided on offering me the appointment, I at once accepted it, and entered on my duties on April 16th, 1856.

"As a large portion of the interval between that date and my departure for Sydney as the necessary preparations for the voyage would allow, was spent in the Greenwich Observatory, for the purpose of completing my knowledge of practical astronomy and making myself familiar with the routine of an observatory, and the most recent improvements in the construction and use of astronomical instruments.

"In attaining this object I was kindly assisted by the Astronomer Royal, to whom I am much indebted for his advice, and for the facilities which he afforded me during my residence at Greenwich. I embarked on the 1st July, and arrived at Sydney on 31st October, 1856.

"My first duty was to fix on a site for the proposed Observatory. For purely astronomical purposes I should have preferred a position farther inland, and at a distance from any town; but as it appeared desirable, for various reasons, that the Observatory should be in the immediate neighbourhood of Sydney, I could find no spot more suitable than that recommended by the Governor-General, close to the Signal Station at Fort Phillip, on which the Observatory now stands. It possessed the advantages of a good foundation on the sandstone rock of which the hill is composed, an unimpeded view, and a considerable exemption from smoke and dust: moreover, from the extent of the Government reserve on which it stands, there will be no difficulty in protecting it from the encroachments of neighbouring buildings.

"The site having been decided on, the planning and erection of the Observatory were conducted by Mr. Dawson, the Colonial Architect, acting on my instructions so far only as the scientific requirements of the Observatory were concerned.

"The building was commenced in May 1857, and is still unfinished, but was so far advanced as to admit of meridian observations being made in June 1858.

"During the interval between my arrival in the colony and the commencement of astronomical observations, I took the opportunity of visiting those places which had been fixed upon for meteorological stations, arranging the instruments and instructing the observers in their duties.

"In concluding these remarks, I may state that, having had a sufficient sum placed at my disposal for the establishment of an Observatory on a good and substantial scale, I have felt it my duty not to be led by false economy to proportion the building to the present state of the instruments at my disposal, but to erect an Observatory sufficiently comprehensive and commodious to satisfy all the astronomical requirements of the colony for the next century.

"Thence it is very probable that, when all is completed, a very small portion, if any, of the sum voted will remain to be disposed of.

"*Buildings.*—The whole of the buildings are completed, with the exception of the dome and interior of the Equatoreal Tower, which will be finished in a few days.

"They comprise the astronomer's residence, which was completed so far as to be habitable, and occupied on the 11th of April last.

"The Astronomer's Office, containing also the library of the Observatory.

"The Computer's Room, at present unoccupied, and a space for unused instruments, &c.

"The Time-ball Tower, which was so far completed as to be made use of on 5th June last.

"The Transit-Room, completed some time in June.

"The Equatoreal Tower, which is still unfinished.

"The Transit-Room is adapted for the reception of two meridian instruments, by means of two sets of shutters in the walls and roof.

"The wall shutters, which are hung on ordinary hinges, and open inwards, have nothing remarkable in their construction.

"The roof shutters are peculiar, but very simple; they open inwards by their own weight, and are closed by means of a rope acting on one arm of a lever, the other arm of which is connected by a jointed rod with the shutter; the force required to close them is very trifling, and they are kept closed by belaying the rope to pins fixed to the wall.

"In order that they may not interfere with each other, one shutter is hinged to the east, and the other to the west side of the opening; the south shutter is capped by an iron ridge-piece, which projects over the north shutter when closed, so as to exclude the rain, which beats for the most part on the south side of the building. A great objection to this style of shutters is that they open inwards, which renders it very difficult to make them quite rain-proof.

"The shutters over the transit instrument are found to admit a considerable quantity of rain, if accompanied by high wind. It is absolutely necessary that this defect should be remedied before the meridian circle is mounted; and it is hoped that a simple expedient which is about to be adopted will have the desired effect. The wall shutters admit a great quantity of rain, a defect which is of much less importance than in the case of the roof shutters, and which may be to a great extent, though I fear not wholly, remedied.

"The Transit-Room is 24 feet long and 17 broad; the width of the meridian opening is 14 inches. I defer the description of the Equatoreal Building until I shall have an opportunity of testing, by experience, the working of the revolving dome and shutters.

"In addition to the works above mentioned, a small piece of ground has been enclosed on the north-east and south sides of the Observatory; that on the south side, 45 feet in depth, is set apart for the reception of the meteorological instruments."

Instruments.—Under this head are mentioned a transit instrument, of $3\frac{1}{4}$ inches aperture, by Troughton; a meridian circle (diameter not stated) in course of being repaired in England, under the superintendence of the Astronomer Royal; a mural circle of two feet in diameter which had been formerly used at the Paramatta Observatory; an achromatic telescope of

3¼ inches aperture, equatoreally mounted; and various smaller instruments. Several defects of the transit instrument are pointed out, which destroyed all confidence in the results furnished by it. The mural circle had not been set up, but appeared to be complete, with the exception of two microscopes sent to England with the meridian circle. The equatoreal was very defective, but might be so far repaired as to be used in the observation of occultations and eclipses.

"*Clocks.*—Those at present in use are an eight-day clock by Hardy, keeping sidereal time, and used in transit observations, and a thirty-day clock by Grimaldi, keeping mean time.

"There are considerable irregularities of rate in one or both of these clocks, but in consequence of the imperfections in the transit observations, I am unwilling to give any decided opinion at present as to their respective merits. There are two other clocks, of no great value, one by Breguet, formerly used as mean-time clock at Paramatta, and one by Barraud, of which the mercurial pendulum has been replaced by a cylinder of lead.

"There is also a box chronometer by Dent, at present used in dropping the time-ball, and a pocket chronometer by Dallas.

"*Time-Ball Apparatus.*—Complete and working satisfactorily, with one exception, that, owing to an inequality in the bore of the air-cylinder, the air escapes at one point too rapidly, and allows the ball to strike somewhat heavily at the end of its descent; this defect is not serious, and may be remedied, if thought desirable, without any great difficulty.

"*Work of the Observatory.*—In consequence of the defects of the transit instrument, already described, and the absence of the meridian circle, no observations can as yet be made of sufficient accuracy to be worthy of publication. I therefore confine myself to the observation of a few *Nautical Almanac* stars every night for the purpose of obtaining the correct time; also, when opportunity offers, of the moon and moon-culminating stars, for determining the longitude of the Observatory.

"The amount of work to be done being thus limited, I have not thought it desirable to appoint a computer until shortly before the arrival of the meridian-circle, after which there will be abundant occupation for a much larger staff than I can expect to have at my disposal.

"Previously to the acquisition of an efficient equatoreal instrument, which cannot be expected to take place within the next twelve months, my attention must necessarily be confined to meridian observations. These may be divided into two sets, namely, 1st, observations of clock-stars, that is, stars given in the *Nautical Almanac*, or others, whose positions have been accurately determined, for ascertaining the clock-errors and rates; observations of the right ascensions and declinations of stars whose positions are not yet determined with sufficient accuracy, consisting for the most part of stars south of the zenith. I propose at first to pay particular attention to stars

which are nearly vertical to Sydney, and other parts of the colony, with a view to their future employment in the accurate determination of latitude with the zenith micrometer.

"My plans, however, for the work of the Observatory are not definitely fixed, and may be somewhat modified by the replies to letters which I have addressed to the astronomers at the Cape of Good Hope and at Santiago, with whom it is desirable that I should act in concert. I have had no means as yet of determining accurately the latitude of the Observatory.

"The longitude, as deduced from twenty-one observations of the moon and moon-culminating stars, is $10^h\ 4^m\ 49^s{\cdot}0$. This determination is, of course, unsatisfactory, owing to the defects of the transit-instrument; the amount of probable error is 3·5 seconds, and the discrepancies between the different determinations are not above one-third as great as those which occur in Mr. Rümker's determinations of the longitude of Paramatta; this superior accuracy must be attributed to the increased accuracy of the lunar tables.

"The time-ball, which was dropped daily at mean noon, is now dropped at 1 P.M., as being a more convenient hour; the contemplated arrangements for dropping it by the action of the mean-time clock have not been carried out; at present, a corrected chronometer is carried to the room in which the machinery is placed, and the ball dropped by a touch of the finger.

"The time thus indicated may be considered correct within half a second, except in case of a succession of cloudy nights rendering observations impossible, in which case an accumulation of errors of clock-rates may lead to an error of more than a second in the time-signal."

Letter from Mr. G. P. Bond to Mr. Carrington.

Will you have the kindness to communicate to the Astronomical Society the following elements of the new Comet, which was discovered independently at three places in the United States, viz., Ann Arbor, April 23d; Washington and Cambridge, April 27th, by Messrs. Watson, Ferguson, and Tuttle, respectively.

Observatory of Harvard College,
Cambridge, Mass., May 5, 1859.

Elements, by T. H. Safford.

T	1859, May 29^d·0030	Washington M.S.T.	
	Log q	9·30403	
	ω	281 59·66	Equinox, April 27th
	Ω	357 41·77	
	i	94 52·78	

By A. Hall.

T 1859, May 29^{d}·0077 Washington M.S.T.

Log q	9·303310		
ω	281 58 10·7	or π =	75 9 46·1
Ω	357 7 56·8		
i	95 50 56·8	i =	84 9 3·2

Motion Retrograde.

The comet will probably be visible after its perihelion passage.

On the Stability of the Motion of Saturn's Rings; an Essay which obtained the Adams' Prize for the Year 1856, in the University of Cambridge. By J. Clerk Maxwell, M.A. late Fellow of Trinity College, Cambridge: Professor of Natural Philosophy in the Marischal College and University of Aberdeen. Cambridge: Macmillan and Co., 1859.

The following abstract of an important paper has been kindly drawn up by the Astronomer Royal for the use of the readers of the *Monthly Notices*:—

The remarkable essay of which we have given the title was published in the beginning of the present year. The subject of it is so interesting, the difficulty of treating it in its utmost generality so considerable, and the results at which the author arrives so curious, that we think a brief abstract of it will be acceptable to the readers of the *Monthly Notices*. We shall commence with a very imperfect reference to preceding investigations on the same subject.

The first to which we shall allude is Laplace's, in the *Mécanique Céleste*, livre III. chapitre vi. Laplace considers a ring of *Saturn* as a solid, the form of which is investigated as if it were fluid (a mode of treatment whose result, in respect of the form of equilibrium, is evidently good for a solid), and finds, that if the breadth and thickness of the ring are very small in comparison with its distance from *Saturn*, its section may be an ellipse; and it appears that the formula for the proportion of the axes of the ellipse admits of its being considerably flattened. But Laplace rather inclines to the supposition that there are several rings, each existing by its own proper theory. Then remarking on the appearances noticed by some observers which seem to indicate irregularities in the rings, he adds, "J'ajoute que ces inégalités sont nécessaires pour maintenir l'anneau en équilibre autour de *Saturne*," and gives an in-

vestigation which shows that, if the ring were rigid and uniform, the slightest disturbance would cause it to fall to the planet. Then he concludes thus, "Les divers anneaux qui entourent le globe de *Saturne* sont, par conséquent, des solides irreguliers d'une largeur inégale dans les différents points de leurs circonférences," &c. And in this state he leaves the theory.

Probably no competent mathematician has ever read this paper without remarking the want of ground for the last conclusion. Professor Maxwell has well given the following as the conclusion that ought to have been drawn: "If the rings were solid and uniform, their motion would be unstable, and they would be destroyed. But they are not destroyed, and their motion is stable; therefore they are either not uniform or not solid."

Omitting notices of some remarks by Plana and others which imply no important departure from Laplace's theory, we come to a paper by Mr. G. P. Bond, in the *Astronomical Journal*, Nos. 25 and 26. In No. 25 Mr. Bond gives arguments from observation tending to show that there are several rings. He then gives investigations, in a great measure similar to Laplace's, but leading more distinctly to a determination of the limits of density and dimensions of any ring: he finds that the number of rings must be considerable, and even makes calculations for eleven rings. He then alludes (without further remark or calculation) to Laplace's idea that a ring, if rigid, must be an irregular solid. And, finally, he gives his opinion that a fluid symmetrical ring is not necessarily unstable.

In No. 27 Professor Benjamin Peirce has commented upon Mr. Bond's paper. He "maintains, unconditionally, that there is no conceivable form of irregularity, and no combination of irregularities consistent with an actual ring, which would serve to retain it permanently about the primary if it were solid." He then refers to the theory of a fluid ring, and asserts that "there is nothing in the direct action of *Saturn* to prevent his ring from moving forward in its plane, in any direction and to any distance, until at last the rings would be brought into collision with the surface of the planet, and so be destroyed." Subsequently "the power which sustains the centre of gravity of *Saturn's* ring is not then to be sought in the planet itself, but in his satellites," and afterwards, "It follows, then, that no planet can have a ring, unless it is surrounded by a sufficient number of properly arranged satellites." This paper, it must be remarked, contains no symbolical investigations. Perhaps many mathematicians will think that a very complete mathematical treatment will be required to establish the two laws, that the attraction of *Saturn* will not support a fluid ring, and that the attraction of satellites will tend to support it.

In the same Journal, No. 86, is a paper of mathematical character on the same subject by Professor Pierce. But after some investigation, skilfully based on the method of potentials,

as to the rotation of a solid ring, he refers merely to general reasoning in order to establish that "the solid ring must be excluded from any physical theory which rests upon a firm basis." He adds, however, "the conditions of permanence would lose the unstable element, if the two successive normals to the level surface of the rings [*quære*, to the surface of equal potentials] at the centre of *Saturn* intersected each other between the centre of *Saturn* and the centre of gravity of the ring." It seems possible that this may refer to the case of a very large protuberance at one point of the ring; but the interpretation of the expression as it stands is somewhat obscure. The paper then proceeds to treat of a fluid ring, and terminates with, "The attractions of the opposite elements of the ring upon *Saturn* balance each other, and the ring has no tendency to move *Saturn*, neither has *Saturn* any tendency to give a motion of translation to the ring, or to check a translation which the ring may have received from other causes. Hence the ring will yield to the slightest foreign action, and may pass through successive normal forms of possible equilibrium without being restrained by the action of *Saturn*. The fluid ring cannot then be regarded as one of real permanence without the aid of foreign support, although the action of the primary is not positively destructive to this, as it is to the solid ring." The inference that "the fluid ring cannot be regarded as one of real permanence without the aid of foreign support," is, we presume, to be limited strictly by the preceding sentence. Thus it permits one elliptic form to be changed, by foreign action, to another elliptic form, each being permanent so long as it is not disturbed by further foreign action. The equations and the conclusions as to attraction are exactly the same as they would be for a ring of satellites not perturbing each other. In any case, we do not perceive the advantage of "foreign support." The author, however, has spoken so briefly on the subject that it is possible that we have mistaken his meaning. An expectation was held out that this paper would be continued, but, so far as we are aware, no continuation has yet appeared.

Professor Maxwell commences his treatise with an abstract of Laplace's principal conclusions. He then states that he has confined himself to those parts of the subject which bear upon the question of the permanence of a given form of motion, and adds,

"There is a very general and very important problem in dynamics, the solution of which would contain all the results of this essay and a great deal more. It is this, 'Having found a particular solution of the equations of motion of any material system, to determine whether a slight disturbance of the motion indicated by the solution would cause a small periodic variation, or a total derangement of the motion.' The question may be made to depend upon the conditions of a maximum or a minimum of a function of many variables, but the theory of the tests for

distinguishing maxima from minima by the calculus of variations becomes so intricate when applied to functions of several variables, that I think it doubtful whether the physical or the abstract problem will be first solved."

Part I. is headed "On the motion of a rigid body of any form about a sphere," attention being confined to the motion in the plane of reference. Equations being formed (founded on the method of potentials), they are then employed in the following propositions.

Prob. 1. "To find the conditions under which a uniform motion of the ring is possible."

Prob. 2. "To find the equations of the motion when slightly disturbed." Symbols for the disturbance of co-ordinates are attached to the symbols for co-ordinates found by the last problem, the new symbols being supposed liable to change with t, and a series of linear differential equations of the second order is formed.

Prob. 3. "To reduce the three simultaneous equations of motion to the form of a single equation."

Prob. 4. "To determine whether the motion of the ring is stable or unstable, by means of the relations of the co-efficients A, B, C, [introduced in prob. 3]."

Problems 3 and 4 give the key of the author's general method. In problem 3, the symbols of operation and of quantity are separated, n being then written for $\frac{d}{dt}$. (This amounts to the same as assuming that the variation of any element may be represented by $\epsilon^{nt+\alpha}$.) Equations are thus formed which exhibit no differential co-efficients. Then it is remarked that, supposing the value of n found, it will consist generally of the sum of a real and an imaginary term. If the imaginary term $= 0$, the variations of elements will increase, with the time, indefinitely and without change of sign. If both terms have values, the variations of elements will be periodical, with constantly increasing co-efficients. If the real term $= 0$, the variations of elements will be periodical, with constant co-efficients. The last-mentioned case is evidently the only one which is compatible with the idea of stability. The relations of the co-efficients A, B, C, are therefore to be ascertained, which will insure that the values of n be purely imaginary. We commend these propositions to the study of the reader, as an interesting example of a beautiful method, applied with great skill to the solution of the difficult problems which follow.

Prob. 5 determines certain quantities required in the application of the theory to a rigid ring of constant or variable section: and Prob. 6 then takes up the question, "To determine the conditions of stability of the motion in terms of the coefficients f, g, h, [found in the last problem], which indicate the distribution of mass in the ring." On applying this to specific suppositions, it is found (1) that a ring of uniform

section is unstable; (2) that a ring, thicker on one side than the other, and varying in section according to the simple law of sines, is unstable; (3) that a uniform ring, loaded with a heavy particle at a point of its circumference, may be stable, provided f be less than ·8279 and greater than ·8159 ($2f$ being the coefficient of cos θ in the expansion of the expression for the section of the ring at every point by Fourier's theorem). On calculating the case of $f = ·82$, it appears that the mass of the heavy particle is to the mass of the ring as 82 to 18. Such a law of structure evidently has no counterpart in *Saturn's* rings. It appears also that, in the case supposed, every variable co-ordinate may then be subject to two periodic variations, the period of one being 1·69 revolutions of the ring, and that of the other being 3·25 revolutions of the ring.

It appears then that, though a rigid ring is not impossible, yet it is impossible that *Saturn's* rings can be rigid rings.

Part II. treats "On the motion of a Ring, the parts of which are not rigidly connected."

The ring considered is small in section, and nearly circular and uniform, and revolving with nearly uniform velocity. The variations from circular form, uniform section, and uniform velocity, are expressed by an appropriate notation. The first step is "To express the position of an element of a variable ring at a given time in terms of the original position of the element in the ring." Fourier's theorem being applied to this, the problem becomes one of treating a number of separate variations, each of which is expressed by a circular function. Then the investigation is proposed "To find the magnitude and direction of the attraction between two elements of a disturbed ring." Having effected this, the author remarks, "We have found the expressions for the forces which act upon each member of a system of equal satellites, which originally formed a uniform ring, but are now affected with displacements depending on circular functions. If these displacements can be propagated round the ring in the form of waves with the velocity $\frac{m}{n}$, the quantities under the brackets will depend on t, and the complete expressions will be

$$\varrho = A \cos (ms + nt + \alpha)$$
$$\sigma = B \sin (ms + nt + \beta)$$
$$\zeta = C \cos (ms + nt + \gamma)$$

[ϱ, σ, ζ, being perturbations of three elements of the particle's place]. Let us find in what cases expressions such as these will be true, and what will be the result when they are not true."

The conditions are shown to depend upon the possible or imaginary character of roots of an equation, nearly as in the

first section; and the author soon arrives at this conclusion: "So that a ring of satellites can always be rendered stable by increasing the mass of the central body and the angular velocity of the ring." As an instance, supposing that there are 100 satellites, then the mass of the planet must not be less than 4352 times the entire mass of the ring.

Then the problem is considered "To determine the nature of the motion when the system of satellites is of small mass compared with the central body." After treating it generally, the author considers,—*First*, the motion of a single satellite with reference to its mean or undisturbed place; and shows that the motion is elliptic, the centre of the ellipse being at the mean place of the satellite, the major axis being in the tangential direction, the time of revolution being different from the time of revolution of the ring, and the direction of revolution being opposite to that of the ring. *Secondly*, the condition of the ring of satellites at a given instant: the author shows that the form of the ring at any instant is that of a string of beads, arranged as a circle affected by m regular waves of transversal displacement at equal intervals round the circle: besides these, there are waves of condensation and rarefaction, in which, according to the relation of coefficients, the points of greatest distance from the centre may be points of greatest or of least condensation. *Thirdly*, the condition of the ring of satellites through a duration of time: it is shown that the waves in question travel, with a motion which, in regard to the ring itself, is opposite to the ring's revolution.

There is then given a solution of the general problem, "From the state and motion of the satellites at one time, to find the state and motion at any future time."

The next problem is, "To consider the effect of an external disturbing force." A result soon reached is, "If the absolute angular velocity of the disturbing body is exactly or nearly equal to the absolute angular velocity of any of the free waves of the ring, that wave will increase till the ring be destroyed."

For the understanding of further results, it is almost necessary to refer to the theory of tides and waves. It is known in that theory that the action of the moon produces on the ocean waves whose periods and lengths are both dependent on the moon alone (called *forced* waves), and that these are or may be accompanied with *free* waves, whose periods depend on the moon but whose lengths do not depend on the moon or whose lengths depend on the moon but whose periods do not depend on the moon; and also with other free waves, entirely independent of the moon. And thus in the ring of satellites there will be forced waves and free waves.

The problem is then considered, "On the motion of a ring of satellites, when the conditions of stability are not fulfilled;" and the directions in which they tend to depart from their mean positions are found.

Satellites of unequal mass, it is considered, may be so grouped in our contemplation of their movements as to be treated nearly in the same manner as equal satellites.

The case of a fluid ring is then considered, and the conclusion arrived at is this:—"It appears, therefore, that a ring composed of a continuous liquid mass cannot revolve about a central body without being broken up, but that the parts of such a broken ring may, under certain conditions, form a permanent ring of satellites."

The author then treats "On the mutual perturbations of two rings." In this investigation arises a most curious complication of eight waves in each ring, with corresponding periods for the waves of the two rings, but with different relations of the coefficients in the two rings.

The author alludes shortly to "the effect of long-continued disturbances on a system of rings," and also to "the loss of energy due to internal friction in a broad fluid ring, the parts of which revolve about the planet, each with the velocity of a satellite at the same distance," which he concludes to be insensible.

The most important paragraph in the author's recapitulation is the following:—

"The final result, therefore, of the mechanical theory is, that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving round the planet with different velocities according to their respective distances. These particles may be arranged in series of narrow rings, or they may move through each other irregularly. In the first case, the destruction of the system will be very slow; in the second case, it will be more rapid; but there may be a tendency towards an arrangement in narrow rings, which may retard the process.

"We are not able to ascertain by observation the constitution of the two outer divisions of the system of rings; but the inner ring is certainly transparent, for the limb of *Saturn* has been observed through it. It is also certain that, though the space occupied by the ring is transparent, it is not through the material parts of it that *Saturn* was seen, for his limb was observed without distortion; which shows that there was no refraction, and therefore that the rays did not pass through a medium at all, but between the solid or liquid particles of which the ring is composed. Here, then, we have an optical argument in favour of the theory of independent particles as the material of the rings. The two outer rings may be of the same nature, but not so exceedingly rare that a ray of light can pass through their whole thickness without encountering one of the particles."

We have omitted allusion to a geometrical representation, by mechanism, of a disturbed ring of satellites, and also of a theorem by Professor W. Thomson, which, being expressed in

a different notation, cannot without difficulty be compared with the investigations of Professor Maxwell.

The abstract which we have given will, we think, fully justify the opinion that the theory of *Saturn's* rings is now placed on a footing totally different from any that it has occupied before, and that the essay which we have abstracted is one of the most remarkable contributions to mechanical astronomy that has appeared for many years.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *July* 8, 1859. No. 9.

Rev. R. MAIN, President, in the Chair.

Robert L. J. Ellery, Esq., Superintendent of the Astronomical Observatory, Williamstown, Victoria, was balloted for and duly elected a Fellow of the Society.

Places of Donati's Comet, from Observations made at the Armagh Observatory. By N. M'N. Edmondson, Assistant Astronomer.

(*Communicated by the Rev. Dr. Robinson.*)

1858..	G.M.T.	R.A. h m s	Par. in R.A. s	N.P.D. ° ′ ″	Par. in N.P.D. ″
Sept.	5·37691	10 49 3·35	+ Π × 0·0355	54 59 49·45	− Π × 0·8874
,,	6·35814	10 51 40·64	0·0391	54 49 37·99	0·8552
,,	10·34839	11 3 36·93	0·0415	54 11 1·43	0·8292
,,	12·34760	11 10 32·81	0·0406	53 54 32·08	0·8406
,,	19·38090	11 42 28·64	+ 0·0351	53 36 51·55	0·8918
,,	20·68441	11 50 1·05	− 0·0425	53 45 48·83	0·8172
,,	21·34392	11 54 6·29	+ 0·0421	53 52 45·09	0·8232
,,	27·36238	12 40 30·55	0·0373	56 47 16·93	0·8706
,,	30·31392	13 9 55·61	0·0444	60 6 14·32	0·7441
,,	·33635	13 10 8·20	0·0431	60 8 0·18	0·7837
Oct.	5·28465	14 8 8·48	0·0403	70 18 57·34	0·7235
,,	7·32000	14 33 38·66	0·0399	76 20 20·85	0·7805
,,	8·29050	14 45 48·82	0·0379	79 31 16·22	0·7688
,,	10·30338	15 10 34·08	0·0374	86 30 55·12	0·8025
,,	11·29005	15 22 20·76	0·0358	90 0 4·01	0·8127
,,	16·28141	16 15 48·88	+ [illegible]1226	106 16 46·94	0·8559

The Stars with which the Comet was compared.

			Assumed Places.			
Sept.	5	46 Leonis Minoris	Armagh Catalogue	5	comparisons	
	6	,,	,,	2	,,	Cloud
	10	L.L. 21400	La Lande's	5	,,	
	12	P. XI. 30	P.	8	,,	
	19	L.L. 22229	L.L.	5	,,	
	20	P. XI. 192	Armagh Catalogue	1	,,	Cloud
	21	,, 195	,,	5	,,	
	27	L.L. 23849	L.L.	5	,,	
	30	,, 24594	,,	6	,,	
	,,	,, 24665	,,	5	,,	
Oct.	5	Arcturus	Nautical Almanac	17	,,	
	7	L.L. 26914	L.L.	3	,,	Cloud
	8	,, 27004	,,	4	,,	,,
	10	P. XV. 4	P.	4	,,	,,
	11	,, 60	,,	5	,,	
	16	8 Ophiuchi ϕ	Armagh Catalogue	2	,,	

The observations were made with the finder of the east equatoreal; its object-glass by the late Peter Dollond is remarkably good; the aperture is $3^{in.}\cdot9$, the focal length $7^{feet}\cdot5$, and the magnifying power with the micrometer was 126.

Results of the Observations of Small Planets, made at the Royal Observatory, Greenwich, in the month of June, 1859.

(*Communicated by the Astronomer Royal.*)

Melpomene (18).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, June 29	12 11 27·6	18 41 37·17	98 43 39·48

Euterpe (27).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, June 17	11 41 28·0	17 24 13·94	112 45 4·59
29	10 42 11·9	17 12 6·85	112 39 20·56

Letter to the President from M. le Comte G. de Pontécoulant, accompanying a Memoir containing " Observations on the New Terms which Mr. Adams has proposed to introduce into the Expression of the Coefficient of the Secular Equation of the Moon."

À Monsieur le Président de la Société Royale Astronomique de Londres.

Monsieur le Président,—La bienveillante impartialité que vous avez montrée envers toutes les opinions, dans le résumé si lucide que vous avez présenté dans le dernier Compte Rendu des Travaux Mensuels de la Société Astronomique de Londres, de la controverse qui s'est élevée récemment relativement à la détermination du coefficient de *l'équation séculaire* de la lune, et le désir que vous avez exprimé de voir cette discussion, qui en se prolongeant pourrait devenir préjudiciable à l'honneur de la science, arriver à une prompte solution, m'engagent à devancer l'époque où j'aurai achevé le travail complet que je prépare sur ce sujet, pour exposer dès aujourd'hui les motifs des doutes que j'ai cru devoir soulever contre les nouvelles formules proposées par M. Adams ; je n'ai pas besoin de répéter, j' espère, que je professe pour le caractère et les utiles travaux de ce savant distingué la plus haute estime, et que l'intérêt de la science a pu seul me forcer à livrer ces observations critiques au grand jour de la publicité.

Agréez, Monsieur le Président,
l'assurance de ma haute considération,

LE COMTE G. DE PONTÉCOULANT,

Membre étranger de la Société Astronomique de Londres, etc.

Paris, 20 Juillet, 1859.

Observations sur les Nouveaux Termes que M. Adams a proposé d'introduire dans l'Expression du Coefficient de l'Equation Séculaire de la Lune. Par M. G. de Pontécoulant.

Lorsqu'il y a quelques années le mémoire sur la variation séculaire du moyen mouvement de la Lune, publié par M. Adams en 1853,* me tomba pour la première fois sous les yeux, je fus étrangement frappé de quelques paroles qui le terminent. L'auteur dit qu'ayant cherché à combler la lacune, qui en effet existe dans mon ouvrage (*Théorie Analytique du Système du Monde*, tome iv.), en déterminant par les formules directes que j'ai employées pour calculer toutes les autres inégalités de la Lune, *l'équation séculaire* de cet astre, il fut étonné de trouver une formule tout-à-fait différente de celle donnée par M. Plana

* *Philosophical Transactions* pour l'année 1853, p. 397.

et résultante des formules ordinaires adoptées depuis Clairaut par tous les géomètres qui s'étaient occupés de cette matière. Ce fut là ce qui lui donna la première idée des termes nouveaux qu'il a proposé d'introduire dans l'expression du coefficient de *l'équation séculaire*, et qui dérivent, comme on sait, de la considération de la variabilité de l'excentricité de l'orbe terrestre dans les équations différentielles du mouvement lunaire où jusque là on avait considéré cet élément comme constant. Ayant introduit, ajoute M. Adams, cette considération dans les formules des deux méthodes, il reconnut alors, avec satisfaction, un parfait accord entre leurs résultats. Or il n'y a qu'une petite difficulté dans cette assertion, c'est qu'elle énonce un fait mathématiquement *inadmissible*, ou du moins si l'accord a existé, ce ne peut être que l'effet, je ne dirai pas d'un heureux hazard, mais au contraire d'une rencontre fatale, puis qu'elle a été la première cause de la fausse voie, où, selon moi, M. Adams s'est engagé. En effet, M. Adams convient quelque part, je crois, et d'ailleurs je le démontrerai bientôt jusqu'à l'évidence, que la considération de la variabilité de l'orbe terrestre, n'exerce aucune influence sur la détermination de *l'inégalité séculaire*, lorsqu'on emploie pour l'obtenir les formules directes que j'ai adoptées dans ma théorie ; on peut donc dans ce cas en faire abstraction, d'où il suit que des formules, où l'on introduirait dans un cas l'idée de la variation de l'excentricité terrestre et où de l'autre on l'aurait tout à fait négligée, s'accorderaient dans leurs resultats finals, de telle sorte que l'immortelle découverte de Laplace, la cause qui produit *l'équation séculaire* de la Lune, aurait pu résulter d'un simple calcul numérique, sans qu'il fut besoin de l'intervention de la sagacité du géomètre!—cela ne peut être admis, le calcul peut quelquefois servir à suivre le génie dans son vol, mais il ne peut lui suppléer. On saurait admettre d'avantage que la même cause produise des effets différents, dans un cas des *inégalités* simplement *périodiques*, dans l'autre des *inégalités séculaires*. D'ailleurs une réflexion très simple et qui aurait frappé M. Adams lui-même, si les choses les plus simples n'étaient pas précisément celles qui échappent aux hommes les plus éclairés, lorsqu'ils sont vivement préoccupés d'une idée, confirme pleinement ce que je viens d'avancer. Qui est-ce après tout que le coefficient de l'équation séculaire ?—une certaine fonction des élements des orbites de l'astre troublé et de l'astre perturbateur, qui se déduit des formules différentielles du mouvement ; cette fonction est la même, selon M. Adams, par quelque méthode qu'on l'obtienne, dans le cas ou l'on considère comme variable l'excentricité de l'orbe terrestre ; à plus forte raison elle doit l'être dans le cas où l'on regarde cette excentricité comme constante, car les formules par lesquelles on traite la théorie de la Lune, ont toute la généralité possible, elles s'appliquent à toutes les hypothèses que l'on peut faire, et *elles doivent* les vérifier aussi bien dans le cas le plus simple

que dans le cas le plus composé. Il est inutile d'insister davantage sur ce point, et il est évident pour toute personne, qui voudra y réfléchir, sans qu'il soit besoin d'effectuer aucun calcul, que si le désaccord, qu'a signalé M. Adams entre les résultats des deux méthodes, existe réellement, il doit être attribué à une autre cause qu'à celle qu'il a indiquée et qui n'est point admissible pour cette explication. Je me propose, si l'espace me le permet, de rechercher ici cette cause qui ne dépend probablement que de la différence des notations employées dans les deux méthodes, car l'on conçoit que si les mêmes lettres représentent des quantités différentes, elles ne sauraient donner, quand on les convertit en nombres, des résultats identiques ; mais, nous devons auparavant examiner en elle-même la nature des nouveaux termes introduits par M. Adams dans *l'équation séculaire*, car ces termes, quand bien même ils ne rempliraient pas le but que s'était proposé l'auteur de rétablir l'accord entre des résultats obtenus par des méthodes différentes, auraient encore l'inconvénient d'altérer profondément l'expression analytique admise jusqu'à présent, du coefficient de cette équation.

Pour bien faire comprendre la question quelques explications sont nécessaires,—je prie qu'on veuille bien me les permettre en faveur de ceux de mes lecteurs qui ne seraient point parfaitement au courant de la question; le point de la discussion pourra être ensuite aisément saisi par tous.

La détermination du coefficient de *l'équation séculaire* de la Lune, quelque méthode que l'on suive pour l'obtenir, dépend du rapport qui existe entre son moyen mouvement dans l'orbite réelle, c'est à dire tel qu'il est donné par l'observation, à celui qui aurait lieu dans l'orbite elliptique, c'est à dire dans l'orbite que la Lune décrirait autour de la terre sans l'action perturbatrice du Soleil. En effet, si ce rapport était constant, le moyen mouvement lunaire serait le même dans tous les siècles, mais si ce rapport est sujet à quelques altérations, il en résultera une accélération ou un ralentissement dans la position moyenne de la Lune calculée dans la première hypothèse, et c'est en effet ce que les observations avaient indiqué aux géomètres longtems avant qu'ils ne fussent parvenus à en découvrir la cause. Désignons donc par n et $n_{,}$ les moyens mouvements de la Lune dans les deux hypothèses, et par a et $a_{,}$ les demi-grands axes qui leur correspondent et qui s'en déduiraient au moyen de la troisième loi de Kepler; pour avoir le rapport de n à $n_{,}$ on calcule d'abord celui de a à $a_{,}$ et celui de a à a, en désignant par α une quantité auxiliaire introduite pour faciliter les opérations ; l'analyse donne aisément ces deux rapports lorsqu'on se borne aux premiers termes des séries qui les expriment ; le calcul se complique en suite lorsqu'on veut porter l'approximation aussi loin que l'exige la précision des observations modernes, mais il n'a de difficulté réelle que son extrême longueur, et rien

n'empêche de le poursuivre jusqu'à tel ordre de quantités que l'on veut atteindre.

Cela posé, considérons d'abord les équations différentielles du mouvement lunaire où l'on prend comme on le fait dans la théorie des planètes le temps t pour la variable indépendante. Dans ce cas, en nommant R la fonction perturbatrice, on a, pour déterminer la longitude v dans l'orbite troublée, l'équation suivante (*Théorie Analytique*, tome iv. p. 133):—

$$\frac{dv}{dt} = \frac{h}{r^2} + \frac{1}{r^2}\int\left(\frac{dR}{dv}\right)dt \qquad (a)$$

Il suffira de développer l'equation précédente et de n'avoir égard qu'à la partie constante de ce développement pour en déduire immédiatement le rapport cherché. En effet en faisant pour simplifier abstraction de l'excentricité de l'orbe lunaire, on aura dans le cas du mouvement elliptique $\frac{dv}{dt} = \frac{h}{a_1^2}$, et en nommant donc $\frac{h}{a_1^2}\Pi$ la partie nonpériodique que l'action des forces perturbatrices ajoute à l'équation (a), et $h(1+p)$ ce que devient, dans ce cas l'arbitraire h, on aura:

$$\frac{dv}{dt} = \frac{h(1+\Pi)(1+p)}{a_1^2}$$

Mais d'après les règles du mouvement elliptique on a $h = \sqrt{a_1}$ et, en faisant abstraction des inégalités périodiques, on a $\frac{dv}{dt} = n$, on aura donc:

$$n = a_1^{-\frac{3}{2}}(1+p)(1+\Pi)$$

et comme la quantité $1+p$ peut être regardée comme déterminée par les autres formules du mouvement troublé, l'équation précédente fera connaître le rapport cherché de n à $a_1^{-\frac{3}{2}}$ où ce n à n_1. Toute la difficulté se réduit donc à former l'expression de la fonction Π.

Pour cela observons que la fonction $\int\left(\frac{dR}{dv}\right)dt$ peut se développer généralement en une suite de termes périodiques dépendants des moyens mouvements de la Lune et du Soleil; pour obtenir un terme constant dans l'expression de Π, il faudra combiner entre eux dans les deux suites de termes qui composent les fonctions $\frac{1}{r^2}$ et $\int\left(\frac{dR}{dv}\right)dt$, ceux qui ont les mêmes arguments. Prenons, pour fixer les idées, dans $\frac{dR}{dv}$ un terme de la forme:

$$\frac{dR}{dv} = A\,e'\sin(ft+l)$$

on aura en multipliant par dt et en intégrant:

$$-\int \frac{dR}{dv} dt = \frac{A}{f} e' \cos(ft + l)$$

Soit dans le développement de $\frac{1}{r^2}$ le terme $B\bar{e}' \cos(ft + l)$ qui a le même argument, on aura en vertu de ces deux termes dans $\frac{h}{\sqrt{a_1^2}} \Pi$ le suivant:

$$-\frac{1}{r^2}\int \frac{dR}{dv} dt = \frac{1}{2}\frac{AB}{f} e'^2$$

et, en opérant de même sur tous les termes semblables, on formera toutes les parties qui composent la fonction Π.

Maintenant observons qu'en intégrant l'expression précédente de $\frac{dR}{dv}$, nous n'avons point eu égard à la variation de l'élément e' qui représente l'excentricité de l'orbite terrestre, je suppose donc qu'on veuille tenir compte de cette variation; en reprenant sous ce point de vue l'opération et en intégrant par parties on aura:

$$\int \frac{dR}{dv} dt = -\frac{A}{f} e' \cos(ft + l) + \frac{A}{f}\int \frac{de'}{dt} \cos(ft + l)$$

Pour aller plus loin il faudra substituer pour $\frac{de'}{dt}$ sa valeur en fonction du temps t et effectuer ensuite les opérations indiquées, mais, soit qu'on regarde l'excentricité e' pour une époque quelconque, exprimée par une série de cosinus ou de sinus d'angles proportionnels au temps t, soit (ce qui suffit dans tous les cas ordinaires), qu'on développe sa valeur en une série de termes proportionnels aux puissances successives de cette variable, il est évident que dans les deux cas, il n'en saurait résulter dans $\frac{1}{r^2}\int \left(\frac{dR}{dv}\right) dt$ que des termes périodiques. Supposons en effet pour fixer les idées la valeur de e' après un temps quelconque t exprimée par la suite $g t + g' t + g'' t^2$, et pour plus de simplicité bornons nous à considèrer le premier terme, on aura $\frac{de'}{dt} = g'$, et la formule précédente deviendra:

$$\int \frac{dR}{dv} dt = -\frac{A}{f} e' \cos(ft + l) + \frac{Ag'}{f} \sin(ft + l)$$

$$= -\frac{A}{f} e' \cos\left(ft + l + \frac{g'}{e'}\right)$$

dans l'ordre d'approximation où nous nous arrêtons.

On voit donc que la considération de la variation de l'ex-

centricité de l'orbite terrestre ne fait qu'altérer d'une manière insensible la partie constante des angles des diverses inégalités lunaires multipliées par e', elle ne change en rien la forme des séries qui déterminent les co-ordonnées du mouvement troublé, et n'introduisent aucun nouveau terme constant dans le développement de l'équation (a); c'est-à-dire qu'elle n'altère en aucune manière le coefficient de *l'équation séculaire* déterminé en regardant cet élement comme constant, et qu'on peut par conséquent en faire abstraction dans cette recherche.

Voyons maintenant si cette conclusion a encore lieu lorsqu'on fait usage de la méthode exposée par Laplace dans la *Mécanique Céleste*, et suivie presqu'exclusivement par tous ceux qui, avant nous, avaient traité la question des perturbations lunaires; ce que nous avons dit précédemment nous sera très utile dans cet examen qui est le point capital de la discussion.

La méthode dont il s'agit consiste, comme on sait, à regarder la longitude de la lune dans son orbite troublée comme la variable indépendante; à déterminer par l'intégration des formules différentielles établies dans cette supposition les valeurs du rayon vecteur, de la latitude, et du temps t en fonction de cette longitude; et à revenir ensuite par la conversion des séries aux expressions ordinaires employées dans les tables astronomiques. Sans nous arrêter à comparer ici les avantages ou les inconvénients des deux méthodes, qui peuvent avoir leur utilité particulière selon les inegalités que l'on a à considérer, voyons quelle serait dans se cas la marche qu'on devrait suivre pour déterminer *l'équation séculaire* par des approximations successives, et avec tout le dégré de précision que l'importance de la question exige.

L'équation qui donne le temps t en fonction de la longitude vraie ν de la Lune, et qui correspond à l'équation (a) devient dans ce cas :

$$dt = \frac{d\nu}{h^2 u^2 \left[1 + \frac{2}{u^2}\int \left(\frac{dQ}{d\nu}\right)\left(\frac{d\nu}{u^2}\right)\right]^{\frac{1}{2}}} \qquad (b)$$

Q représente ici la fonction perturbatrice, $\frac{1}{u}$ est la projection du rayon vecteur lunaire sur le plan de l'écliptique, h une constante qui se réduit à $\sqrt{a_1}$ dans le cas de l'orbite elliptique, et lorsqu'on fait abstraction des termes dépendants des excentricités et des inclinaisons.

La détermination de l'équation séculaire au moyen de cette formule exige que l'on calcule, avec toute la précision désirable, la partie constante qu'elle peut renfermer lorsqu'on l'aura développée. Pour cela il faut d'abord y substituer à la place de Q sa valeur, qui se développe aisément en une suite de termes périodiques dépendants des longitudes vraies du soleil et de la

lune. En nommant v' et $\frac{1}{u'}$ par rapport au Soleil ce que nous avons nommé v et $\frac{1}{u}$ pour la Lune, on trouve ainsi une équation de cette forme :

$$\frac{dt}{dv} = \frac{1}{k u^2} + \frac{3 m'}{2 k^3 u^2} \int \frac{u'^3 dv}{u^4} \sin (2v - 2v') + \text{etc.}$$

m' représentant la masse du Soleil.

Il ne s'agit plus que de substituer à la place de $\frac{1}{u}$, de $\frac{1}{u'}$, et de v', et en général *à la place de toutes les variables relatives au mouvement du Soleil*, leurs valeurs exprimées en fonction de la longitude v de la Lune ; la quantité sous le signe intégral se développera alors en une suite de *sinus* ou de *cosinus* d'angles proportionnels à la longitude v, multipliés par des coefficients constants, et l'intégration sera facile à effectuer. Prenons pour fixer les idées dans la suite de termes que produit ainsi la fonction $\frac{3 m'}{2 k^3 u^2} \int \frac{u'^3 dv}{u^4} \sin (2v - 2v')$ le suivant :*

$$\frac{dt}{dv} = \frac{21}{4} m^2 \frac{a^2}{\sqrt{a_,}} \int e' dv \sin (2v - 2mv - c'mv).$$

Ce terme n'engendrera évidemment aucun terme non périodique, c'est à dire aucun terme du genre de ceux que nous considérons, lorsqu'on y regardera e' comme une constante, mais je dis qu'il en serait encore de même si l'on voulait avoir égard à la variation de cet élément. En effet, en effectuant l'intégration dans cette supposition et en négligeant, pour plus de simplicité, les termes d'un ordre supérieur à m^2, on aura :

$$\frac{dt}{dv} = -\frac{21}{8} m^2 \frac{a^2}{\sqrt{a_,}} \left\{ e' \cos (2v - 2mv - c'mv) - \int \frac{de'}{dv} \cos (2v - 2mv - c'mv)\, dv \right\}$$

Pour achever l'intégration il faudra, *d'après ce que nous avons dit précedemment*, et conformément aux principes élémentaires du calcul intégral, à la place de l'élément e' qui se rapporte à l'orbite du soleil, ainsi qu'on l'a fait par rapport à toutes les autres co-ordonnées de cet astre, substituer sa *valeur exprimée en fonction de la longitude v de la* Lune, qu'on a prise pour la variable indépendante ; supposons donc que l'excentricité de l'orbe terrestre au bout d'un temps quelconque t soit représentée par une série de cette forme $f + f'v + f''v^2 +$ etc.; en se

* Voir le Mémoire de M. Damoiseau, *Mémoires de l'Institut, Savants Etrangers*, tome 1^er^.

bornant au premier terme $\frac{de'}{dv} = f$, et en substituant cette valeur dans la formule précédente, on voit, sans qu'il soit besoin d'effectuer le calcul, qu'il en résultera simplement dans $\frac{dt}{dv}$ une inégalité périodique dépendante de l'angle $2v - 2mv - c'mv + \frac{g'}{e'}$ mais aucune partie constante, c'est-à-dire que dans ce cas le temps t sera représenté par la même série de sinus que dans le cas où l'on aurait regardé sous le signe f l'excentricité e' comme invariable, seulement les arguments des termes multipliés par e' seront augmentés de petites parties constantes, d'une grandeur insignificante et qui n'altéreront aucunement la forme de la série. Il est clair que le même résultat subsisterait encore si au lieu de supposer l'expression de e' développée par rapport aux puissances ascendantes de v, on voulait employer la valeur finie de cette quantité : il faudrait alors substituer dans les séries de *sinus* et de *cosinus* qui la représentent (*Théorie Analytique du Système du Monde*, tome iii.) à la place du temps t sa valeur en v ; on en déduirait par la différentiation la valeur de $\frac{de'}{dv}$, que l'on substituerait sous le signe intégral dans la valeur précédente de $\frac{dt}{dv}$; l'intégration s'effectuerait ensuite sans difficulté et l'on entirerait les mêmes conséquences que dans le premier cas, seulement cette fois les angles des *sinus* de la série qui représente le temps t, seraient augmentés de termes proportionnels à la longitude v ; mais comme ces termes se trouveraient multipliés par des coefficients très petits, la forme de la série n'en serait point sensiblement altérée. Ainsi donc de quelque manière que l'on procède, soit que l'on regarde le temps t ou la longitude v de la Lune dans les équations différentielles du mouvement, comme la variable indépendante, l'analogie des deux méthodes sera parfaitement conservée, les mêmes arguments se reproduiront dans les deux cas, et l'on pourra repasser de l'une à l'autre par la conversion des séries. Il résulte enfin de l'analyse précédente que lorsqu'on n'a en vue que de déterminer *l'équation séculaire* on peut, quelque méthode que l'on emploie pour lá calculer, regarder comme une quantité constante l'excentricité e' de l'orbe terrestre pendant tout le cours de cette recherche et n'avoir égard à sa variation que dans l'expression finale du moyen mouvement.

Ce n'est point ainsi que M. Adams a considéré cet objet dans son mémoire de 1853 ; il trouve que la considération de la variation de l'élément e' introduit des parties non périodiques dans le terme que nous avons considéré plus haut, et dans tous les termes semblables qui entrent dans la valeur de $\frac{dt}{dv}$, mais le procédé qu'il emploie pour les faire naître pourrait s'appeler une véritable *supercherie analytique*, et il suffira de la rappeler

ici pour en faire concevoir le vice. Ainsi, par exemple, ayant à effectuer l'intégrale indiquée dans l'expression précédente de $\frac{dt}{d\nu}$, M. Adams écrit ainsi le dernier terme :—

$$\int \frac{de'}{d\nu} \cos(2\nu - 2m\nu - c'm\nu) = \int \frac{de'}{ndt} \cdot \frac{ndt}{d\nu} \cos(2\nu - 2m\nu - c'm\nu)\, d\nu \qquad (c)$$

Il regarde ensuite $\frac{de'}{dt}$ comme constant, il substitue pour $\frac{ndt}{d\nu}$ sa valeur en série, et, combinant sous le signe $\int$ les termes qui ont le même argument, il en résulte une quantité non périodique de la forme $H \int e' \frac{de'}{ndt}\, n\, dt$ ou $\frac{1}{2} H e'^2$.* Ainsi l'expression de $\frac{ndt}{d\nu}$ renfermant le terme $-\frac{77}{8} m^2 e' \cos(2m - 2cm - c'm\nu)$, on aura en vertu de ce terme et en négligeant les parties simplement périodiques :†

$$\frac{dt}{d\nu} = -\frac{a^2}{\sqrt{a_1}} \left(\frac{21}{16} \times \frac{77}{16} = \frac{1617}{256}\right) m^4 e'^2$$

Il suit de là que le même terme, dont nous avons prouvé plus haut, par une analyse rigoureuse, qu'il ne pouvait sortir que des quantités périodiques, produirait selon M. Adams dans l'expression de $\frac{dt}{d\nu}$, et par conséquent dans celle de l'équation séculaire de la Lune, le terme $\frac{1617}{256} m^4 e'^2$. On conçoit aisément combien l'introduction de ce terme et des termes semblables provenant de la même cause, altérerait profondément la valeur réelle de cette inégalité, mais il suffit, je crois, d'avoir exposé clairement le procédé de M. Adams pour que chacun en reconnaisse le défaut et l'écart de tous les principes reçus. Ce n'est pas ici l'analyse qui a tort mais le mauvais usage qu'on en fait. En effet, dirai-je à M. Adams, de deux choses l'une, ou vous regardez $\frac{de'}{d\nu}$ comme une quantité constante et alors vous tombez dans le cas que nous avons considéré précedemment, et nous avons montré qu'il n'en résulte aucune inégalité non périodique dans $\frac{dt}{d\nu}$, ou vous regardez $\frac{de'}{d\nu}$ comme variable ce qui est le cas ou l'on considère l'expression finie de e'. Il faut se rappeler

* On ne saurait s'empêcher de remarquer ici une singulière distraction. L'auteur pour effectuer l'intégration suppose maintenant $d\nu = n\,dt$, mais cette supposition donnait $\frac{de'}{d\nu} = \frac{de'}{ndt}$, et toute son argumentation serait alors tombée d'elle-même dès le principe.

† Mémoire de M. Adams, p. 402.

que dans ce cas, les valeurs qui déterminent l'excentricité e' de l'orbe solaire, sont représentées par des séries de *sinus* et de *cosinus* de la forme $\Sigma B \sin (f t + l)$, $\Sigma B \cos (f t + l)$ (*Théorie Analytique*, t. 3), il faut donc préalablement y substituer à la place du temps t sa valeur en fonction de la longitude v, ainsi qu'on l'a fait dans l'expression du *rayon vecteur* $\frac{1}{u}$, et *de la longitude* v' du soleil; on en déduira ensuite $\frac{de'}{dv}$, et, en substituant cette valeur sous le f dans l'expression précédente, l'intégration s'effectuera sans difficulté; mais, sans faire le calcul, il est évident encore une fois qu'il n'en résultera que des inégalités périodiques et aucune inégalité indépendante de *sinus* et de *cosinus*.

Je crois qu'il est inutile d'insister davantage sur l'erreur que, selon moi, a commise M. Adams, et que lui-même sera le premier à la reconnaître dès qu'on la lui aura indiquée, mais une conséquence qui en découle naturellement et qui est écrite dans son propre mémoire, permettra pour ainsi-dire de la toucher au doigt. On trouve, page 401, l'équation suivante:

$$\frac{de'}{dv} = \frac{ndt}{dv} \cdot \frac{de'}{ndt} = \frac{de'}{ndt} \left\{ 1 - \frac{11}{4} m^2 \cos (2v - 2mv) - \frac{77}{8} m^2 e' \cos (2v - 2mv - c'mv + \text{etc.}) \right\}$$

Supposons l'excentricité e' après un temps quelconque t représentée par la formule $q + q' n t + q'' n^2 t^2 +$ &c., on aura, en substituant cette valeur dans l'expression précédente et intégrant, l'équation résultante:

$$e' = q + q' \left(v - \frac{11}{8} m^2 \sin (2v - 2mv) - \frac{77}{16} m^2 e' \sin (2v - 2mv - c'mv) + \text{etc.} \right)$$

C'est-à-dire, que l'excentricité de l'orbite terrestre, outre sa variation séculaire, serait soumise à toutes les inégalités du mouvement lunaire; c'est-à-dire, à des variations dont la période serait d'un mois, d'une année, etc., ce qui est contraire, quelque petitesse qu'on suppose au coefficient q', à tous les principes de la théorie. Si l'on veut donc introduire la longitude v de la Lune à la place du temps t dans l'expression de e', et qu'on ne croie pas suffisamment exact de substituer cette longitude à la place du moyen mouvement nt dans la formule $q + q' n t + q'' n^2 t^2$, il faudra de toute nécessité, introduire l'angle v dans l'expression finie de e', développer ensuite l'expression résultante par rapport aux puissances ascendantes de v, et l'on aura la véritable expression de e' en fonction de v correspondante à l'expression $q + q' n t + q'' n^2 t^2$ &c. relative au temps. De quelque manière

au reste qu'on l'ait déterminée il faudra substituer cette expression de e' en fonction de ν dans l'équation (c) *avant d'effectuer l'intégration; sans cette opération préalable* on n'obtiendrait jamais que des résultats défectueux.

Il résulte, je pense, sans *contestation possible désormais*, de la discussion précédente, que les formules employées jusqu'ici pour déterminer *l'équation séculaire* de la Lune, ont toute la correction nécessaire à cet important objet. M. Plana a développé le coefficient de cette équation avec une approximation qui ne saurait être dépassée ; j'ai depuis vérifié par mes propres calculs, et par des procédés différents des siens, mais en suivant toujours les formules de Laplace, l'exactitude des principaux resultats qu'il avait obtenus ; les petites différences qui peuvent encore exister entre nous, disparaîtraient sans doute par une nouvelle révision de ces longues opérations, et elles tombent d'ailleurs sur des quantités d'un ordre si élévé, qu'elles sont sans aucune influence sensible sur les résultats numériques. Sur ce point la question peut donc être regardée comme vidée, mais il en reste une autre importante à résoudre ; il s'agirait de fixer le coefficient de *l'équation séculaire* en employant les formules directes du mouvement lunaire, c'est-à-dire, les formules ou le temps est pris pour la variable indépendante, comme je l'ai fait pour toutes les autres inégalités périodiques ou à longues périodes ; car ce procédé seul conduit à la véritable *équation séculaire* qui affecte la longitude vraie de la Lune ; la méthode ordinaire donne *l'équation séculaire* qui affecte l'expression du temps et on en déduit par une inversion, tout-à-fait arbitraire, *l'équation séculaire* de la longitude vraie, la seule qu'il importe aux Astronomes de connaître. Or il paraîtrait, d'après l'observation qu'en a faite M. Adams, que la formule analytique ne coïncide pas dans les deux méthodes, bien que convertie en nombres elle doive donner sans doute des résultats peu différents. Je dirai, dans un autre note, la raison de cette discordance qui tient simplement à la différence des notations employées dans les deux méthodes, et nullement, comme on l'a vu, à la cause à laquelle M. Adams avait cru devoir l'attribuer. La détermination de *l'équation séculaire* par la formule directe, sera encore beaucoup plus laborieuse que par la méthode inverse, si l'on veut pousser l'approximation jusqu'au même ordre de quantités, et c'est probablement ce qui avait fait préférer cette dernière par tous les géomètres qui se sont occupés jusqu'ici de cette recherche ; mais l'importance de l'objet doit faire affronter toutes les difficultés, et si personne ne se présente pour entreprendre cet utile travail, je ne balançerai pas à m'encharger moi-même.

G. P.

Sur les Réfractions Anormales dans les Eclipses de Soleil et la Détermination des Longitudes par les Eclipses, by M. Liais.

(*Communicated by the Astronomer Royal.*)

On n'a pas l'usage de tenir compte de la réfraction de l'atmosphère dans le calcul des éclipses de soleil. Ces réfractions interviennent cependant contrairement à l'opinion générale, en faisant tomber l'ombre sur une localité autre que celle que l'on a calculée, car le cône d'ombre est réfracté dans l'atmosphère. Le sens de ces réfractions étant ordinairement oblique à la marche du cône d'ombre, ce cône est déplacé à la fois dans un sens perpendiculaire à son mouvement, et dans le sens de ce mouvement, ce qui change à la fois les localités où passe l'ombre et l'heure de l'éclipse.

Ce que nous venons de dire pour les limites de l'ombre a lieu également pour les limites de la pénombre. Or dans chaque localité, la position du soleil changeant entre le premier et le dernier contact, les réfractions changent également, de sorte que l'avance ou le retard de l'éclipse dû à la réfraction n'est pas le même pour les deux contacts extrêmes, et par suite la durée totale de l'éclipse est modifiée par les réfractions. Entre les deux contacts intérieurs, au contraire, le soleil change peu de position, vu le peu de durée de l'éclipse totale. La durée de l'obscurité totale n'est donc pas sensiblement modifiée par les réfractions régulières, sauf toutefois dans le cas où l'éclipse a lieu près de l'horizon.

Les diverses modifications des éclipses, dues à la réfraction régulière, et dont nous venons de parler, modifications qui ont également lieu dans les occultations d'étoiles et les passages de *Venus* et de *Mercure* sur le soleil, sont aisement calculables ; et il importe d'en tenir compte dans la détermination des longitudes par les éclipses. Mais, en outre, nous allons faire voir qu'il existe surtout pendant les éclipses totales une réfraction anormale qui doit en général racourcir la durée de l'obscurité totale, et à laquelle est due sans doute en grande partie la diminution que dans toutes les éclipses on a été obligé de faire subir au diamètre tabulaire de la lune pour rendre compte de la durée du phénomène. Dans certains cas ces réfractions anormales peuvent acquerir de très-grandes valeurs, et l'éclipse du 7 Septembre, 1858, parait en être un exemple.

M. Faye est le premier astronome qui ait appelé l'attention sur les réfractions anormales pendant les éclipses. Dans un mémoire, publié, je crois, dans les *Comptes Rendus* de 1851, il a fait remarquer que la variation de température qui a lieu pendant l'éclipse, variation qui existe entre les couches menées parallèlement aux limites même de l'ombre avec lesquelles elle se transporte, doit modifier la forme des couches d'air de même densité, lesquelles ne restent plus alors parallèles à la surface

du sol, comme on le suppose dans les réfractions astronomiques, mais qui se présentent alors très obliquement aux rayons solaires. A cette époque, M. Faye tenta d'expliquer à l'aide de ces réfractions anormales les nuages rouges que l'on avait remarqués sur les bords de la lune en 1842, et dans plusieurs éclipses antérieures, lesquels nuages n'eussent été alors que des phénomènes de mirage. Après les observations de l'éclipse de 1851, il renonça à cette explication de l'origine des flammes rouges.

Mais si les réfractions anormales sont insuffisantes pour expliquer l'origine des flammes rouges, que l'on s'accorde à regarder comme un phénomène réel, elles peuvent du moins faire concevoir comment en 1842, ces flammes ont paru à divers observateurs transportées sur le disque même de la lune. Elles peuvent en outre expliquer facilement les points lumineux vus sur la lune par M. Valz et par Ulloa pendant une éclipse, la tache lumineuse sur la lune vue en Septembre 1858 sur la côte du Pérou, les vapeurs jaunâtres apparues à St. Christophe sur ce même satellite dans la même éclipse, ainsi que le point rouge vu à Cherbourg par M. Fleury sur le bord de la lune projeté sur le soleil pendant l'éclipse presque totale du 15 Mars, 1858. Il est d'autant plus rationnel d'attribuer ces apparences aux réfractions que la théorie nous donne la certitude de l'existence de ces dernières cas un changement de température par suite un changement de densité dans l'air ne peut avoir lieu sans modifier la réfraction.

Examinons maintenant comment les réfractions anormales peuvent changer la durée d'une éclipse totale. Considérons, pour fixer les idées, le cas d'une éclipse au zénith, cas qui est précisement celui où il semblerait que les réfractions doivent le moins agir.

Dans le cône d'ombre la température est plus basse qu' à l'extérieur, l'air y est donc plus lourd. Sur la limite de ce cône d'ombre, les couches de même densité sont donc inclinées en s'abaissant du cône d'ombre vers l'extérieur, car dans l'air chaud, il faut descendre évidemment à un niveau plus bas pour trouver la même densité que du coté de l'air froid. Si donc on trace la section du cône d'ombre dans l'atmosphère et si on même le long de ses limites les normales aux couches d'égale densité, on voit immédiatement que la réfraction a pour effet de rapprocher successivement à mesure que l'on descend dans l'atmosphère les deux limites opposées du cône d'ombre, de telle sorte que l'intersection de cône par la surface terrestre est moindre que l'intersection qui aurait lieu sans ces réfractions, et qui est celle que l'on calcule. Cette intersection mettra donc à passer par le même lieu moins de temps que n'indique le calcul, d'où il suit que la durée de l'éclipse totale est racourcie. Ce que nous venons de dire pour une éclipse au zénith à pareillement lieu pour une éclipse à toutes les hauteurs sur l'horizon, comme il est facile de le voir.

Les réfractions anormales pendant les éclipses, réfractions qui rentront dans la classe des mirages, peuvent acquerir de grandes valeurs, surtout quand les mouvements de l'air sous l'influence du changement de température contournent les couches d'égale densité sur les bords de l'ombre. Si les phénomènes se passent avec une certaine régularité sur le contour du cône d'ombre, on pourra alors observer une diminution considérable de la longueur de l'éclipse. Il y a donc tout lieu de croire que les réfractions anormales se sont combinées avec les phénomènes particuliers d'irradiation signalés également par M. Faye pour diminuer la longueur de l'éclipse du 7 Septembre à Paranagua. Ce fait me parait d'autant plus probable que les distances lunaires et les hauteurs du soleil mesurées pendant le phénomène, présentent des anomalies que indiquent une grande intensité dans les réfractions anormales.

Ce que nous venons de dire des réfractions anormales pendant l'éclipse est de nature à jeter beaucoup de doute sur la valeur des déterminations de longitudes par les contacts intérieurs que l'on regarde comme les plus sûrs, et dont cependant les instants sont changés d'une manière inégale et inconnue. Cette considération m'a fait chercher une autre méthode pour ces déterminations. Cette méthode consiste à photographier le phénomène à diverses phases avant et après la totalité, et de déduire de la variation de l'angle de position des cornes la plus courte distance des centres dans le lieu d'observation. L'intersection de la ligne des points pour lesquels avait lieu la distance trouvée des centres et que le calcul fait connaître, avec le parallèle du lieu d'observation détermine la position du point d'observation sur la surface terrestre et par suite fait connaître sa longitude. Cette méthode est indépendante des réfractions anormales, si surtout ouest près la ligne centrale, auquel cas la partie anormale de la réfraction dont le sens est à-peu-près perpendiculaire à la ligne des cornes n'influe sensiblement que sur le temps et non sur le lieu de la phase, et de plus à moins qu'on n'en soit averti par des déformations de l'image, les angles de position ne sont pas modifiés d'une manière appréciable par ces réfractions ou que tous les rayons qui forment l'image, ont à très peu près suivi le même chemin dans l'atmosphère. En outre les observations peuvent être répétées un grand nombre de fois, si on a obtenu beaucoup de photographie. La méthode ne cesse d'être précise que si la ligne centrale de l'éclipse diffère peu d'un parallèle dans le voisinage du lieu d'observation, mais ce n'est pas le cas ordinaire. Comme précision, quand la ligne centrale diffère notablement d'une parallèle la méthode que je propose, donne une approximation supérieure à l'observation des contacts, quand bien même cette dernière ne serait pas entachée des causes d'erreur dont nous avons parlé. Ainsi à Paranagua par les photographies tirées pendant l'éclipse du 7 Septembre, il a été reconnue que la plus courte distance de centre à la station d'observation avait été sensiblement nulle; et il faudrait sup-

poser une erreur considérable et inadmissible dans les angles de position observés pour que l'on puisse admettre une distance des centres égale à 1″. L'erreur sur la plus courte distance des centres est donc au plus égale à celle que l'on a à craindre de la part des inégalités du contour de la lune en déduisant cette distance d'une observation de contact, et dans ce dernier cas en tenant compte des réfractions anormales, les erreurs peuvent être de six à huit fois plus grandes, comme l'a prouvé l'éclipse de Paranagua.

Lorsque le premier contact extérieur d'une éclipse à lieu, les couches d'air de même densité sont encore horizontales. Pareillement l'horizontaleté est sensiblement rétablie au dernier contact extérieur. Les instants de ces deux contacts peuvent donc être regardés comme modifiés seulement par la réfraction régulière dont l'effet peut-être calculé avec une assez grande sûreté à moins que l'éclipse ne soit très près de l'horizon.

Le gouvernement de sa majesté l'Empereur du Bresil m'ayant fait l'honneur de me confier la direction générale des travaux d'une commission scientifique composée d'officiers des divers corps de l'armée et de la marine, et chargée d'une nouvelle reconnaissance géographique et hydrographique de la côte de l'Amérique des Sud comprise entre l'Amazone et la Plata, j'ai eu à m'occuper des moyens de rapporter les longitudes de l'Amérique du Sud à celles des observatoires d'Europe, et c'est ce qui m'a conduit en outre des dispositions prises pour l'emploi de diverses autres méthodes, aux considérations qui précèdent relativement à l'emploi des éclipses, et en particulier de l'éclipse du 7 Septembre, 1858. Pendant la reconnaissance de la côte que nous allons faire avec un navire de guerre à vapeur, secondé quand il sera nécessaire par des navires auxiliaires des stations du Para, de Maragnan, Pernambuco, Bahia, et Montévidéo, la commission déterminera de nouveau, et avec soins, les positions géographiques des diverses stations où ont été observés les deux contacts extérieurs de l'éclipse du 7 Septembre de manière à employer l'ensemble de ces observations à corriger la parallaxe de la lune, la somme des deux diamètres et la différence de latitude des deux astres fournies par les tables au moment de la conjonction. A l'aide de ces corrections et de celles des tables de la lune fournies par les observations de Greenwich faites vers cette époque, on calculera de nouveau le trajet de la ligne centrale de l'éclipse en tenant compte de la réfraction du cône d'ombre, et l'intersection de cette ligne et du parallèle de la station centrale fournira la position de Paranagua rapportée a Greenwich, et par suite aux divers observatoires de l'Europe. La commission aura aussi un très bon point de départ pour la longitude de la nouvelle carte qu'elle est chargée d'exécuter.

Les réfractions anormales qui doivent avoir lieu pendant une éclipse de soleil, peuvent encore donner lieu à une remarque intéressante. Si ces réfractions présentent des inégalités dans la direction des diverses points du contour solaire, on voit

qu'il y aura certains points de ce croissant qui disparaîtront plus-tard que d'autres et on aura ainsi une explication du phénomène des peignes et de "Baily—Beads" dans le cas où le contour de la lune projeté sur le soleil offrait une parfaite régularité sur les bords, même avec de forts grossisements. Sans doute les montagnes lunaires concourent au phénomène de "Baily—Beads," mais il y a des cas où il est difficile de les expliquer par ces montagnes. C'est encore peut-être à la dispersion due à des réfractions anormales de la couronne qu'il faut attribuer les cercles irrisés vus en dehors de cet anneau par quelques observateurs, et en particulier à Paranagua, par MM. D'Azambuju et de Brito. Quant à la couronne, sa fixité et surtout la direction de son plan de polarisation s'opposent à toute explication de ce phénomène par les réfractions anormales.

Note on the Triplicity of ν Scorpii. By Captain Jacob.

(*Letter to the Editor.*)

Referring to p. 292 of the last *Monthly Notice*, the triplicity of *ν Scorpii* is rather an old story, having been discovered by me in 1847, and I think you will find some mention of it in one of the volumes of *Monthly Notices* about that time.*

Captain Noble may find measures of B, C, in vol. xvii. of our *Memoirs*, and later ones in the Madras vol. for 1848–52.

[Since Captain Jacob's discovery in 1847 the star has been measured by Mr. Dawes, Admiral Smyth, and Lord Wrottesley. The Rev. T. W. Webb, in a communication dated July 20, states that with his telescope, which has a clear aperture of 5·5 inches, the small star is very conspicuous.—Ed.]

Micrometrical Measures of the Triple Star ν Scorpii.
By Capt. Noble.

A B	Pos 337° 44′	Distance 40″·3
B *b*	Pos 39 40	Distance 2″ ?

Epoch 1859·48

Note on the Occultation of Saturn, May 8, 1859. By G. F. Pollock, Esq.

(*Extract of a Letter to the Assistant-Secretary.*)

"My own telescope being with the makers to effect an

* Vol. viii. p. 16.—Ed.

improvement in the stand, I had provided myself with a small telescope by Tulley with an aperture of $2\frac{1}{4}$ inches.

"As seen from my garden at Wimbledon, the dark portion of the moon was distinctly visible, both to the naked eye and in the telescope, and there were no clouds whatever in that part of the heavens, about a quarter of an hour before the occultation. Miss H——, a lady who was staying with me, and whose entire ignorance of celestial phenomena will presently be apparent to you, came out of the house and asked me what I was looking at, to which I replied that I was waiting to see the moon pass before *Saturn;* she then stayed to see it with the naked eye. Just before the occultation, and while I was looking through the telescope at the planet which was still some 3 or 4 diameters of the ring distant from the moon's edge, Miss H—— exclaimed, "You made a mistake in what you told me, for I see the star is passing in front of the moon instead of behind it." I immediately took my eye from the telescope, and saw apparently the dark limb of the moon with a bright star *entirely* on it, but touching the inner side of the circumference. On looking through the telescope again, I perceived that the planet had not yet actually arrived within a distance of the moon which I should estimate at more than twice the diameter of the ring. I did not take my eye from the telescope again, and observed nothing remarkable in the occultation, except that there appeared to be a slight retardation in the final disappearance of the planet, the last portion of the ring that was visible seeming to hang upon the moon's edge.

"What I have described is very different from the projection of a fixed star upon the moon's disk after it should have disappeared behind it. A phenomenon which even if not entirely dependent upon the diffraction of the telescope as suggested by Mr. Airy, could scarcely be seen by unassisted vision. The great irradiation of the planet as seen by the naked eye, would account for its appearing to overlap the moon's edge. The appearance, however, both to Miss H—— and to myself was, as I have already said, that of a bright star *entirely* on the moon's disk, though touching the edge on the inside, and this some time before actual contact on the exterior edge had taken place.

"The force of imagination may sometimes unconsciously assist us in seeing phenomena with which, though new to ourselves, we are familiar from description, what I may call the very innocent and unpremeditated piece of evidence afforded by Miss H——'s remark to me is quite free from any objection of this kind.

"*Wimbledon*, 7 *July*, 1859."

Mr. Alvan Clark's New Micrometer for measuring large Distances.

At the monthly meeting of the Society in June, Mr. Alvan Clark, of Boston, U.S., exhibited a micrometer invented by himself, which is capable of measuring with accuracy any distance up to about one degree. It is also furnished with a position-circle. Its character is essentially the same as that of the parallel-wire micrometer; but it has some peculiarities not, it is believed, previously introduced, and on which its wide range depends.

The most remarkable of these peculiarities consists in its being furnished with *two eye-pieces*, composed of small single lenses, mounted in separate frames, which slide in a groove, and can be separated to the required distance. A frame carrying two parallel spider-lines, each mounted separately with its own micrometer-screw, slides in a dove-tailed groove in front of the eye-pieces; and, by a free motion in this frame, each web can be brought opposite to its own eye-lens.

In using this micrometer, the first step is, to set the position-vernier to the approximate position of the objects to be measured. Then the eye-lenses are separated till each is opposite to its own object. The frame containing the webs and their micrometer-screws is then slidden into its place; and, the webs having been separated nearly to their proper distance by their free motion in the frame, they are placed precisely on the objects by their fine screws, the observer's eye being carried rapidly from one eye-lens to the other a few times, till he is satisfied of the bisection of each of the objects by its own web. The frame is then removed for reading off the measure by means of an achromatic microscope, on the stage of which it is placed. One of the webs is brought to the intersection of cross-wires in the eye-piece of the microscope; and by turning a screw (the revolutions of which are counted), the frame travels before the microscope, and the other web is brought to the intersection of the cross-wires. The *parts* of a revolution are read off by a vernier from a large divided circle attached to the screw.

The advantages arising from the peculiar construction of this micrometer are the following:—

1. Distances can be observed with great accuracy up to about *one degree*, and the angles of position also.

2. The webs, being in the same plane, are free from parallax, and are both equally distinct, however high the magnifying power may be.

3. The webs are also free from distortion and from colour.

4. A different magnifying power may be used on each of the objects; which may be advantageous in comparing a faint comet with a star.

MISCELLANEOUS INTELLIGENCE.

Professor Wolf of Zurich has recently published his eighth chapter or tract (*Mittheilung*) on the subject of the solar spots. He states that whereas in the year 1852 he had only materials at his command for tracing back the history of solar action through 12 epochs of maximum or minimum energy, the kind communications of friends and his own continued private research have so far added to his stock of examined records that he now counts up 32 cases of established epochs. In the present tract Professor Wolf gives the results of his examination of the MSS. observations of C. Tevel, a silversmith of Middelburg, in the years 1816, 17, 18, 19, 24 and 25, which were brought under his notice by Dr. Buys-Ballot of Utrecht. It appears that from the paucity of spots in the years '24 and '25 Tevel's interest in the subject was so far diminished that he discontinued his journal, and only resumed it again in the year 1828 with much less energy, from which time occur occasional notes as far as the year 1836. Another and smaller series of records here examined are those of an observer named De Hagen, brought under Prof. Wolf's notice by Herr Wagner of Pulkova. The observations were taken between the years 1739 and 1751. At the end of the tract Professor Wolf adds that in consequence of his perusal of Mr. Carrington's paper on the distribution in latitude of the solar spots during the years 1854 to 1858, he was led to specially arrange the results of Dr. Böhm of Vienna for the years 1833 to 1836, in order to see if any similar change of distribution occurred at or about the minimum of 1833. The result of his research is that such a change did occur at that time, and that the extremes of latitude for those years may be taken to have been as follows :—

	1833.	1834.	1835.	1836.
Latitude	$\pm 10^\circ$	$\pm 25^\circ$	$\pm 22^\circ{\cdot}6$	$\pm 16^\circ{\cdot}7$

apparently offering a very precisely corresponding instance to that first pointed out by Mr. Carrington. Dr. Böhm's observations, it may be added, were treated with a degree of skill and knowledge of the subject which entitles them to full confidence. [R.C.C.]

Professor Secchi, in a recent letter to one of the Secretaries, mentions that the observations of the solar disk made at Rome since the month of July 1858, so far as he has as yet examined, indicate that the limiting parallels for the faculæ correspond pretty closely to those for the dark spots. A clear climate, such as that of Rome, is necessary for this research, which it is to be hoped Prof. Secchi will be induced to pursue.

Lieut. Gillis's account of his eclipse-expedition to Olmos in Peru, last autumn, has been delayed in its transmission from America, and one copy only has as yet come to hand. The remainder are however in London, and will be distributed shortly. Lieut. Gilliss (U.S.N.) experienced much inconvenience of travel, suffering prior to and on the day of the eclipse from severe ague, and on his return contracting Panama fever, which remained about him for some time. The account will be read with interest, but as the principal contents and results of observations lie in the well-executed plate which accompanies the account and which cannot here be reproduced, an effective *précis* cannot well be given.

Lieut. Gillis is already anxious to learn the proposed plans of European observers on the occasion of the next total eclipse, visible in Spain and Algeria in July 1860.

[R.C.C.]

Professor Mitchell, of Cincinnati, has been appointed to the Directorship of the Dudley Observatory, Albany, U. S. Dr. Brünnow, late Director of the Ann-Arbor Observatory, Michigan, having accepted the position of Associate-Director of the Dudley Observatory, has removed to Albany. Dr. Brünnow purposes applying the Olcott Meridian Circle to a new determination of the stars in Bradley's Catalogue, and to the observation of some of the fainter asteroids. Respecting other operations intended to be prosecuted at the Dudley Observatory, in connexion with the observatory of Cincinnati, Professor Mitchell makes the following statement in a letter, dated May, 1859, which is published in No. 7 of Brünnow's *Astronomical Notices*:—"It is intended to mount two transits of equal size and power, the one in the Dudley, the other in the Cincinnati Observatory, to each of which the new declinometer and electro-magnetic machinery will be attached. With these two instruments in the hands of able observers, I hope to commence the execution of a long-cherished plan, involving the formation of a great Catalogue of Stars down to the tenth magnitude inclusive, by sweeping the heavens in zones. Should the new methods prove as rapid and delicate as experiment has thus far shown them to be, this work will advance with extraordinary rapidity under the combined action of the two observatories. It is intended to verify each by the other, as the same zones will be swept at the same time by the two instruments."

Dr. Brünnow having removed from Ann-Arbor to Albany, the *Astronomical Notices* edited by him will henceforward be published at the latter place. We find in No. 7 of that Journal the first recorded observations made with the Olcott Meridian Circle, consisting of places of the asteroids, *Flora*, *Europa*, *Fortuna*, and *Euterpe*, determined by August Sonntag, acting Assistant.

Professor Moesta, Director of the Observatory of Santiago, states in a letter to Dr. Peters of Altona, dated June 30, 1859, that his observations of the great comet of 1858 extend from the 30th of October, 1858, to the 1st of March, 1859. On the last-mentioned date the light of the comet was hardly the half, *cæteris paribus*, of the light which it exhibited on the day of its discovery.

Professor Moesta further states that in the course of last year the variable star η *Argus* has very strikingly diminished in brightness. In the two previous years the star continued to shine by a pretty constant light and to be as bright as β *Crucis*. It now appears smaller than γ *Crucis*.

Discovery of a New Planet.

On the evening of the 22nd September, Dr. R. Luther of Bilk discovered a new asteroid (the 57th). Its place was as under:—

	M.T. Bilk.	R.A. in Time.	Decl.
	h m	h m s	° ′
1859, Sept. 22	8 40	0 5 18	+ 8 13
Hourly motion		− 1ˢ·5	− 0″·4

It appears as a star of the 10th magnitude.

A Supplement to this Number will shortly be issued, containing some additional matter, including the Index to Vol. XIX. of the *Monthly Notices*.

ERRATUM.

Page 291, lines 10 and 18 from top, *for* ξ *Boötis read* ι *Boötis*.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIX. *Supplemental Notice.* No. 10.

On the Determination of Personal Equation by Observations of the Projected Image of the Sun.

(*Letter from C. Ragoonatha Charey to Captain W. S. Jacob.*)

"I beg to trouble you once more on the subject of the Observation of the Sun's Image. Prior to your departure to England, you were good enough to approve the observations of the above, taken by me with the transit instrument of the observatory, and you yourself continued to make a few observations, together with two or three observers simultaneously; and on my addressing you while here, on the 23rd April, 1858, (a copy of which is herein enclosed), you were pleased to apprise me that you find no deficiency in the mode of observation, that you will consult on the subject with some of the scientific friends in England, and will inform me the same.

"Having had in view for personal conference on the subject after your arrival in India, I was hitherto unmindful of intruding upon your valuable time; but, to our greatest misfortune, we came to know that you gave up your appointment as astronomer at Madras.

"Though I have a very poor knowledge in scientific matters, yet I presume to think from little experience that this mode of observing the sun is in no way adverse to accuracy, but it only adds a greater weight (than that we have hitherto obtained) to the result of observations, in proportion to the increase of the number of simultaneous observers; and, further, it affords ample opportunities to ascertain the personal equation of the observers easily.

"I happened to see a few months ago in the introduction of the first volume of the *Washington Observations*, p. 49, where Lieutenant Maury, the Director of the Observatory, states as follows, 'I have made trials for a personal equation, but could not succeed in establishing any uniformity as to the difference of eye and ear among the observers.'

"One of these experiments consisted in casting, by a proper adjustment of the eye-piece, a sharp image of the sun and wires at the time of transit, upon a screen placed at a little distance from the telescope, and to require each of the four officers to observe and record in silence the transit of the sun. The differ-

ence among them as to the mean of the seven wires was altogether insignificant.

"From the above I see that the transit of the sun's image is only taken for ascertaining the personal equation, and that also for an experimental purpose, without recommending the mode as one that can be employed daily for the general use of the observatory.

"I beg leave herewith to show the results of the personal equation deduced from the observations of the sun's image, with the transit instrument here, by yourself with the assistants of the observatory, together with the results derived from recent observations, made with the 5-feet achromatic telescope on the outside of the observatory, when the sun's altitude was about 50 degrees.

"For observing the sun's image on the outside of the observatory, I selected a spot where the action of the wind may not shake the instrument, and a shade for protecting the chronometer and the observers from the sun, and made the following contrivance to obtain the observations.

"I made a cylindrical cage of about ten inches in diameter, of bamboo slits and black cloth, covered its mouth with an oiled white paper, containing nine parallel lines, finely ruled in ink, at an interval of one inch in lieu of wires, introducing the same to the eye-end of the telescope, leaving a projection of about one foot and a half from it, and tieing up the other end on the body of the telescope, adjusted the eye-piece so as to cast a sharp image of the sun's limb on the paper, and thereby made a series of observations with a chronometer, for both the upper and lower limbs of the sun by myself, Sashoo Iyengar and Mootosamy Pillay.

"The following are the results of personal equation, deduced from the observations made by the transit instrument, between the months of February and May 1858:

The numerical coefficients are the number of observations of transit taken in each wire of the transit instrument.

$$\begin{cases} 50\,(J-R) = -20^{s}\cdot80 \\ 30\,(J-S) = -26\cdot50 \\ 20\,(J-M) = -\ 0\cdot50 \\ 50\,(R-S) = -22\cdot50 \\ 64\,(R-M) = +19\cdot30 \\ 31\,(S-M) = +14\cdot40 \end{cases}$$

"Solving the above (as treated in the introduction of the *Greenwich Observations*, by the Astronomer Royal) and assuming J to be zero, we get the final result as follows,—

$$\begin{aligned} J &= 0^{s}\cdot000 \quad \text{or } J-S = -0\cdot801 \\ S &= 0\cdot801 \quad\quad J-R = -0\cdot413 \quad \text{and } M-S = -0\cdot644 \\ R &= 0\cdot413 \quad\quad J-M = -0\cdot157 \quad\quad M-R = -0\cdot256 \\ M &= 0\cdot157 \end{aligned}$$

"And the results of personal equation deduced from the ob-

servations made with the 5-feet telescope, on the 5th April, 1859, between 3 and 4 P.M., are as follows:

The numerical coefficients are the number of observations of transit taken in each line of the cage.	30 (R−M)	= 8ˢ·50
	20 (S−R)	= 9·20
	19 (S−M)	= 11·60

"Solving the above and assuming M to be zero, we get,

$$M = 0^{s}\cdot000 \qquad S = 0\cdot662 \qquad R = 0\cdot251$$

or $M-S = -0\cdot662$, $M-R = -0\cdot251$

"The differences between the former and the latter values of M—S and M—R, are, I think, not of great consequence, considering the small number of observations.

"In conclusion, I beg to request that you will be pleased to take into consideration all the above my humble statements, and enlighten me as to the propriety and accuracy of this mode of observing the great luminary, and what you think proper on the subject.

"*Madras, April* 25, 1859."

Note in reference to the foregoing Communication.
By Capt. W. S. Jacob.

"This method has for some time been in use at Madras, having been proposed by the writer of the letter and approved of by me after due trial; it has several advantages, one of the chief of which is that it enables any number of observers to make the same identical observation simultaneously, and thus furnish a correct value of the personal equation of each. The method seems to have been tried for this special purpose at Washington, but found not to answer; we, however, found it to succeed perfectly, the personal equations appearing to have the nearly same value with those determined in other modes, but showing much greater uniformity in the separate measures. As the ordinary mode of taking the sun's transit it has further the advantage (a very great one in a tropical climate) of not exposing the observer's eyes to be scorched, or breaking the coloured glasses, a circumstance of continual recurrence even when the aperture has been reduced to an inconvenient degree, and this with no sacrifice, but rather an increase in point of accuracy.

"I am aware that the method does not possess any novelty, having been occasionally employed by myself more than 20 years ago, and probably by many others; being also the mode regularly employed by Mr. Carrington in fixing the places of the solar spots; but it is not, I believe, used in any other observatory besides that of Madras as the mode of observing the sun at noon, and therefore it may be useful to bring it to the notice of the directors of observatories."

Note from the Astronomer Royal to the Editor.

"In the last number of the *Monthly Notices* there is an account of the occultation of *Saturn* on May 8, as observed at Wimbledon by Mr. Pollock. I do not doubt the perfect accuracy of the description; but I do not clearly understand the tenor of the remarks which follow it.

"The phenomena of diffraction are produced as distinctly by the lens of the eye as by the object-glass of a telescope. They are, however, enormously increased by the want of accurate definition in the eye. Very few eyes (especially among those accustomed to look at near objects), perhaps none, see the moon without an enlargement of its diameter to the extent of several minutes of arc. This will easily be perceived upon examining, with the naked eye, the moon at the latest period at which the obscure disk is visible, or 'the young moon with the old moon in her arms,' when it will be seen that the apparent limb of the bright surface belongs to a much larger circle than that of the dark surface. I need not say that this entirely explains the appearance of projection.

"The irradiation of the planet has no part in producing the appearance of projection.

"The expression, 'projection of a fixed star upon the moon's disk after it should have disappeared behind it,' refers to some supposition with which I am not acquainted. The disappearance occurs, I conceive, exactly at the time at which it ought to occur, but the image of the moon's limb expands at that time so as to cover that of the planet; in the case of view with a telescope adjusted to focus, in consequence of diffraction; and in the case of view with the unarmed eye, in consequence principally of the imperfect definition by the eye.

"*Royal Observatory, Greenwich, Oct.* 4, 1859."

List of Star-Magnitudes not included in Baily's edition of Lalande's Histoire Céleste. By M. Hencke.

(*Extract of a Letter to the Astronomer Royal.*)

"Will you kindly permit me to annex a list of star-magnitudes, not given in Baily's Lalande's Catalogue: should these deficiencies already be supplied, and I thus come too late, still I hope that my list may not be quite superfluous, as the estimation of magnitude by a second person may be useful in doubtful instances. In any case, will you kindly accept the intention for the deed? Willingly would I annex, besides, a list of the many errors (nearly 3000) contained in that Catalogue, which I discovered in the course of making my Charts (segments of a 14-foot sphere), but with that also I shall, perhaps, come too late: moreover, this catalogue of faults might itself be still faulty, as I have been guided by eye-estimation only. At all events, I shall expect your kind command. A similar catalogue, *referring* to the Star-Charts of the Berlin Academy, stands [illegible]ally at your service.

List of Star-Magnitudes which remain for insertion in Baily's Edition of Lalande's Histoire Céleste.

The sign of μ affixed to the Magnitudes signifies a half.

No.	Mag.	No.	Mag.	No.	Mag.	No.	Mag.
16	8	3321	8	5364	9	7412	8
30	8 μ	3343	8	5531	7 μ	7415	8 μ
99	8	3347	8 μ	5601	7 μ	7511	8
102	9	3363	8 μ	5674	8	7523	8
248	8	3575	9	5680	8	7561	7 μ
373	7 μ	3605	8 μ	5711	8 μ	7586	9
482	8	3610	8 μ	5767	9	7608	9
507	8 μ	3638	9	5813	8 μ	7630	8
664	8	3655	6	5876	7	7631	8 μ
740	8 μ	3709	8	5893	6 μ	7683	7 μ
910	8	3720	8	5911	8	7729	8
936	9	3838	9	6013	8 μ	7773	8 μ
982	8	3920	8	6027	8 μ	7782	9
1021	8 μ	4014	7 μ	6169	8	7919	8 μ
1250	7 μ	4036	8	6183	9	7959	8
1303	8 μ	4103	7	6220	9	7962	8
1386	8 μ	4121	8	6269	8 μ	7996	8 μ
1429	9	4123	7 μ	6281	8 μ	8012	8
1725	8	4124	9	6335	7	8062	8
1758	9	4131	7 μ	6368	7	8226	7 μ
1759	7 μ	4259	7 μ	6502	8	8235	8
{1886 Pos. incorrect.}	7 μ	4269	8 μ	6531	6	8263	8 μ
		4238	7	6560	8	8301	7
1952	8	4391	7 μ	6576	8 μ	8306	8
1974	8	4465	8	6601	8 μ	8366	7 μ
1977	8 μ	4478	8	6644	8	8379	7 μ
2219	7	4508	7	6686	7	8411	6
2562	8 μ	4541	7	6754	9	8451	7
2586	8	4589	7	6866	8 μ	8505	7 μ
2691	8	4606	7 μ	6880	9	8615	7 μ
2717	8	4674	7 μ	6922	8	8657	7 μ
2778	8	4678	8	6925	8 μ	8699	7 μ
2814	7 μ	4700	7 μ	6935	7 μ	8733	8
2839	7 μ	4736	8	6989	6 μ	8735	7 μ
2863	8 μ	4748	8 μ	7082	9	8806	7 μ
2995	7 μ	4793	7 μ	7164	7	8814	8
3042	8	4795	9	7266	6	9090	8 μ
3064	8	4872	8	7309	8	9172	7 μ
3126	7 μ	4888	9	7310	9	9253	8
3167	8	4904	8 μ	7401	8	9366	8 μ
3182	8	5114	7 μ	7410	8	9367	8

No.	Mag.	No.	Mag.	No.	Mag.	No.	Mag.
9379	8	10611	8	13259	8	16734	8 μ
9388	8	10614	8	13338	8 μ	16776	8 μ
9392	8	10623	8	13378	8	16877	7 μ
9400	8 μ	10633	8 μ	13554	7 μ	16888	8 μ
9408	8	10759	8	13557	8 μ	17028	7 μ
9437	7	10866	8	13564	9	17172	8 μ
9493	7	10884	8	13695	6	17175	7
9584	8 μ	10891	8	13758	8 μ	17254	8
9623	8	10892	8	13812	7 μ	17720	8 μ
9626	8 μ	10921	8	13914	8 μ	17724	8
9638	8 μ	10933	4	13972	8 μ	17936	8 μ
9670	8	10975	7 μ	14031	8	17949	8
9687	8	11081	8 μ	14039	9	17969	8
9694	8 μ	11106	8 μ	14088	6	17998	8 μ
9715	9	11146	7	14099	7 μ	18215	8
9720	8 μ	11258	8	14112	8 μ	18230	8 μ
9721	8	11285	9	14295	8	18453	7 μ
9728	8	11288	7	14405	8	18681	8 μ
9754	7	11342	8	14412	9	18942	8 μ
9772	8 μ	11926	8	14415	8	19019	8
9797	8	12074	6	14421	8	19284	8
9805	7 μ	12151	8	14427	8	19486	8
9874	8	12194	8 μ	14431	7 μ	19566	8
9888	8 μ	12280	7 μ	14464	7	19649	8
9911	9	12319	8 μ	14563	8 μ	19655	8 μ
9977	8 μ	12389	8	14641	9	19698	8
10039	9	12399	8 μ	14830	8	19712	8
10099	8	12430	6 μ	14969	8 μ	19799	8 μ
10129	8 μ	12437	7 μ	15070	7 μ	19853	8
10189	8 μ	12453	6 μ	15134	8 μ	19869	7 μ
10205	8	12502	8 μ	15218	8	20363	8 μ
10264	6	12503	7 μ	15286	7	20400	6
10277	8	12504	7 μ	15330	8	var. 20592	9 μ
10306	9	12625	8 μ	15331	7	20624	8 μ
10309	6	12657	8	15447	8 μ	20764	7 μ
10318	8	12764	8	15571	8 μ	20771	7 μ
10327	8 μ	12775	9	15589	8 μ	20778	8 μ
10332	7 μ	12822	8 μ	15657	6 μ	20781	8 μ
10389	8	12960	8	15699	9	20819	8 μ
10399	7 μ	13031	8	16199	9	20886	8
10492	7 μ	13088	4	16356	7 μ	20968	8 μ
10497	8	13128	7 μ	16545	8 μ	21044	9
10566	8 μ	13205	7	16550	9	21056	9
10584	8	13207	8 μ	16571	7 μ	21146	8

No.	Mag.	No.	Mag.	No.	Mag.	No.	Mag.
21256	8 μ	23352	8	25786	8 μ	28069	8 μ
21266	7 μ	23356	8 μ	25822	9	28305	9
21313	8 μ	23372	9	26011	8 μ	28339	8 μ
21374	8 μ	23386	8 μ	26065	8	28375	8 μ
21425	8 μ	23614	8 μ	26141	8	28408	9 μ
21688	6 μ	23665	8 μ	26227	8	28416	9
21698	7 μ	23699	7 μ	26251	7	28436	7
21746	8 μ	23705	9	26425	8	28446	7 μ
21752	9	23737	8	26437	9	28460	6
21889	9	23762	8 μ	26481	8	28565	8
21846	7 μ	Pos. incorrect.		26468	7	28468	6
21957	8 μ	23926	7	26727	8	28608	6
22001	8 μ	23941	8 μ	26729	9	28683	9
22089	8 μ	23966	8 μ	26736	7 μ	28743	8
22117	8 μ	23969	8	26765	7 μ	28767	8
22154	8 μ	24046	8 μ	26788	8	28774	8
22255	9	24352	9	26992	7	28964	8
22259	8	24410	8 μ	27040	8	28976	7 μ
22282	8	24414	7 μ	27120	7 μ	29063	8
22303	8 μ	24426	8 μ	27138	6 μ	29171	8 μ
22357	7	24438	8 μ	27146	8	29203	8
22386	8	24473	9	27176	7 μ	29411	8
22456	9	24474	9	27182	5	29487	8
22479	8 μ	24505	7	27186	9	29495	7 μ
22482	8 μ	24549	9	27235	8 μ	29505	8
22496	7 μ	24638	8 μ	27267	8	29510	7
22510	9	24688	8	25286	8	29520	8
22520	6 μ	24698	9	27348	9	29683	6
22525	6 μ	24729	8	27373	8 μ	27775	8
22527	6	24747	7	27505	8	29798	7 μ
22536	7 μ	24789	8	27518	7 μ	29826	8
22575	7 μ	24932	8	27557	8 μ	29841	7 μ
22602	8	24981	8 μ	27588	8	29866	8
22619	8 μ	24997	8	27653	9	29898	8
22640	8	25136	6	27676	9	29935	6
22669	9	25234	8	27679	8	30034	8
22688	8 μ	25303	8	27831	8 μ	30054	9 μ
22801	7 μ	25347	7 μ	27873	8 μ	30089	8
22901	8	25354	8 μ	27906	9	30120	8 μ
22931	8 μ	25469	8	27927	6	30156	9
22942	7	25483	7	27955	8 μ	30411	9
23161	9	25492	9	27961	8	30457	7 μ
23166	9	25542	6	27984	8	30513	6 μ
23306	8	25560	8 μ	28027	7 μ	30522	9

No.	Mag.	No.	Mag.	No.	Mag.	No.	Mag.
30659	8	32801		34537	9	35403	8
30679	7 μ	32840	8 μ	34538	7	35409	9
30721	7	32891	8 μ	34570	7 μ	35442	8
30783	8 μ	32930	8	34578	8 μ	35443	8
30977	9	32946	6	34581	7	35456	8
31085	9	32971	7 μ	34628	6 μ	35520	8 μ
31131	9	33011	8 μ	34689	7 μ	35539	8 μ
31136	7	33065	8 μ	34693	7 μ	35566	7 μ
31138	8	33143	8 μ	34769	8	35614	8
31144	8 μ	33162	7 μ	34819	8 μ	35615	8
31155	9	33233	8 μ	34845	8 μ	35616	8
31162	7	33243	8	34846	8 μ	35648	8
31186	9	33254	7	34847	8 μ	35656	8
31191	3	33284	8	34860	7 μ	35666	6
31237	9	33307	8	34862	7	35696	8
31241	7	33312	8	34882	8 μ	35704	8 μ
31254	8 μ	33402	5 μ		5	35827	8
31277	9	33505	7	34905	8	35828	8
31292	7	33534	8	34915	6	35829	8
31397	9	33542	9	34933	7 μ	35870	6 μ
31491	8	33591	8 μ	34935	8	35878	8
31498	7 μ	33637	8 μ	33945	8	35910	8 μ
31503	8	33683	7 μ	34954	8 μ	35990	9 μ
		33704	7 μ	34955	7	36039	8 μ
31544	Neb.	33720	8	35028	6 μ	36071	8
31682	7 μ	33758	7	35067	7 μ	36072	8
31686	8	33759	7	35105	7 μ	36073	8
31838	8	33762	7	35109	8 μ	36095	8 μ
31856	9	33849	9	35119	7 μ	36141	8 μ
31881	8	33907	6	35120	7 μ	36142	6
31890	8	33968	9	35121	7 μ	36223	8 μ
31943	7 μ	34026	8	35154	7	36257	6
31948	7	34041	6 μ	35155	7 μ	36268	6
32024	7	34043	6 μ	35176	8	36331	8
32096	6	34155	9	35181	8	36339	9
32256	7 μ	34209	6 μ	35199	8 μ	36347	7
32322	8	34210	6 μ	35225	7	36355	8 μ
32449	7 μ	34290	8 μ	35230	7	36427	8 μ
32580	8	34346	9	35288	8	36473	6
32607	8	34417	7 μ	35306	8	36598	7 μ
32647	7	34432	7 μ	35307	8	36674	6
32699	8 μ	34448	8	35308	8	Position incor.	
32743	8 μ	34500	8	35318	8	36701	8 μ
32780	8 μ	34507	9	35334	4 μ	36710	7 μ

No.	Mag.	No.	Mag.	No.	Mag.	No.	Mag.
36712	7	38444	8	39830	8	41294	8 μ
36781	7	38515	7	39925	8	41410	8 μ
36833	7	38560	8	39927	8 μ	41430	8 μ
36855	8 μ	38666	8 μ	39944	8	41482	7
36858	8 μ	38667	9 μ	39960	8 μ	41553	8
36875	8 μ	38689	8 μ			41630	8
36911	8	38692	6	39988	8 μ	41631	8
37064	6 μ	38739	9	40091	8 μ	41654	8 μ
37090	8 μ	38740	8	40103	7 μ	41678	8
37116	8 μ	38755	9	40108	9	41682	9 μ
37126	7 μ	38768	9	40167	8	41771	9
37138	8 μ	38782	8	40295	8	41815	Neb.
37144	8	38794	8 μ	40373	7	41816	Neb.
37176	8	38814	7	40538	7	41827	8 μ
37285	8	38927	8	40574	7	41855	8 μ
37311	7 μ	38937	7 μ	40662	8 μ	41858	8 μ
37320	8	38944	7 μ	49675	6	41927	8
37382	8 μ	38969	8	40679	6	41928	Neb.
37430	8 μ	38975	9	40686	9	41931	9
37448	7	38986	7 μ	40773	7	41939	9
37490	9	39016	9	40779	8 μ	41944	8 μ
37640	8 μ	39062	9	40785	7	42025	8 μ
37656	8 μ	39081	9	40796	8 μ	42049	8 μ
37684	8	39110	7	40800	8	42067	8 μ
37717	7	39130	7 μ	40829	9	42233	7
37770	8	39242	8 μ	40858	9	42324	8
37808	8 μ	39257	3	40987	8	42354	7 μ
37874	8 μ	39281	7	40990	8	42375	7 μ
37883	8 μ	39495	8	41006	9	42392	7 μ
37899	8 μ	39496	7	41009	8 μ	42552	8
37907	7 μ	39497	7 μ	41010	9	42564	8
37916	8 μ	39506	7 μ	41019	9	42577	8
37918	8 μ	39538	0	41029	8 μ	42606	7
37962	8	39564	8 μ	41035	9	42656	8
38036	8 μ	39619	7 μ	41039	7 μ	42694	7 μ
38156	7	39620	9	41067	8	42696	8 μ
38172	7 μ	39643	8 μ	41081	8	42740	8
38187	9	39646	8 μ	41085	8	42767	8 μ
38241	8 μ	39651	9	41115	8	42821	7
38285	6	39663	5 μ	41158	8 μ	42910	8
38288	7 μ	39667	8	41199	7	42939	7
38403	7 μ	39669	8 μ	41201	7	42985	8 μ
38414	8 μ	39670	8 μ	41204	8 μ	43023	8
38424	8	39687	8 μ	41254	8	43151	6

No.	Mag.	No.	Mag.	No.	Mag.	No.	Mag.
43202	8 μ	44003	8	45232	8	46678	6
43208	8 μ	44018	9	45274	8	46686	8
43281	8	44035	7	45301	8	46792	8 μ
43283 Position incor.	8 μ	44062	9	45424	9	46906	6
		44111	7	45488	8	46956	8
43402	8 μ	44112	8 μ	45517	8 μ	47024	8 μ
43414	7 μ	44130	8 μ	45624	8	47069	8 μ
43448	7 μ	44154	8	45684	8 μ	47095	8 μ
43464	9	44172	7	45766	9	47100	7
43484	8	44177	6 μ	45771	7	47183	8
43504	7	44332	7 μ	45822	8	47186	8 μ
43505	7	44390	μ	45846	8	47207	7
43563	8 μ	44534	6 μ	45920	8 μ	47216	7
43623	8	44753	6 μ	45960	6 μ	47222	9
43634	8	44869	9	46020	9	47232	7 μ
43686	7 μ	44904	7	46115	8 μ	47251	7
43723	8	44911	8	46130	9	47259	7 μ
43751	6 μ	44917	7 μ	46143	8 μ	47287	7
43758	8 μ	44933	8	46153	8	47301	9
43759	8 μ	45016	8 μ	46373	8 μ	47316	7 μ
43819	8 μ	45092	5 μ	46496	8	47349	8 μ
43988	8	45162	7	46547	8	47368	8 μ

"The many thousand errors which still remain to be corrected, so far as relates to the star-magnitudes in this Catalogue, would occupy too much space. This, however, may be effected by means of my Charts.

"*Driesen*, 1859, *June* 30."

Note from Capt. W. S. Jacob to the Editor.

"In my paper on *Jupiter's* Mass, lately printed in vol. xxviii. of the Society's *Memoirs*, there occurs a passage which Mr. Airy has pointed out to me as requiring correction. It is at p. 110, l. 18:—'My reason for not adopting Mr. Airy's epoch was, *that it was not apparent whether the light-equation was included in it or not.*' The portion in *italics* would read more correctly as follows: 'that I was not certain of the extent to which the light-equation had been included in it.'

"I was aware that the equation had been in some way taken into account, Mr. Airy having distinctly stated the fact in the paper referred to (*Mem. R. A. Soc.*, vol. vi., p. 91); but the term '*light-equation*' has not an invariable meaning, being sometimes taken as the whole interval occupied in the passage

of light from the planet to the earth, as, for instance, in Herschel's paper on *Saturn* in his volume of *Cape Results;* while at others it means the excess of such interval above a definite minimum, as in Delambre's Tables of *Jupiter's* Satellites.

"Now, in attempting to transform Airy's formulæ so as to suit my purpose, from grades to degrees, and from Greenwich mean time to Madras sidereal time, and taking the light-equation in its *first* sense, I found a wide difference between the observed and computed places of the satellite; and though this was reduced by adopting Delambre's sense, the difference was still large enough to leave room for doubt. This might arise from some error in my computation; but as no such error could be readily detected, and as there was no one on whom I could thoroughly depend to repeat the computation, it seemed better to adopt an independent epoch, of the meaning of which I could be quite certain, than to lose time in trying to hunt up my error, or in a reference to Mr. Airy himself, the reductions having been made in Madras.

P. S. I regret to have to indicate the following errata, which have been discovered in my papers:—

Pages 37 and 38, column 12, heading, *for* $\frac{\text{Epoch}}{1855+}$, *read* $\frac{\text{Epoch}}{1850+}$.

39, column 12, heading, *for* $\frac{\text{Epoch}}{1858+}$, *read* $\frac{\text{Epoch}}{1850+}$.

40, columns 8 and 12, heading, *for* $\frac{\text{Epoch}}{1858+}$, *read* $\frac{\text{Epoch}}{1850+}$.

45, line 12 from foot, *for* $= -86{\cdot}59$, *read* $= +86{\cdot}59$.

45, line 10 from foot, *for* $+348{\cdot}7\,x$, *read* $+3487\,x$.

Raglan House, West Malvern, August 29, 1859.

Corrections of the Elements of the Moon's Orbit, deduced from the Lunar Observations made at the Royal Observatory of Greenwich from 1750 *to* 1851. *Being an extension of a preceding Memoir entitled "Corrections of the Elements of the Moon's Orbit, deduced from the Lunar Observations made at the Royal Observatory of Greenwich from* 1750 *to* 1830." By G. B. Airy, Esq., M.A., Astronomer Royal.

The author commences with a statement of the reasons which induced him to continue the reduction of the Greenwich Lunar Observations subsequent to 1830, on the same plan as that pursued in the great work on the reductions from 1750 to

1830; and he at the same time mentions the circumstances which led him to fix upon 1851 as the final year which would be distinguished by the system of reduction hitherto employed. In the year 1836 some changes were made in the system of observations, and very important changes in the system of daily reductions. The elements in these were varied from time to time as the observations seemed to indicate, and the tabular comparisons were made with Burckhardt's Tables, then employed in the computations of the *Nautical Almanac*. Thus there were inconsistencies of reduction which it was desirable to remove, and tables of inferior character had been employed, which it was desirable to replace by tabular calculations made on the same theoretical elements as those of the preceding reductions. An occasion for recommencing the work, and a reason for fixing a definite limit, were alone wanting.

"The occasion and the reason," says the author, "were supplied by Mr. Adams' discovery of the error in Burckhardt's Tables of the Moon's Parallax, and his corrections to that element published in the *Nautical Almanac* for 1856. Astronomers cannot be too grateful to Mr. Adams for that investigation; not only because it corrects a very important error in the construction and numerical values of the Parallax Tables, but also because it brings together the investigations founded on different theories and different observations, shows their precise agreement when correctly treated, and finally establishes the whole doctrine of Lunar Parallax on a firm basis. Nothing has been done which has aided so much the daily reductions of lunar observations."

The author was enabled to use the parallax as corrected by Mr. Adams from the beginning of 1852. This circumstance alone was sufficient to induce him to fix on the end of 1851 as the definitive limit of the proposed reductions. He was further prompted to adopt this resolution by the probability which existed of the speedy publication of Hansen's Lunar Tables, and their employment in the computations of the *Nautical Almanac*.

The proposal of the author having met with the approval of the Board of Visitors of the Observatory and of the Lords Commissioners of the Admiralty, the computations were commenced in the beginning of 1856, and were finally brought to a close in the summer of the present year.

Before investigating the results which form the object of his paper, the author gives a statement of the groups in years into which he has divided the period (1750–1851) over which his observations extend, with the range of longitude of the moon's node corresponding to each group. It is worthy of remark that the materials of the last of these groups (1847–1851) consist solely of observations with the altazimuth.*

* Of course there is a corresponding group (1843–51) comprising exclusively meridional observations.—Ed.

After giving the forms of the expressions for the comparison of the moon's observed places in longitude and in ecliptic polar distance with the corresponding tabular places, the author exhibits the equations depending on the new groups with the corresponding values of the unknown quantities, and combining the latter with the results of his former memoir (*Mem. Ast. Soc.* vol. xvii.) he obtains the definitive value of each unknown quantity. By means of these corrected values he then determines the most important corrections of elements and inequalities.

We now proceed to give the principal results of this important paper.

I.

Correction of Moon's Epoch of Longitude and Mean Motion.

The correction to Damoiseau's mean motion for one year is + 0″·596. Damoiseau's epoch is very nearly correct for 1806.

The difference between these numbers and those in the former memoir arises principally from the change made by Professor Hansen in the equations of long period.

II.

Correction of the Equation depending on the Elliptic form of the Earth.

This inequality has for its argument the mean place of the moon's node. The corrections being applied its entire value is found to be,—

$$+ 6'.41 \times \sin \text{longitude of moon's node}$$
$$- 1'.08 \times \cos \text{longitude of moon's node.}$$

III.

Correction of the principal coefficient of the Equation of the Centre.

The corrected value of the coefficient of the sine of mean anomaly when the true longitude is expressed in a function of the mean longitude, as in Damoiseau's form, is:

$$22639''.04$$

again the correction applicable to the moon's longitude,

$$\{- 0''.69 \times \cos \text{long. node} + 0''.59 \times \sin \text{long. node}\} \sin \text{mean anomaly.}$$

IV.

Correction of Epoch and Motion of Mean Anomaly.

Damoiseau's epoch of anomaly is nearly correct for 1787 and the motion of anomaly in 100 years must be diminished by 35″·5.

The corrections to the moon's longitude are,—

$$\{+ 0''.19 \cos \text{long. of node} + 1''.60 \sin \text{long. of node}\} \cos \text{mean anomaly.}$$

V.

Small Terms depending on Combinations of the mean anomaly and the longitude of the node.

These terms may be expressed in the following form :—

+0″·46 sin (mean anom. + long. node) −0″·20 cos (mean anom. + long. node)
−1″·15 sin (mean anom. − long. node) +0″·39 cos (mean anom. − long. node)

VI.

Correction of the Coefficient of Parallactic Equation.

The observations with the altazimuth play an important part in the determination of the correction applicable to this coefficient. The author concludes that the value of increase cannot be far from +2″·6, whence the real value of the coefficient = 124″·7.

VII.

Correction of the Coefficient of Variation.

The correction derived from the meridional observations = +0″·61 ; that from the altazimuth observations = +0″·30. Adopting +0″·50, the corrected coefficient will be +2370″·8.

VIII.

Correction of the Coefficient of Annual Equation.

The corrected value of the coefficient is found to be 670″·04.

IX.

Correction of the Coefficient of Evection.

The corrected value of this coefficient is =4586″·81.

The author next investigates the corrections applicable to the elements of Ecliptic North Polar Distance.

X.

Correction of the Constant Term of Parallax.

The corrected value of this constant is found to be 57′ 3″·89.

"The constant thus found," says the author, "is somewhat greater than Plana's, which is 57′ 3″·16, or Mr. Adams', which is 57′ 2″·3 ; and a little greater than that found from the Cambridge Observations (*Mem. Ast. Soc.* vol. xvii. page 51), which is 57′ 3″·46. It is worthy of remark that in each instance the constant derived from observation is greater than that derived from theory."

By confining his investigation to the observations subsequent to 1811, which are much more certain than those preceding that year, the author finds the corrected value of the constant to be 57′ 3″·55. This brings the various numbers into better harmony.

Under this head the author has inserted the following foot-note, to which he calls especial attention:—

"I regret to say that in the former *Memoir*, vol. xvii., pages 51 and 52, I have committed a most serious error in my statement of the constant of parallax employed in the former *Reductions*. I have given it as 57′ 3″·16, whereas it is stated explicitly in the *Reductions* (Introduction, pages xxi and xxii) and has been verified by reference to the manuscripts that the constant employed was 57′ 1″·8. I cannot account for this confusion in a research which so much engrossed my attention, and in which I was so much impressed with the difficulties of reconciling results."

The rectification of the error here referred to has enabled the author almost entirely to remove the discordances which presented themselves in the former paper between the definitive value of the constant of parallax which he arrived at by a discussion of the observations down to 1830, and the values of the same constant indicated respectively by observations made at Cambridge and Greenwich subsequently to that date.

XI.

Correction of the Inclination of the Moon's Orbit; Terms of Long Period in the Inclination of the Orbit.

The value of the inclination is found to be

$$18535''\cdot55$$

The inequality in inclination is represented by the following terms:—

$$-\,0''\cdot73 \times \text{cos long. of node} - 1''\cdot82 \times \text{sine longitude of node.}$$

XII.

Correction of the Argument of Latitude and of the Motion of the Argument: Terms of Long Period in the Argument.

The secular motion of argument is to be diminished by 68″·6. Damoiseau's epoch is correct for 1781 nearly. The inequalities in argument of latitude are found to be

$$+\,23''\cdot46 \times \text{cosine longitude of node} - 5''\cdot09 \times \text{sine longitude of node.}$$

XIII.

Small Terms in the Moon's Latitude produced by the Combination of the Small Terms of Long Period, in the Inclination and in the Argument of Latitude.

Omitting two small terms of insensible value, the author obtains for the terms of ecliptic north polar distance, depending upon the argument "longitude from the first point of *Aries*,"

$$+\,1''\cdot96 \cos u - 8''\cdot59 \sin u,$$

u denoting the moon's true longitude.

XIV.

Correction of the Coefficient of Evection in North Polar Distance.

The tabular coefficient 527″·5 may be considered as sensibly correct.

Suggestions as to the Structure of the Tails of Comets. By the Rev. T. W. Webb.

Though there seems to be no question that the dark space so frequently included in the tails of great comets is the result of a hollow structure, an attentive consideration of the appearances exhibited by the comet of Donati has led me to think it probable that some other cause may concur in its production. The ordinary laws of perspective certainly seem inadequate to its explanation, except in cases in which the darkness bears a large proportion to the brighter streams on each side of it: for unless the difference is but small between the radius of the hollow interior and that of the whole tail, the sine will not exceed the versed sine in a sufficient ratio to account for so great an increase of luminosity as is frequently witnessed. On the contrary, at one period in the course of Donati's comet, at the end of September and during the first few days of October, the central darkness in the train, though very intense, occupied a comparatively small part of its whole breadth. My own estimate on September 30 gave it but one-eighth of the entire width of the tail, in which case a simple calculation will prove that the sine would exceed the versed sine only by something less than one-eighth part, and consequently the resulting difference in brightness would by no means accord with observation. The supposition of a shadow projected from the nucleus might seem at first to assist us with a supplementary amount of darkness; but it will be found unavailing when we have compared the case of the comet of 1811, in which a transparent space surrounded the nucleus alike *on every side*. Hence it may be thought probable that there must be some other cause for this appearance; and I have been induced to conjecture that, admitting the existence of a hollow interior, the difficulty might be met by the additional supposition of a radiated structure, in consequence of which the luminous particles, drawn out into a lengthened form, would in the apparent centre of the tail present their ends only, but on each side of it their full extent, to the observer's eye. At any rate such a conjecture would be fully in accordance with the hypothesis of a polar force of repulsive character, of which there seem to be other evident indications.

May 12, 1859.

RECENT PUBLICATIONS.

Mr. De la Rue on Celestial Photography.

In the *Photographic Journal* for October 1, 1859, we find the substance of an interesting paper on Celestial Photography, read by Mr. De la Rue at the meeting of the British Association held at Aberdeen in the month of September last. We extract the following passages:—

"The mention of stellar photography — one of the last applications of our art — reminds me that the image of such a heavenly body as a star being of the most simple form, it would render what I shall hereafter have to say more easily understood if I were at once to introduce to your notice what happens in applying photography to sidereal astronomy. The optical image of a fixed star, be it remembered, is an optical point, which, in consequence of the properties of light, is seen in the telescope as a very minute disk surrounded by certain rings, which become fainter and fainter as they enlarge; these rings are always more or less broken up, according to the state of the atmosphere. The photographic image, on the other hand, is a mere speck, difficult to find among other specks present in the most perfect collodion film, when viewed with a high magnifying power.

"Let us now suppose we have a suitable telescope turned upon *α Lyræ*, which is conveniently situated, from its great altitude on the meridian, for photography, and is moreover sufficiently brilliant to give an instantaneous picture. If the telescope be steadily supported at rest, the star will, in consequence of the earth's rotation, course along the field of the telescope in a line parallel to the earth's equator; and as it produces an instantaneous picture, the image obtained is a line indicating the path of the star. We should be led to expect, *à priori*, that the line, for the short distance it is made, would appear straight; but so far from this being the case, the line is much broken up and disturbed, and consists of an immense number of points, crowded in some places and scattered in others. This arises from disturbances in our own atmosphere, which cause the optical image to flit before the eye, which nevertheless can make out the proper figure of the image, although it dances before it several times in a second, and the mind is able to select and remember only the states of most perfect definition. The photographic plate, however, remembers and records all the disturbances, and hence presents, as a result, a number of positions of the point of light, and consequently a less beautiful picture than we see optically.

"In the foregoing remarks it was supposed that the telescope was at rest; but now let us suppose that the telescope is mounted on an axis parallel with the earth's axis, and provided with a driving apparatus, capable of carrying the telescope

round in the direction of the star's apparent path, so equally that, if viewed by a micrometer eyepiece, the star would remain in contact with one of the wires of the eyepiece. The photographic image of a star obtained by a telescope under these conditions, after some seconds' exposure, is not one clear disk or point, but a conglomeration of points, extending over a greater or less surface, according as the atmosphere produces a greater or less flickering.

"A photographic image of a star, after an exposure of some seconds, is consequently a disk of comparatively large dimensions in comparison with the true image, and can be really seen on the plate. It will readily be seen, that as a single point like a fixed star acquires comparatively large dimensions on a sensitised plate exposed for some seconds to its action, so must every optical point in an image of other celestial objects from the same cause occupy a space of greater or less dimensions; hence the photographic image will never be so perfect as the optical image given by the same telescope until we can produce pictures of all objects instantaneously, and we are a long way from this desirable end at present.

"Notwithstanding, however, the disadvantages under which the photographer labours, I have obtained pictures of celestial objects, showing details which occupy a space less than two seconds in each dimension—I might, I think, say even one second. Now two seconds = $\frac{1}{900}$th of an inch on the collodion plate, and a second on the lunar surface at the moon's mean distance, is about one mile. The lunar picture in the focus of my telescope is about $1\frac{1}{10}$th inch diameter, but this varies, of course, with the distance of our satellite from the earth. It will be conceded that much valuable work has already been done, and that if the photographs are taken for a number of years, selenological disturbances will not escape detection if they take place.

"With regard to the size of focus stated, it might be suggested that it would be better to enlarge the image; but this would prolong the time, and allow greater disturbance to take place. Thus the result would not be so good. It is by magnifying the photographs afterwards that we get good positive copies. One of these on the table is about eight inches diameter; that is to say, it is magnified about seven and a half times.

"Occasionally I take photographs of the fixed stars, and have made pictures of the double star, *Castor*, and others, but, as a general rule, I devote my attention chiefly to the moon.

"As in the production of the lunar pictures some few seconds of exposure are required, it is essential to have a clockwork driver to the telescope, capable of adjustment to lunar time, which differs from sidereal time. In my own telescope this is at present effected by altering the length of the conical pendulum or friction governor, thus altering the time of its rotation (or double beat). And this plan, or some modification

of it, is universal. My experience, however, has pointed out several inconveniences in thus changing the speed of the governor or pendulum; and it is my intention to make such alterations in the construction of the clock as will enable me to alter the going of the telescope without changing the rate of the pendulum. This I proposed to do by substituting what is known as the disk and plate in mechanics for the wheel-work now immediately connecting the machinery of the clock with the pendulum,—the disk and plate being capable of producing a variable motion according as the disk is nearer to or farther from the centre of the plate. The pendulum will, by the proposed plan, be driven by frictional contact, and having employed this system in other machinery, I feel persuaded that its application to the clock-driver will not be attended with difficulty.

"Until very lately my lunar pictures were obtained by placing the sensitised plate at the side of the tube, opposite the diagonal reflector of my Newtonian telescope, and hence the light before it reached the plate had been twice reflected. I would remark that it requires a very firm support for the diagonal mirror of even a 13-inch mirror; hence the arm carrying this mirror was firmly screwed to the side of the telescope-tube, and being immovable I could not make experiments in taking the pictures direct; that is to say, with the light only once reflected without some contrivance for removing the small mirror. I have within the last few months contrived a proper apparatus, and I now take celestial pictures at will, either direct or reflected, at the side of the tube, and it does not require more than a minute to change the apparatus to produce either result. So that I can make experiments to determine the relative actinic intensity of the light after one or two reflections. The experiments are still in progress, and have been begun so recently that it is scarcely advisable to hazard a conjecture as to the result; but I may say that I am disappointed as to the increased rapidity of the production of a lunar picture by the direct method over the twice reflection method. And I am inclined to infer that Steinheil's result, as to the loss by reflection of the luminous ray, does not hold as regards the actinic ray.

"The reflecting telescope has considerable advantage over the refracting one for celestial photography, on account of all the rays coming to a focus at the same plane, which is not the case with the refractor; hence the focus having been adjusted for the luminous image, it is correct for the chemical image, and has not to be disturbed as with the refractor. I attributed much of my success to the employment of a reflector, while my fellow-labourers in the same field have used refractors.

"The time occupied in taking lunar pictures varies considerably. It depends on the sensitiveness of the collodion, on the altitude of the moon and her phase. I have recently pro-

duced an instantaneous picture of the full moon, and usually get strong pictures of the full moon in from two to five seconds. Of course it is important to have the collodion in a right state, and to be prepared to operate with clean hands and not with dusty apparatus — the moon as a crescent under like circumstances would require about 20 to 30 seconds, in order to obtain a picture of all the parts visible at the dark limb.

"Portions of the moon equally bright optically, are by no means equally bright chemically; hence the light and shade in the photograph do not correspond with the light and shade in the picture; and therefore the photograph frequently renders visible details which escape optically. Those portions of the moon, near the dark limb, are copied photographically with great difficulty, and it frequently requires an exposure five or six times as long to bring out those portions illumined, by a very oblique ray, as others apparently not more bright when more favourably illuminated. The high ground in the neighbourhood of the southern portion of the moon is more easily copied than the low ground, usually called seas, and I ventured in another place to suggest that the moon may have an atmosphere of great density, but of small extent; and this idea has, I imagine, received some confirmation from a recent observation of Father Secchi's, of the lunar surface polarising light more in the great low-lands, and in the bottoms of the craters, and not appreciably on the summits of the mountain-ridges.

"Photography brings out palpably to our senses several facts, which are of course well known, but we do not always think of them — for example, every 29 days we talk of the full moon. Now there is never a full moon visible except before or just after a lunar eclipse. At all other periods of the full moon we are unfavourably situated for seeing the whole of the illuminated hemisphere. The different apparent diameter of the moon at different times, dependent on her distance from the earth, comes out in unmistakeable prominence. We are familiar with the moon's librations in latitude and longitude, yet we fail to realise the great amount of disturbance unless aided by photography, when we see it palpably before us. Taking advantage of the libration of the moon, we get stereoscopic pictures which present to the eye what the Astronomer-Royal has said was the only experimental proof of the rotundity of our satellite. Mr. Claudet has told us that a dispute has been going on between photographers as to the proper angle for taking photographic pictures, and I infer that one side of the disputants would call my arrangement of the moon-pictures to produce photographs unnatural. But, to use Sir John Herschel's words, the view is such as would be seen by a giant with eyes thousands of miles apart."

Excellent views of *Jupiter* and *Saturn* were exhibited. Amongst the views of *Saturn* exhibited, there was one taken

at the period of the late occultation of the planet by the moon, and on the photographic plate were delineated the planet and the moon — the former as just emerging from the moon's bright limb. The views of *Jupiter* were sufficiently large to give stereographic pictures. With respect to *Saturn*, the pictures were so small as not to be visible in an ordinary stereoscope; but there was exhibited on the table a stereoscopic view of *Saturn*, composed of two photographic reductions from the original drawings of Mr. De La Rue made with great accuracy in 1854 and 1856 respectively — the body of the planet standing out as a spheroid encircled by its rings.

Mr. De La Rue proceeded to give a description of the work performed at Kew Observatory (under the British Association), where, at the suggestion of Sir John Herschel, a photoheliograph had been erected, under the direction of Mr. De La Rue, at the request of the Royal Society. Many obstacles, which have been overcome only by repeated experiments, stood in the way of obtaining good pictures of the sun; and these difficulties arose in a great measure from the extreme brilliancy of the sun's image. Mr. De La Rue described the photoheliograph as follows: — The instrument consists of a three-inch object-glass (made by the late Mr. Ross), corrected specially to insure coincidence of the chemical and visual ray. The image is not received directly on the sensitive plate, as is the case in taking lunar and planetary photographs, but is enlarged before it reaches the plate, by means of a secondary lens, which magnifies the sun's image to about four inches in diameter. The time of exposure is so short, that there is a necessity for a special contrivance for regulating the time of exposure. This is effected by means of a sliding-plate placed just before the secondary lens. In this plate is a slit which is adjustable in width. The plate before taking the picture is held up by means of a thread. In this position the light is shut off from the sensitive plate. When the picture is about to be taken the retaining thread is set fire to, and a spring pulls the plate rapidly across the secondary lens. The time of exposure depends on the rapidity of passage of the sliding-plate before the secondary lens.

There have been recently some remarkable spots on the sun; and several views of that luminary were exhibited by Mr. De La Rue, which showed the progress of these spots across the sun's disk, and the remarkable changes they had undergone during the intervals. The photographs also rendered evident the *faculæ*, or bright spots on the sun. Taking advantage of the change of position of the sun's spots in the interval of one, two, or three days, Mr. De La Rue had produced some stereoscopic views of the sun, by grouping together two photographs taken at those intervals.

The president of the section, Lord Rosse, added some observations on the method of obtaining lunar photographs. He

agreed entirely with Mr. De La Rue as to his use of a reflecting telescope; and also felt sure he had hit upon the right method of procedure in many of the very nice contrivances he had described to them.

The Rev. T. Chevallier remarked upon the interesting question of how to get rid of the great inconvenience of excess of light in taking photographs of the sun; and alluded to the experiments of M. Porro to overcome this difficulty by the application of polarised light for the purpose of getting the image.

Astronomical, Magnetical, and Meteorological Observations, made at the Royal Observatory, Greenwich, in the Year 1857. Under the direction of G. B. Airy, Esq., M.A., Astronomer Royal. 4to. London, 1859.

This volume contains the usual meridional observations of the sun, moon, and planets, and the standard stars of the *Nautical Almanac*. The other stars observed are principally,—a list of stars whose right ascensions have been well determined, used in conjunction with the *Nautical Almanac* stars, for determining clock-error; stars remaining from former years, including stars to complete previous catalogues; moon-culminating stars; stars which have been occulted by the moon; stars observed with comets; variable stars discovered by Mr. Hind; stars observed with the zenith sector at the Cape of Good Hope; stars which pass near the north horizon; stars suspected of having, or known to have, large proper motions; stars near the pole, used by Mr. Carrington for determination of azimuthal errors; stars compared with *Sirius;* and, finally, stars observed by Lieut. Tennant in the determination of the latitude of Karachi in connexion with the great Indian Survey.

The volume also contains observations of the moon with the altazimuth; observations with the reflex zenith tube; and observations of occasional phenomena with the north and east equatoreals.

ERRATA.

Vol. XIV., page 53, foot-note, Hooke, as early as 1679, remarked that bodies falling from an elevated position deviate a little towards the south. The merit of having first called attention to this fact was by an oversight attributed to Oersted in the foot-note here referred to.

Vol. XVIII., page 324, line 5 from bottom, *for* 4544 days, *read* 4·544 days.

— — — 140, in the Apparent N.P.D. of *Irene*, Jan. 28, *for* 77° 20′ 36″·84, *read* 77° 20′ 28″·83.

Vol. XIX., page 1, line 1, *for* Jackson, *read* Graham.

— — — 1, line 2, *for* Nicholl, *read* Nichol.

— — — 18, line 7 from the bottom. The sentence beginning with "To the naked eye," &c., and ending with "*Ursæ Majoris*," should have been attached to the observations of Oct. 5 instead of those of Oct. 2.

— — — 28, line 18 from bottom, *for* Lieut. Chinmo, *read* Lieut. W. Chimmo.

— — — 79, line 10–12, 18–20, the numbers here stated refer to seconds of arc.

— — — 79, line 23, *for* uninterruptedly, *read* interruptedly.

— — — 86, line 20, *for* 11 A.M. *read* 11 P.M.

— — — 198, *for* $c = 280{\cdot}4 \pm 83$, *read* $c = 280{\cdot}4 \pm 8{\cdot}3$.

— — — 198, *for* $\delta r = (\ldots) \sin^2 \lambda$, *read* $\delta r = (\quad) \sin^2 2\lambda$.

— — — 291, lines 10 and 18 from top, *for* ξ *Boötis read* ι *Boötis*.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1859, *TO JUNE* 1860.

VOL. XX.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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1860.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *Nov.* 11, 1859. No. 1.

The Rev. R. MAIN, President, in the Chair.

Lieut. Wm. Chimmo, H. M. S. Seagull,

was balloted for and duly elected a Fellow of the Society.

The Astronomische Nachrichten.

A new volume of this important periodical, the fifty-second, is about to commence. The price of a volume is fifteen shillings, payable in advance. There are generally published two volumes in the course of a year. Subscriptions are received by Mr. Williams, Assistant-Secretary of the Society. It may be mentioned that the *Astronomische Nachrichten* admits into its columns contributions in any other European language besides the staple German.

The Astronomer Royal has lately circulated the following Notice :—

"The construction of the meridional instrument, and the general arrangements in this observatory, offer great advantages for the meridional observations of the small planets ; and it has been my wish to maintain these observations with the utmost practicable regularity. This, however, can be done only with the aid of a very approximate ephemeris.

"In general, ephemerides of the small planets are not wanting; but their errors are frequently so large as to make it nearly impossible for observers to assure themselves that the object which they see in the meridional telescope is the planet which they are seeking. A great amount of labour is thus wasted. And, finally, I have been compelled to lay down, as a general rule, that small planets are not to be sought on the meridian unless we have a pretty accurate knowledge of the error of the ephemeris.

"It is much to be wished that information on the magnitude of the error could be furnished to me before the meridional observations commence. As good extrameridional determinations are usually obtained before meridional instruments can be used, I venture to request that, as soon as you have secured one or two certain observations of a planet, you will have the kindness to communicate to me a comparison of the observed places with the corresponding places in any published ephemeris. This would probably enable me to observe the planet on the meridian through its entire meridional apparition.

Royal Observatory, Greenwich,
November, 1859.

Measures of Saturn and his Rings. By Captain W. S. Jacob, Director of the Madras Observatory.

Measures of the Satellites of Saturn with the Madras Equatoreal. By Captain W. S. Jacob.

Measures of Saturn and his Satellites. By Captain W. S. Jacob.*

The first of these papers contains a series of measures of *Saturn* and his rings, executed at the Madras Observatory in the month of January, 1856. The second paper contains a series of measures of the satellites of *Saturn*, executed at the same observatory, and extending from November 6, 1856, to April 10, 1858. In the third paper the author has given a very elaborate discussion of these measures, and has deduced from them corrections to the elements of the orbits of the respective satellites. The materials which form the basis of investigation are divided into two groups, from each of which he derives a corresponding set of elements. The first group consists of measures executed in 1856 and in the early part of 1857. An abstract of the results for this group will be found in Vol. xviii. of the *Monthly Notices* (page 1 *et seq.*). The

* These papers by Captain Jacob, as well as those which follow, and also the papers of which abstracts are given at pages 61 and 68 of vol. xix. of the *Monthly Notices*, are published *in extenso* in the twenty-eighth volume of the *Memoirs* of the Society, at the expense of the India Government.—R. G.

second group consists of measures executed in 1857–8. Upon these data the author thus remarks:—

"The observations of 1857–8 are on the whole slightly inferior to those of the previous season, the weather being on the average less favourable, and the observer not quite in so good working condition, owing to the state of his health; but the distance-measures of *Titan* are at least equally good, and those near elongation more numerous, so that the mean result should be more trustworthy; also there were two nights of such exceptional fineness as to admit of distance-measures being taken of *Enceladus*."

The following are the elements of the five exterior satellites as deduced from the observations of 1857–8. The epoch of mean longitude is 1858, Jan. 0·0.

	Japetus.	Titan.	Rhea.	Dione.	Tethys.
	° ′	° ′	° ′	° ′	° ′
ι	294 26·1	260 26·5	215 14·2	245 19·2	47 15·2
ϵ	18 49·4	27 33·8	28 13·7	28 10·4	28 10·4
$\S\S$	142 46·2	167 54·5	166 49·6	167 38·0	167 38·0
ϖ	353 30	257 27	69 34	330 10	158 47
e	·027960	·029236	·00252	·00470	·00874
a	514″·34	176″·61	76″·12	54″·48	42″·92
μ	4°·538031	22°·577030	79°·690190	131°·534944	193°·69775

The author is disposed to regard the elements deduced from the first series of observations as more trustworthy than those deduced from the second, since in the former case the position of the planet was more favourable for determining ι, ϵ and Ω. He remarks, however, that in the case of *Titan* the second set of elements present a better agreement with those of Bessel than the first do. They are, in fact, almost identical with the results given by that astronomer in No. 242 of the *Astronomische Nachrichten*, allowing for the annual motion of ϖ and Ω.

The author, referring to the considerable differences observable in the values of ι for *Rhea* and *Tethys*, suggests that it is probably due to the mutual action of the satellites, which must occasion inequalities of long period in several pairs of those whose mean motions are nearly commensurate. Thus the mean motions of *Japetus* and *Titan* are nearly as 1 to 5; of *Titan* and *Rhea*, 2:7; of *Dione* and *Enceladus*, *Tethys* and *Mimas* both, 1:2. As regards the values of a for *Titan*, derived from the two sets of observations, and the effect of their discordance upon the value of *Saturn's* mass, the author makes the following remarks:—

"The most unsatisfactory difference is that in the values of a for *Titan*, amounting to 0″·29, being rather more than I am inclined to admit as mere error of observation, considering the number of measures and the care with which they were made; being also more than twice the sum of the probable errors; as the observations extend *over so* many revolutions, the casual

perturbations would also probably be eliminated, being of an order not exceeding that of errors of observation."

The author suggests that the discordance may arise from two distinct causes; 1st, the long equation between *Japetus* and *Titan*; and, secondly, errors in the tables of *Saturn*, especially in the eccentricity or place of perihelion, which, by affecting the distance of the planet from the earth, would alter the reduction of the observed elongation to the mean distance. He accordingly concludes that until we have obtained corrections to the tables of *Saturn*, or until we have good measures of *Titan's* elongation at opposite points of the planet's orbit, we shall be unable to determine with certainty the value of the mass of the latter. He is of opinion that in the present state of the researches on the subject the mass of the planet may be assumed to be included between $\frac{1}{3500}$ and $\frac{1}{3470}$.

Measures of Jupiter and his Satellites, taken with the Madras Equatoreal. By Captain W. S. Jacob.

These measures of the third and fourth satellites of *Jupiter* were executed during the latter half of the year 1857. They were undertaken with the view of obtaining a test for estimating the value to be assigned to the measures of the Saturnian system made by the same observer with the same instrument; for by comparing the mass of *Jupiter* deduced from the author's measures with the value as already very approximately determined by Airy and others, a fair opinion might be formed respecting the degree of accuracy attributable to the measures. Moreover, as there was reason to suppose that the elements of *Sat.* IV. were not sufficiently exact, measures of that satellite were taken in other positions besides near the greatest elongations and approximate corrections of the elements obtained. The mean value of the Jovian mass as deduced from the measures of both satellites is $\frac{1}{1047\cdot34}$, agreeing almost exactly with Airy's value, $\frac{1}{1046\cdot77}$ (*Mem. Ast. Soc.* vol. x.) and with that assigned by Bessel in the *Astronomische Nachrichten* (No. 443), viz. $\frac{1}{1047\cdot81}$.

Occultations of Stars, observed at Madras. By Captain W. S. Jacob.

These observations extend from February 20, 1856, to December 29, 1857. They contain several interesting cases of the projection of the star on the moon's disk.

Notes on the Zodiacal Light, as observed at Madras in the Years 1856-8, *also on the Brightness of certain Stars.* By Captain W. S. Jacob.

The observations which form the groundwork of these notes extend from the 24th of January, 1856, to the 10th of

April, 1858. Their number, however, is inconsiderable, the author's attention having been mainly occupied with other matters. We quote the following remarks:—

"The light at its brightest was considerably fainter than the brighter portions of the milky way, and shaded off so gradually that its limit could hardly be guessed at within several degrees. Colour white, much like that of the milky way, with at times a very slight bluish, and at others a yellowish hue,—tints most probably due to our atmosphere. The great variations in extent and intensity, sometimes occurring in the course of a few minutes, are most probably also dependent on the transparency of our atmosphere; but these variations interfere considerably with any attempt to determine its exact figure or dimensions. The outline generally appeared of a parabolic or probably elliptical form, and it would seem to be excentric as regards the sun, and also inclined, though but slightly, to the ecliptic. In one point it would seem to extend to or beyond the earth's orbit, viz. at about 100° or 105°; indeed, from the middle of November till the end of February, the elongation in a clear sky appears to exceed 90°. At the part opposite to this there are scarcely any observations, but in April and August the elongation appears not much to exceed 60°.

Observations of Eclipses of Jupiter's Satellites.
By Captain W. S. Jacob.

These observations extend from September 25, 1856, to January 16, 1858. They relate to occultations of the satellites by their primary as well as eclipses.

Observations of Comet VIII., 1858, *Iris, Lutetia, and Mnemosyne.*
By Norman Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

Comet VIII., 1858 (Tuttle's).

Greenwich Mean Time.	Mag.	Apparent R.A.	Log. Par. × Δ	Apparent N.P.D.	Log. Par. × Δ	Comparisons.
h m s		h m s		° ′ ″		
Oct. 8 13 2 55		22 19 54·26	+9·510	69 56 4·3	−0·729	8 with *g* & *h*

Iris.

1858						
July 19 13 2 19		19 45 25·90	+8·993	105 32 46·2	−0·894	14 with *e*
21 12 53 6	9·3	19 43 16·69	+9·000	105 34 3·8	−0·894	8 with *e* & *f*
21 13 17 44		19 43 16·26	+9·135	105 34 4·8	−0·892	10 with *e*
Aug. 1 12 46 50	9·4	19 31 49·21	+9·235	105 42 58·4	−0·889	12 with *c*
5 12 49 58	8·5	19 28 3·59	+9·309	105 47 5·0	−0·885	7 with *b*
6 12 31 21	8·3	19 27 10·17	+9·266	105 48 2·7	−0·887	6 with *b*
7 12 24 37	8·7	19 26 17·82	+9·260	105 49 1·9	−0·888	12 with *b*

Mnemosyne.

1859											
Oct.	[illegible]	12	9	27	[illegible]	23	58	[illegible]	[illegible]	83	31
	4	10	17	42	10·7	23	57	28·69	−8·903	83	40
	5	10	47	37	11·2	23	56	51·01	−8·411	83	50
	5	11	29	28		23	56	49·37	+8·600	83	50

The observation of Tuttle's comet was equatoreal of the Radcliffe Observatory and meter. The comet was also observed simul Quirling, with the heliometer, and I regret append his position, as intended, for compa methods.

The observations of *Iris* were made wi Smythian telescope, at my residence, in South and by means of a sidereal chronometer kindly Royal Geographical Society. The first of the tw on July 21st was determined by Mr. Quirling, lawski method or difference-micrometer, for com ring-micrometer observation taken immediately presents a most satisfactory agreement therewi

Lutetia and *Mnemosyne* were observed wi equatoreal and ring-micrometer, except on Octol first position was taken with the wire-microme was, however, much too faint to bear the requis and the second place is consequently the most

The ring-micrometer, though much used a but little employed in this country for planeta and indeed a general prejudice appears to prev as to deter amateurs from giving it a fair tri minor planets are for the most part [illegible]

comet is bisected by a thick wire, or the straight edge of a bar, than by a segment of a circle. But for a minute planet the ring is less troublesome, as one comparison-star is sufficient, while the difference-micrometer requires two, and therefore more reduction. That the calculations and the long refraction correction ever necessary are tedious must be admitted, but these are considerations which ought not to outweigh the importance of procuring positions of an object beyond the reach of meridian instruments or illuminated fields. In order to give some idea of the degree of accuracy attainable in the determination of the radii of the outer and inner edges of a ring, I beg leave to record the measures recently made here, of a new micrometer with which we have provided ourselves in case of need, together with the stars employed, and probable errors of single and mean results. It must of course be borne in mind, that in using different pairs of determining stars, the errors of the actual result depend far more upon the catalogue differences of N.P.D. than upon the observation, which is one of extreme accuracy. Bessel's incomparable *Verzeichniss von 53 Sternen der Plejaden* (*Ast. Unters.* vol. i. p. 237) affords the best and readiest choice of suitable pairs throughout the heavens; and if the night be slightly cloudy or foggy so much the better, the brighter, *i.e.*, the standard stars of the group can be employed, instead of the 8th or 9th magnitude stars preferable in a clear sky. Our results are these:—

Outer edge of the Hartwell equatoreal ring-micrometer.

	Determining pair of Stars.	Authority.	Radius.	Measures.	Probable errors of one measure.	Probable errors of mean result.
			″		″	″
1859, April 6	2665 Gr. & ✱ 19′ *np* 3ˢ	Rad. Obs.	595·83	10	±0·48	±0·15
Oct. 17	18 Tauri & 22 Tauri	Bessel	596·30	10	±0·43	±0·14
	Adopted mean		596·1	20	±0·45	±0·10

Inner edge of the Hartwell equatoreal ring-micrometer.

May 9	2678 Gr. & 2687 Gr.	Rad. Obs.	453·82	10	±0·44	±0·14
Oct. 8	Celœno & 22 Tauri	Bessel	453·42	10	±0·48	±0·15
	Adopted mean		453·6	20	±0·47	±0·11

Similarly for the outer edge of the 5-foot telescope ring-micrometer.

1857, Aug. 2	1669 Gr. & R Urs. Maj.	Rad. Obs.	828·06	5		
1858, Feb. 28	Electra & Merope	Bessel	826·35	5		
	Adopted mean		827·2	10	±0·76	±0·24

Inner edge of the 5-foot telescope ring-micrometer.

1857, Aug. 2	1669 Gr. & R. Urs. Maj.	Rad. Obs.	660·67	4		
1858, Feb. 28	Anonyma 25 & 40 Pleiadum	Bessel	660·38	5		
	Adopted mean		660·5	9	±0·65	±0·22

... the epoch 1859·0, were as follows

Reference.	Authority.	Mag.	Mean R
a	A Z 239·50 = A Z 240·39	8·7	19 28
b	A Z 230·161	9·5	19 28 2
c	37403 Lalande	6·5	19 35 2
d	A Z 240·53	8·5	19 38 4
e	A Z 249·9	8·0	19 42 2
f	A Z 249·10	9·0	19 42 26
g	Maedler's General Cat. (33 Pegasi)	...	22 16 52
h	B Z 196	9·0	22 21 4
k	Weisse 23·1011 = 46931 Lalande	9·3	23 49 48
l	Nautical Almanac (ω Piscium)	4·0	23 52 4

Hartwell, November 10th, 1859.

On some lately discovered Double Sta

By M. Otto Struve.

The double stars on which this paper treats h covered by me since the publication of my catal the greater part on occasion of certain researcl dertaken concerning stars with large proper moti only 9 in number, and bear in my catalogue the in the order of their discovery. The approximate correspond to the epoch 1860·0.

No. 531, P. M. 242. $\alpha = 3^h\ 58^m{\cdot}2$: $\lambda =$ - -

Argelander - - -

$\Delta\delta = -1'\ 56''{\cdot}1$. Hence the change in the relative positions of the two stars in the course of about 30 years would only be $+0^s{\cdot}03$ in R.A. and $+1''{\cdot}67$ in Decl.; and supposing that the annual proper motion of 50 *Persei* was exactly determined by the comparison of Argelander's observations with Bradley's to $+0^s{\cdot}0157$ in R.A. and $-0''{\cdot}208$ in Decl., that of P. M. 242 would follow $+0^s{\cdot}0167$ in R.A. and $-0''{\cdot}152$ in Decl.

This pair of stars can pretend to a higher interest still, by the fact that P. M. 242 is in itself a double star of Herschel's first class; being composed of two stars (6.7th and 8.9th magnitude) of 3″ to 4″ distance. The physical connexion of these two stars is proved by our observations continued through 8 years, for until now a change in their relative position cannot yet be made out with certainty. The last measures give,—

$$1859{\cdot}15 \qquad e = 3''{\cdot}48, \qquad P = 150°{\cdot}4.$$

A fourth star (7.8th magnitude) is south proceeding to P. M. 242 at a distance of nearly 4′. This star does not belong to the system, for the changes observed in its position with regard to P. M. 242 correspond exactly to the proper motion of the latter. This star, therefore, would be a very qualified object of comparison for determining the relative parallax of P. M. 242, for which a considerable amount is indicated by the proper motion and by the probable physical connexion with 50 *Persei* at 15′ distance.

No. 532, β Aquilæ, $\alpha = 19^h\ 48^m{\cdot}4$; $\delta = +6°\ 4'$.

This well-known fundamental star has a small companion (11.12th magnitude), at the distance of about 12″, which participates in its large proper motion of nearly half a second a-year. It is, therefore, a physical double star. Micrometrical measures continued through 8 years do not yet indicate any change in the relative position due to orbital motion. My last observations give,

$$1859{\cdot}65 \qquad e = 12''{\cdot}34, \qquad P = 17°{\cdot}0.$$

The observations of this pair of stars belong to the most difficult, from the splendour of the principal star and the faintness of the companion.

No. 533, 4 Delphini, $\alpha = 20^h\ 32^m{\cdot}3$; $\delta = +9°\ 36'$.

In appearance this double star is analogous to β *Aquilæ*, being composed of two stars (4.5th and 11.12th magnitude), 10″ distant, and the companion being exactly north, but contrary to β *Aquilæ*. The observations continued through 8 years prove that the small star does not participate in the proper motion (of 0″.33 a-year) of the principal stars. Hence it follows that they are not physically connected, but form only

for the present time an optical double star. My last measures are,—

1859·62 $\quad e = 10''\cdot09, \quad P = 359°\cdot4.$

This position, combined with the proper motion of 4 *Delphini*, shows that within 30 or 40 years the distance of the small stars will exceed 16″, so that then, according to the limits adopted by my father, this pair will no longer have the right to figure in the lists of double stars.

Argelander mentions, in a note to his catalogue, that there is a star of the 9th magnitude, $14^s\cdot3$ following 4 *Delphini*, and 0′·7 to the south. This star, estimated by me of the 8th magnitude, has been repeatedly joined by micrometrical measures with the bright stars. Until now there is not the least trace of change in their relative position, whence we must conclude that we have here another binary system of very wide aperture of the orbit. My observations give the mean relation:

1855·02 $\quad e = 214''\cdot72;\ P = 100°\ 45'\cdot0$

or Δ R.A. $= +14^s\cdot263;\ \Delta$ Decl. $= -40''\cdot05.$

This Δ R.A. compared with that given in Argelander's Catalogue, is in itself a sufficient proof that both stars are transported on the celestial sphere by the same proper motion.

No. 534 $\quad \alpha = 17^h\ 58^m\cdot9;\ \delta = +21°\ 25'.$

This double star has been discovered on looking out for No. 341 of our catalogue, to which it precedes 52^s nearly upon the parallel. The magnitudes of the two components are 7.8 and 9.10. Until now this pair offers no particular interest, but must be noticed to avoid mistakes with the following pair. For the mean of three observations we have found:

1852·95 $\quad e = 2''\cdot14;\ P = 270°\cdot7.$

No. 535, δ Equulei $= \Sigma\ 2777,\ \alpha = 21^h\ 7^m\cdot7;\ \delta = +9°\ 27'.$

In the introduction to the *Positiones Mediæ Stellarum Duplicium*, p. 232, my father has proved that the small star discovered by W. Herschel in the neighbourhood of δ *Equulei* changes its relative position according to the proper motion of the principal stars; whence it follows, that the two stars form only optically double stars. At present even the designation of optical double stars in the limits introduced by my father, would no longer be quite exact, as the distance exceeds already 32″. A very careful calculation, executed by M. Schiaparelli, a young Italian astronomer residing at Pulkowa, on the Dorpat observations, combined with the whole series of measures I have made here until now, confirms entirely my father's conclusions. On considering the long series of measures made on this star at Dorpat and Pulkowa, it will rather excite surprise when I

mention that only in 1852 I happened to discover that the principal star was in itself a close double star. However, this late discovery explains itself sufficiently by the circumstance that the observation of the distant companion required only slow magnifying powers, and could be made in ordinary atmospheric condition, without expecting the best definition, which is a necessary condition for seeing the division of the principal star even, as it appears, at the epoch of the greatest distance of the two stars.

The two components of δ *Equulei* are nearly of the same magnitude. Therefore, until now we have no certain indications to discern the two opposite directions. As this star might pretend to a higher interest, I will give here the entire list of my observations on it, which, however, is only of very moderate length, for the reason that a definition, as it is wanted for this object, does not frequently occur in the course of a year.

1852·63 $e = 0''\cdot37$ $P = 16°\cdot3$
1852·66 0·34 12·6 } the two stars distinctly separated

1853·71 oblong, distance does not exceed $0''\cdot2$, $P = 191°\cdot7$

1854·69
1856·58 } Simple

1857·67 oblong, distance of centres by estimation $0''\cdot16$, $P = 199°\cdot8$

1857·67 ,, ,, ,, 0 ·18 200 ·8

1858·59 the two stars sometimes distinctly separated, distance by estimation $0''\cdot30$, $P = 8°\cdot9$

1859·65 oblong, distance by estimation $0''\cdot30$, $P = 6°\cdot6$.

Though in the last observation, the distance was found by estimation equal to that I had estimated the year before, I am inclined to think that in the interval it has decreased a little, on the ground that in the present year I never have been able to separate distinctly the two stars.

The preceding observations show, 1, that the two stars form a binary system; 2, that there is a very rapid motion in the orbit; 3, that apparently the plane of the orbit is not much inclined to the line drawn from the sun; but on account of the near equality of the two stars it cannot yet be decided if the same star, after the period of being simple, has emerged again on the same or on the opposite side of the other star. Evidently we have here an object analogous to 42 *Comæ Ber.*, with this difference that in the present instance the axis of the orbit appears to be still smaller, and again the period of revolution even more rapid than in the other case.

No. 536, Arg. 533 $\alpha = 22^h\ 51^m\cdot5$; $\delta = +8°\ 37'$.

According to Argelander this star has a proper motion of about half a second a-year. It is composed of two stars 7th and 7.8th magnitude. For 1852–53 the distance between them

did not amount to 0″·4. In the present year the star appeared simple in a very good condition of the atmosphere. These short statements are sufficient to show that here again we have to do with an interesting binary system with rapid orbital motion.

No. 537, $\alpha = 21^h\ 51^m{\cdot}5$; $\delta = +59°\ 13'$.

This double star (8th and 10.11th magnitude) has been entered in our lists only for its neighbourhood to our No. 458, to avoid mistakes. The relative position was found,

1856·73 $\quad e = 2''{\cdot}59$, $P = 11°{\cdot}3$.

No. 538, σ Coronæ = Σ 2032 $\quad \alpha = 16^h\ 9^m{\cdot}4$; $\delta = +34°\ 13'$.

Besides the two stars that form this well-known binary system and the third star *c* of the 10.11th magnitude, which my father has proved (*Pos. Med.* p. 229) not to belong to the system, there is at the distance of about 20″ from the principal couple a very faint object. Observations continued through six years prove that it likewise does not belong to the system, as it is not affected with the proper motion. The mean of four measures I have found to be,

1854·43 $\quad e = 20''{\cdot}46$, $P = 231°{\cdot}6$.

The observations of this star are, on account of its faintness, the most difficult I ever have made with our refractor, and as by the effect of proper motion it approaches to the principal pair, in a few years hence it will perhaps be quite impossible to see it. According to our mode of estimation, the 13th magnitude must be assigned to it, as it is very near the limit of the penetrating power of the Pulkowa refractor. In this regard for the next time it is a fair object for testing the power of other telescopes in comparison with ours.

No. 539, Σ 1516 $\quad \alpha = 11^h\ 6^m{\cdot}0$; $\delta = +74°\ 14'$.

This double star had been observed 23 times by my father at Dorpat, and at least 12 times by me at Pulkowa, before I remarked last year that there was at the distance of only 8″ a third star of 10.11th magnitude. If this star has been simply overlooked, or if the late discovery is to be attributed to variability of light, future observations must decide. Though the proper motion of the principal star is more than 0″·4 a-year in space, the interval between my extreme observations is yet so short as not to admit of a decision in regard to the existence of a physical connexion, but next year this point might be settled. In the mean of three observations we have the following relation,—

1858·86 $\quad e = 8''{\cdot}01$; $P = 291°{\cdot}6$.

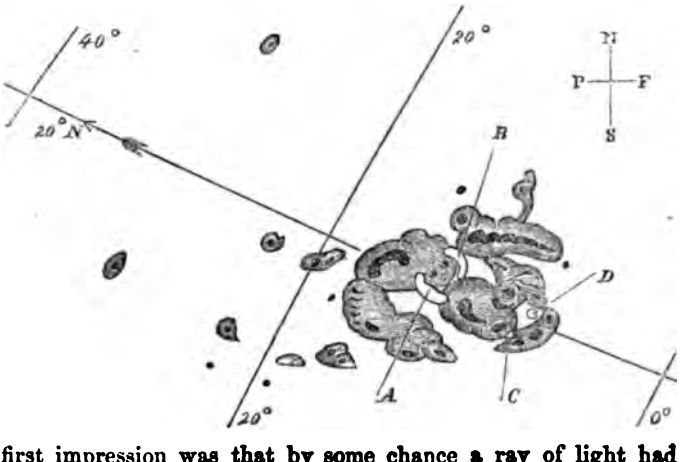
The measures of the principal pair obtained since the publication of the *Positiones Mediæ* have been submitted, in com-

bination with the ancient measures, to a new computation by M. Oom, of the Royal Observatory of Lisbon, at present living at Pulkowa. The results of his computation have entirely confirmed my father's conclusions, that the changes observed in the course of 28 years in the relative positions of the two stars find a complete explanation in the proper motion of the principal star, but the new formula does but very little diminish the discordance of the results obtained in 1823 by transit observations.

Pulkowa, October, 1859.

Description of a Singular Appearance seen in the Sun on September 1, 1859. By R. C. Carrington, Esq.

While engaged in the forenoon of Thursday, Sept. 1, in taking my customary observation of the forms and positions of the solar spots, an appearance was witnessed which I believe to be exceedingly rare. The image of the sun's disk was, as usual with me, projected on to a plate of glass coated with distemper of a pale straw colour, and at a distance and under a power which presented a picture of about 11 inches diameter. I had secured diagrams of all the groups and detached spots, and was engaged at the time in counting from a chronometer and recording the contacts of the spots with the cross-wires used in the observation, when within the area of the great north group (the size of which had previously excited general remark), two patches of intensely bright and white light broke out, in the positions indicated in the appended diagram by the letters A and B, and of the forms of the spaces left white. My



first impression was that by some chance a ray of light had penetrated a hole in the screen attached to the object-glass, by

which the general image is thrown into shade, for the brilliancy was fully equal to that of direct sun-light; but, by at once interrupting the current observation, and causing the image to move by turning the R.A. handle, I saw I was an unprepared witness of a very different affair. I thereupon noted down the time by the chronometer, and seeing the outburst to be very rapidly on the increase, and being somewhat flurried by the surprise, I hastily ran to call some one to witness the exhibition with me, and on returning within 60 seconds, was mortified to find that it was already much changed and enfeebled. Very shortly afterwards the last trace was gone, and although I maintained a strict watch for nearly an hour, no recurrence took place. The last traces were at C and D, the patches having travelled considerably from their first position and vanishing as two rapidly fading dots of white light. The instant of the first outburst was not 15 seconds different from $11^h\ 18^m$ Greenwich mean time, and $11^h\ 23^m$ was taken for the time of disappearance. In this lapse of 5 minutes, the two patches of light traversed a space of about 35,000 miles, as may be seen by the diagram, which is given exactly on a scale of 12 inches to the sun's diameter. On this scale the section of the earth will be very nearly equal in area to that of the detached spot situated most to the north in the diagram, and the section of *Jupiter* would about cover the area of the larger group, without including the outlying portions. It was impossible, on first witnessing an appearance so similar to a sudden conflagration, not to expect a considerable result in the way of alteration of the details of the group in which it occurred; and I was certainly surprised, on referring to the sketch which I had carefully and satisfactorily (and I may add fortunately) finished before the occurrence, at finding myself unable to recognise any change whatever as having taken place. The impression left upon me is, that the phenomenon took place at an elevation considerably above the general surface of the sun, and, accordingly, altogether above and over the great group in which it was seen projected. Both in figure and position the patches of light seemed entirely independent of the configuration of the great spot, and of its parts, whether nucleus or umbra. The customary observation was shortly resumed, and the diagram engraved, as well as the larger drawing exhibited at the Meeting on Nov. 11, was deduced from an exact reduction of the recorded times.

It has been very gratifying to me to learn that our friend Mr. Hodgson chanced to be observing the sun at his house at Highgate on the same day, and to hear that he was a witness of what he also considered a very remarkable phenomenon. I have carefully avoided exchanging any information with that gentleman, that any value which the accounts may possess may be increased by their entire independence.

(Mr. Carrington exhibited at the November Meeting of the

Society a complete diagram of the disk of the sun at the time, and copies of the photographic records of the variations of the three magnetic elements, as obtained at Kew, and pointed out that a moderate but very marked disturbance took place at about $11^h\ 20^m$ A.M., Sept. 1st, of short duration; and that towards four hours after midnight there commenced a great magnetic storm, which subsequent accounts established to have been as considerable in the southern as in the northern hemisphere. While the contemporary occurrence may deserve noting, he would not have it supposed that he even leans towards hastily connecting them. "One swallow does not make a summer.")

Mr. Carrington regrets having omitted previously to communicate the following passage in a letter from M. Schwabe, dated March, which is plainly intended for others as well as himself:—"The remarks and drawings of *Jupiter* in the *Monthly Notices* by Dawes, Murray, and Lassell, have much interested me, as I also have long followed the changes which take place in his bands. Although none of the observations in the *Monthly Notices* coincide in time with those which I have obtained, yet I recognise a good accordance in general, especially in the two figures given by Sir W. Murray.* Yet I do not find indicated in those two figures, that, as I observe, the whole surface of *Jupiter* is covered with delicate parallel dark lines, which are the most conspicuous in the dark belts. The most delicate ones are to be found in the bright equatoreal zone, the zone of Schroeter, in the middle of which, or on the equator itself, they appear rather more distinctly, although this middle line is not always there. . . . In the figures of the *Monthly Notices* there are representations of bright round beads which I do not observe under that character. My impression is that I see longish bright places between the above-named parallel lines, in which, or through which, the bright surface of the planet is discernible. I much wish to learn whether Messrs. Dawes, Murray, or Lassell, or either of them, see also those delicate parallel lines. The large and almost black spots which were seen a short time ago, also dissolved into similar but much broader and darker parallel lines. . . ."

On a curious Appearance seen in the Sun.
By R. Hodgson, Esq.

"While observing a group of solar spots on the 1st September, I was suddenly surprised at the appearance of a very brilliant star of light, much brighter than the sun's surface,

* Lassell (?).—Ed.

... 2 in., 100, with a pale
glass; the whole aperture was used with a di

"The phenomenon was of too short dura
micrometrical drawing, but an eye-sketch
which the enlarged diagram* has been made;
tograph taken at Kew the previous day, the s
appears to have been about 2^{m} 8^{s}, or (say) 60,

"The magnetic instruments at Kew wer
disturbed to a great extent.

"*Nov.* 11, 1859."

Results of the Meridional Observations of . Occultations of Stars by the Moon; and Jupiter's Satellites, observed at the Roy Greenwich.

(***Communicated by the Astronomer Royal.***)

Hebe ⓐ.

Mean Solar Time of Observation.			Apparent R.A.
		h m s	h m s
1859, Oct.	3	13 7 54·5	1 56 42·67
	19	11 54 13·9	1 45 54·83
	26	11 21 36·8	1 40 48·24
	29	11 7 45·5	1 38 44·34

Psyche ⊙.

Mean Solar Time of Observation.			Apparent R.A.
		h m s	h m s
1859, July	4	13 35 45·8	20 25 52·01
	7	13 21 57·8	20 27 [illegible]

Parthenope (11).

Mean Solar Time of Observation.					Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1859, Oct.	22	14	58	42·8	5	2	43·71	73	23	41·52
	26	14	41	39·2	5	1	23·55	73	30	20·96

Melpomene (18).

Mean Solar Time of Observation.					Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1859, July	1	12	1	32·9	18	39	34·00	98	50	50·02
	4	11	46	39·0	18	36	27·31	99	2	51·57
	5	11	41	41·0	18	35	25·02	99	7	19·37
	6	11	36	43·0	18	34	22·74	99	11	51·77
	7	11	31	45·1	18	33	20·66	99	16	35·32
	11	11	11	56·9	18	29	15·44	99	37	7·11
	12	11	7	1·3	18	28	15·52	99	42	41·44
	15	10	52	17·9	18	25	19·39	100	0	2·50
	16	10	47	25·0	18	24	22·30	100	6	6·06
	19	10	32	52·6	18	21	37·18	100	25	2·49
	26	9	59	40·3	18	15	55·33	101	13	7·73
Aug.	12	8	44	54·0	18	7	58·16	103	22	56·00

July 6.—The observation of N.P.D. has been altered by increasing the reading of each microscope-micrometer one revolution.

Massilia (20).

Mean Solar Time of Observation.					Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1859, July	1	14	9	48·0	21	7	52·91	105	24	56·68
	7	14	5	13·7	21	7	14·43	105	27	33·09
	11	13	46	44·6	21	4	28·56	105	38	50·40
	12	13	42	4·3	21	3	43·99	105	41	51·96
	13	13	37	22·8	21	2	58·36	105	44	57·50
Aug.	1	12	5	33·9	20	45	48·88	106	54	18·04
	2	12	0	39·2	20	44	49·99	106	58	8·15
	4	11	50	50·1	20	42	52·34	107	5	56·09
	5	11	45	55·7	20	41	53·72	107	9	44·86
	12	11	11	44·3	20	35	12·57	107	35	53·32
	15	10	57	19·4	20	32	28·70	107	46	25·26
	19	10	38	2·8	20	29	1·48	107	59	37·28
	27	10	0	34·2	20	22	59·18	108	22	51·71
	30	9	46	52·3	20	21	4·67	108	30	10·94
	31	9	42	21·3	20	20	29·43	108	32	32·44
Sept.	6	9	15	47·6	20	17	30·77	108	44	28·56
	21	8	13	41·7	20	14	22·97	109	0	7·48
	24	8	2	1·7	20	14	30·71	109	0	51·09
	29	7	43	8·0	20	15	16·66	109	0	20·53
Oct.	1	7	35	46·2	20	15	46·72	108	59	29·60

Proserpine (26).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Oct. 19	13 2 39·6	2 54 31·83	73 59 24·47
31	12 4 50·5	2 43 51·89	74 33 51·28

Euterpe (27).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, July 4	10 18 6·8	17 7 40·59	112 36 47·23
6	10 8 37·3	17 6 2·58	112 35 53·64

Lutetia (21).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, July 7	12 30 15·6	19 32 0·69	114 34 34·00
12	12 5 50·1	19 27 14·03	114 54 52·53
Aug. 2	10 24 45·3	19 8 40·30	115 59 19·47
4	10 15 34·4	19 7 21·01	116 3 6·19
5	10 11 1·9	19 6 44·33	116 4 54·38
16	9 23 13·0	19 2 9·75	116 17 31·01
19	9 10 54·9	19 1 39·26	116 18 56·60

Fides (37).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Aug. 15	12 31 24·7	22 6 55·80	105 13 9·10
22	11 57 31·3	22 0 32·66	105 41 35·10
24	11 47 49·6	21 58 42·52	105 49 19·84
Sept. 24	9 23 0·4	21 35 42·64	106 59 30·59
Oct. 1	8 53 9·9	21 33 23·13	106 58 29·28

Mnemosyne (57).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1859, Sept. 29	11 27 51·6	0 0 37·19	82 53 7·95
Oct. 1	11 18 42·8	23 59 19·97	83 11 59·18
4	11 5 2·8	23 57 27·39	83 40 34·42

The observations of N.P.D. have been corrected for parallax, excepting in the case of *Mnemosyne*, the distances of which from the earth were not at hand.

Occultations of Stars by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
1859, April 13	37 Sextantis disapp.	Dark	8 44 35·0	E.
13	37 Sextantis disapp.	Dark	8 44 36·1	C.
Sept. 21	μ^2 Cancri disapp.	(*a*) Bright	13 30 1·0	E.
21	μ^2 Cancri reapp.	(*b*) Dark	14 20 52·4	E.
Oct. 14	19 Tauri disapp.	(*c*) Bright	11 44 9·4	C.
14	20 Tauri disapp.	(*d*) Bright	12 1 18·6	C.

(*a*) Uncertain to about a second of time.
(*b*) Instantaneous; the moon's dark limb was distinctly visible.
(*c*) Star exceedingly faint when close to the moon.
(*d*) Very doubtful, the sky being cloudy.

The initials E. and C. are those of Mr. Ellis and Mr. Criswick.

The observations of the occultation of *Saturn* by the moon on May 8, and that of the disappearance of a small star which was occulted about the same time, have been already published in the *Monthly Notices*, vol. xix., No. 7.

Eclipses, Occultations, and Transits of Jupiter's Satellites.

Day of Observation. 1859.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
Jan. 10	II	Ecl. reapp.	5 11 15·2	L
19	I	Ingress, 1st cont.	5 30 17·4	C
19	I	,, central bis.	5 31 47·2	C
19	I	,, total ingr.	5 34 16·7	C
19	I	Egress, 1st app.	7 43 25·5	C
19	I	,, last cont.	7 47 54·8	C
22	II	Ingress, 1st cont.	10 44 49·4	J C
22	II	,, total ingr.	10 50 18·5	J C
22	II	Egress, 1st app.	13 20 53·9	J C
22	II	,, last cont.	13 26 23·0	J C
26	I	Ingress, central bis.	7 22 14·7	E
31	II	Occ. disapp.	8 12 33·9	E
Feb. 3	III	Occ. disapp. 1st cont.	8 37 5·7	L
3	III	,, central bis.	8 39 30·3	L
3	III	,, total disapp.	8 42 24·8	L
7	II	Occ. disapp. central bis.	10 38 48·4	E
10	I	Ecl. reapp.	11 37 11·7	D

Day of Observation. 1856.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
Feb. 21	III	Ingress, 1st cont. (*a*)	6 9 20·6	C
21	III	,, central bis. (*a*)	6 12 50·0	C
21	III	,, total ingr. (*a*)	6 15 19·6	C
23	II	Ingress, total ingr.	10 6 22·5	E
23	II	Egress, last cont.	12 41 57·1	E
Mar. 4	II	Occ. disapp. 1st cont. (*b*)	7 27 10·4	W C
4	II	,, central bis. (*b*)	7 28 40·3	W C
4	II	,, total disapp. (*b*)	7 30 54·8	W C
5	I	Occ. disapp. central bis.	8 21 7·3	E
18	III	Occ. reapp. last cont. (*c*)	11 2 25·6	D
21	I	Ecl. reapp.	10 14 35·4	E
22	II	Ecl. reapp.	7 9 5·5	D
April 6	I	Ecl. reapp.	8 34 54·2	D
13	I	Ecl. reapp. (*d*)	10 32 44·0	E
21	I	Egress, central bis.	8 39 40·6	E
Aug. 20	IV	Occ. disapp. total dis. (*e*)	16 0 31·9	D
Oct. 19	III	Ecl. disapp. (*f*)	13 16 41·7	E
19	III	Ecl. reapp. (*g*)	16 21 15·3	L
19	I	Ecl. disapp. (*g*)	17 14 57·0	L
22	I	Egress, last cont.	12 33 13·5	C
26	IV	Ecl. reapp. (*h*)	10 31 39·0	E

(*a*), Cloudy. (*b*), Planet exceedingly faint at first; the time of total disappearance is, therefore, the most reliable. (*c*), Planet excessively tremulous; observation very unsatisfactory. (*d*), Observation not good; *Jupiter* very low and tremulous. (*e*), Exceedingly doubtful; planet behind thin cloud, and very tremulous; observation uncertain to at least a minute. (*f*), At the time noted, the last faint trace of the satellite disappeared. (*g*), Satisfactory. (*h*), Unsatisfactory, *Jupiter* being very near the horizon; the observation uncertain to a quarter of a minute.

The initials D, E, C, L, J C, and W C, are those of Mr. Dunkin, Mr. Ellis, Mr. Creswick, Mr. Lynn, Mr. Carpenter, and Mr. Christy.

Catalogue of Multiple Stars observed in Upper India in 1855–6.

By Charles Gubbins, Esq.

These observations were made at the Civil Station of Allygurh, situated in the Upper Doab, between the Ganges and Jumna, about 600 feet above the level of the sea. Its geographical position was found to be

Latitude N.	27° 53′ 56″
Longitude E.....	78 7 10

This was determined by triangulation from the church not far distant, which had been one of the stations of the second Trigonometrical Survey of India.

The instrument employed in these observations was a small achromatic telescope of $3\frac{1}{2}$ inches aperture, mounted equatoreally. It was supported on a mahogany tripod, terminating in brass caps with levelling screws, which were received on three metal plates, imbedded in circles of masonry on a slightly raised platform. The hour and declination circles were each 7 inches in diameter.

The instrument was furnished with a position-micrometer, to which were adapted two eye-pieces, with powers 40 and 80. Of these the latter, giving a field of 2^m of time, was almost exclusively used. The time was determined with a chronometer which was regulated, in the beginning of the operations, by observations of the eclipses of *Jupiter's* satellites, and subsequently by observing the distances of known stars from the meridian, and comparing the results with those contained in Dietrichsen's *Almanac*, the only substitute which the author possessed for the *Nautical Almanac*.

The Catalogue contains the approximate places of 122 double stars, with micrometrical measures of their angles of position and distances. The nomenclature used by the author is that of the Catalogue contained in Sir John Herschel's *Results of Astronomical Observations made at the Cape of Good Hope*, the only astronomical work on the subject of double stars to which he had access at the time of his observations.

The author states, that although he commenced his observations in the autumn of 1855, it was not until the beginning of the following year that he had acquired sufficient mastery over his instrument to be able to turn it to the best possible account; and by the middle of summer he was interrupted by a severe illness, which eventually compelled his return to England.

The following is a synopsis of stars which appear from the author's measures to indicate a change of angle of position. This conclusion, he states, being also supported by Captain Jacob's measures:—

Nomenclature of Star.	
617	Direct.
3447	Do.
p, 6, Eridani	Retrograde.
39 Eridani	Direct.
Σ 1121	Retrograde.
Δ 70	Do.
4608	Direct.
h Centauri	Retrograde.

Nomenclature of Star.	
54 Hydræ	Retrograde.
B 7080	Do.

Accompanying the Catalogue are charts of two clusters of stars (H vii, 17, and H viii, 38), which, the author states, were constructed with the utmost care. "The latter," he adds, "may be depended on for accuracy in all its leading features; and the great number of cross-measurements and eye-allineations will afford facilities for correcting minor errors. Of the other I cannot speak so confidently, as the great majority of its stars were too faint to be seen with light enough to show the wires; and the measurements of angles can therefore only be regarded as approximations."

The author's paper has been deposited in the archives of the Society. It may hereafter afford useful corroborative evidence in cases where a change of relative position in any of the objects to which it refers shall have been suspected.

Two letters from the Rev. J. B. Kearney, of King's School, Canterbury, to Sir John Herschel, were read. In the first of these, dated February 9, 1858, Mr. Kearney expresses his opinion that the star *Capella* is diminishing in brightness. He had watched the star attentively with the naked eye for fifteen years, and felt a strong suspicion that it was waning in lustre. In the years subsequent to 1843 its superior brightness* attracted his attention. In the second letter, dated September 9, 1859, he states that the star had resumed its original brightness. We extract the following passage:—

"My favourite star *Capella* has reasserted her superiority. From the receipt of your letter, dated February 25, 1858, I have carefully watched *Capella*, and compared it with *α Lyræ* and *Arcturus*. From February '58 to April '58, *Capella* and *α Lyræ* appeared about equal. From August '58 to November '58, I fancied that *Capella* gained on *α Lyræ* after November 30, '58. I also fancied that *Capella* lost ground compared with *α Lyræ* till August 19, '59, when, at 8 o'clock, *Capella* was brighter, beyond all possibility of doubt, than either *Arcturus* or *α Lyræ*. At 10 o'clock *Capella* was magnificent, very far surpassing all its rivals. It flashed all manner of colours, blue and crimson being perhaps the most striking. *Capella* was very decidedly A 1 on August 26 and September 1, particularly

* Sir John Herschel, in a paper read January 10, 1840, on the variability of *α Orionis*, published in vol. xi. of the *Memoirs* of the Society, remarks, in a foot-note, page 269, "I have a strong impression that *Capella* within my recollection has increased in brightness. M. Struve is of the same opinion." —*R. G.*

the latter, when it seemed as brilliant as on August 19, except as regards change of colour. By the way, the colour of *Capella* seems permanently much less blue than it used to be. To-night (9 o'clock) I consider *Capella slightly* first, *Arcturus* a good second, and *α Lyræ*, though *very* bright, third. *Arcturus* seemed A 1 both on Monday and Tuesday last; but it was very damp, so that perhaps *Capella* may not have had a fair chance. I think it may be laid down as certain that both *Arcturus* and *Capella* have lately gained on *α Lyræ*."

Sir John Herschel communicated an interesting series of delineations of the surface of the sun taken between the 27th of August last, and the 12th of September, by the Rev. F. Howlett, a near neighbour of his at Hawkhurst and a gentleman of much skill as a draughtsman, as well as a careful microscopist. Sir John believed that they would interest the members of the Society, and hoped that by the reception they met with Mr. Howlett might be encouraged to continue his record. Some nuclei of very curious forms are represented in these drawings, and excited considerable interest at the meeting.

Sir John Herschel deposited with the Society three MS. volumes of observations of the solar spots made by the late M. Pastorff. These volumes were originally presented by the author to Sir John Herschel. They were now transferred to the Society on the understanding that they should continue to be considered as belonging to Sir John Herschel during his lifetime; but, after his decease, should become the property of the Society. In the meantime, Fellows of the Society interested in the subject of those observations would always have the opportunity of consulting them. The thanks of the Society were awarded to Sir John Herschel for this valuable donation.

MISCELLANEOUS INTELLIGENCE.

Professor Wolf, of Zurich, mentions, in a recent letter, that, in consequence of the liberal aid afforded by a gentleman of his neighbourhood, who has recently succeeded to a considerable property, and who evinces great interest in his researches, such fresh energy has been given to the project of suitably mounting the instruments of the Zurich Polytechnic Institute, that he hopes now shortly to be in a position to take observations of precision. The tenth number of his *Mittheilungen über die Sonnenflecken* is in the press.

M. C. Bulard has been recently appointed by the Prince Napoleon to arrange and duly furnish a small Astronomical Observatory at Algiers. It is proposed that his attention be principally given to physical details of the heavenly bodies, for the delineation of which M. Bulard possesses remarkable aptitude. He is at present provided with a transit instrument and two silvered speculum reflecting telescopes. M. Bulard will gladly receive publications on astronomical subjects and communications addressed to him as

Astronome de l'Observatoire à Alger.

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*Suspected Existence of a Zone of Asteroids revolving between Mercury and the Sun.**

A few weeks since the eminent Director of the Imperial Observatory at Paris addressed a letter to M. Faye, on the subject of the motions of *Mercury*, so forcibly reminding the astronomical public of the period preceding the discovery of *Neptune*, that it will not be matter of surprise that in the minds of many an expectation has been in some degree raised that the comet of the year 1846 may be shortly expected at the other extremity of the planetary system. Some conversation took place at the evening-meeting of the Society, at which the following account of M. Le Verrier's letter was read. The original appeared in the *Comptes Rendus* of the French Institute for the 12th of September.

Substance of M. Le Verrier's Letter to M. Faye.

"In my studies on the planetary motions, I have, as you know, met with many difficulties. The outstanding discordances in the case of *Uranus* were ultimately explained by the discovery of *Neptune*. The difficulties presented by the sun have been considerable; and they led me, first, to revise at great length the processes and principles on which were based the concluded positions of the fundamental stars. I next revised with care the whole theory of the inequalities of the earth; and in the comparison of the theoretical and observed positions, I have discussed not less than 9000 observations from various sources.

"The theory of the sun once placed beyond doubt, it has been possible for me to resume with confidence the study of the movements of *Mercury;* and it is to this portion of my

* Drawn up for the use of the readers of the *Monthly Notices* by a *Member of the* Council.—R. G.

recent labours that on this occasion I particularly call your attention. For this inquiry we possess, for the last century and a half, records of 21 observations of transits of the planet over the sun's disk, which admit of comparisons of the distances of the centres of the sun and *Mercury* at definite instants, liable to error not exceeding a second of arc, and consequently far exceeding in precision the meridian observations of the planet at the earlier date. In my former examinations of the ancient observations of *Mercury*, I had, in 1842, recognised considerable discordances, but into the discussion of which I abstained from entering until the theory of the sun should be more fully and precisely established. Now that this is done, and the positions of the sun given by my latest tables are introduced, systematic discordances remain between the theoretical and observed positions of *Mercury*, which can only be annihilated by such violent suppositions as that Lalande, Cassini, Bouguer, and others, committed errors of whole minutes of time in recording contacts, and errors which have a progressive variation of increase or decrease, wholly impossible. But what is most remarkable, I find that by simply increasing by 38 seconds the secular movement of the perihelion of the planet, all the 21 transits are at once satisfied to such a degree of exactness that the greater part exhibit residues of half-seconds, and a few of seconds only. This empirical correction is accordingly so successful as to show at once that the theory of *Mercury* requires this modification; and it remains to inquire whether any modification of the masses of the exterior planets employed in the calculation of *Mercury's* perturbations will produce the required increase, or whether by its inadmissibility it is necessary to search further." M. Le Verrier then states that if the error is assumed to lie in the mass of *Venus*, a correction of $\frac{1}{10}$th of that mass would be required to meet the case; and he shows such a correction to be inadmissible, from the error it would, per contra, introduce into the theory of the Earth. M. Le Verrier commends the difficulty to the serious consideration of astronomers, and propounds, as an explanation to which he himself inclines, the possible existence of a planet hitherto undetected between *Mercury* and the sun. In this case, inasmuch as no discordance of similar amount of the motion of the node of the orbit of *Mercury* is found to exist, the unseen planet must be assumed to have its orbit but little inclined to that of *Mercury*. There will be conditions connecting its supposed mass and distance from the sun, similar to those which it was necessary to set out with in the research after *Neptune;* it being necessary, in order to produce the desired inequality, to increase the mass the less the distance from sun of the hypothetical planet is taken, and *vice versâ;* and for a distance a little less than half that of *Mercury*, the sought mass will require to be equal to that of *Mercury*—a very admissible assumption.

But if the improbability of the existence of such a planet undetected, with its necessary brilliancy, be urged, M. Le Verrier remarks that it may be replaced with equal success as an explanatory hypothesis by a series of corpuscles circulating round the sun interior to *Mercury*, whose united action would be equivalent in perturbating effect, while their individual minuteness might explain their non-detection.

M. Le Verrier asks, "May not some of the larger of these corpuscles, in their occasional transits over the solar disk, yet be detected? Astronomers, already so attentive to the phenomena exhibited on the disk, will doubtless, in these reflections of mine, find a new motive for carefully examining the motions of the smallest and least-defined spots."

The ideas of Professor W. Thomson, of Glasgow, respecting a zone of meteors in the neighbourhood of the sun, thrown out as a possible source of supply of the solar heat and energy, in his paper "On the Mechanical Energies of the Solar System," published in the *Philosophical Magazine* for December, 1854, may be read in connexion with the above.

In a letter to Mr. Warren De La Rue Dr. Steinheil states that he is progressing favourably with the 13-inch glass mirror he undertook to make for him, and that in the meantime he has completed a 12-inch mirror, which is quite perfect in its definition. With parallel rays the foci of the several zones do not differ more than $\frac{1}{50}$th of a line. Dr. Steinheil's price for a 12-inch glass reflector with diagonal mirror and eye-piece is 1000 florins.

For disposal, a 5-foot telescope, 4-inch object-glass, by Dollond, with eight astronomical eye-pieces, diagonal reflector, Barlow lens, and day eye-piece, spider-line micrometer, and illuminating apparatus, with long mahogany polar axis, circles of 15 inches, counterpoise, &c. &c. There is also a mahogany tripod-stand and cradle, with steadying rods. This telescope was formerly the property of Admiral Sheriff, and afterwards was mounted in Mr. Drew's observatory, Southampton.

For further particulars, apply to Mr. Williams, Assistant Secretary, Somerset House.

The planet discovered by Dr. Luther on the 22d of September has been called *Mnemosyne*. The following elements of its orbit, extracted from No. 1223 of the *Astronomische Nachrichten*, have been calculated by M. Tiele:—

Epoch 1860, Jan. 1·0.

M	.....	335° 26′ 7″·0	
☊	.....	200 8 58·7	Mean Equinox of the Epoch.
π	.....	53 24 35·7	
i	.....	15 4 3·9	
φ	.....	6 5 32·8	
μ	.....	633″·0918	
Log *a*	.....	0·4990266	

These elements are founded on the Berlin observations of Sept. 25, Oct. 11, and Nov. 2, executed by Dr. Förster. M. Tiele computed from them an ephemeris of the planet for the month of November.

RECENT PUBLICATIONS.

Places of 5345 *Stars, observed from* 1828 *to* 1854, *at the Armagh Observatory*. By Rev. T. R. Robinson, D.D. F.R.S., &c. &c. Printed at the expense of Her Majesty's Government, on the recommendation of the Royal Society. 8vo, pp. 847, Dublin, 1859.

In an Introduction extending to 67 pages Dr. Robinson has given a description of the instruments and methods of observation employed by him, with accompanying remarks suggested by his long experience as an observer, which cannot fail to be of great value to the practical astronomer. The Armagh Observatory was founded, in 1793, by Primate Robinson, but it continued to be inadequately provided with instruments, and little work was consequently performed, until the present Archbishop of Armagh, Lord John George Beresford, remedied this defect by his gift of a transit instrument, a mural circle, and a reflector of 15 inches aperture, equatoreally mounted. The first of these instruments was erected in 1827, and the last in 1835.

The Observatory is situated on a hill in the immediate vicinity of Armagh, about 100 feet above the general level of the district. In the *Philosophical Magazine* for August, 1846, Dr. Robinson has called attention to a periodical motion of this hill, as indicated by variations of the transit instrument in level, and of the index correction of the circle, and he has given an explanation of its origin. The existence of such a variation, whatever be its cause, combined with the fact of

similar variations having been subsequently established at other observatories, and more especially the liability of such variations to sudden change, appear to Dr. Robinson "to furnish a strong argument against any system of observing which assumes an invariable position of the instruments for any considerable period." He accordingly considers it absolutely necessary to verify the principal corrections within a few hours of the time of observation.

The author remarks that the tremors caused by the traffic on the Ulster Railroad, the nearest part of which to the Observatory is about 700 yards distant, has been found sensibly to affect the observations.* The most important disturbance arising from this cause is "the motion of the circle if a train passes *after* the bisection, but *before* the microscopes are read." This appears to the author to arise from the friction at the pivots being released, in consequence of the jar caused by the vibration of the ground. The disturbance here referred to has been known to amount to as much as 4″. The author accordingly was in the habit of rejecting all observations made under such circumstances.

Latitude.—This was determined, first, by direct and reflected observations of stars; secondly, by the reflection of the N.P.D. wire of the circle; thirdly, by the vertical collimator of the late Captain Kater. The first of these methods gave 125° 38′ 47″·317 for the horizontal point; the second method gave 128° 38′ 47″·151. The third method is referred to by the author merely from his wish to put on record his experience of the instrument used, and to point out what he conceives to be its defects. Forty-seven sets of observations made from 1835 to 1839 gave 47″·158. The close accordance of this result with that furnished by the second method is very remarkable, considering that the probable error of a set of observations amounts to ±1″·044, being three times as great as by the second method, and that two of the observations differ no less than 5″·76. The author cites this fact as a striking proof of the precision of a mean *when the errors are casual.* After some further interesting details, he concludes this part of his subject with the remark that he does not feel disposed to change the value of the latitude communicated by him some years ago to the *Nautical Almanac*, viz. 54° 21′ 12″·70.

Longitude.—This was determined: 1st, by chronometers; 2d, by occultations; 3d, by moon-culminating stars. In 1838, Mr. Edward Dent transported fifteen of his best chronometers from Greenwich to the Dublin and Armagh Observatories. The mean of thirty comparisons, going and returning, gave −26ᵐ 35ˢ·44 for the longitude of Armagh.† Eight com-

* The author here refers to a paper by him on this subject, which is published in vol. v. of the *Proceedings* of the Royal Irish Academy.

† See a paper by the author on the subject published in vol. xix. of the *Transactions of the Royal Irish Academy.*

parisons of the author's own chronometer with Greenwich gave also 35ˢ·44. The difference of longitude between Armagh and Dublin was determined by Sir W. Hamilton and the author, by means of rocket signals, and found to be — 1ᵐ 14ˢ·26. This result, combined with the difference of longitude between Greenwich and Dublin, as indicated by Dent's chronometers, viz. — 25ᵐ 21ˢ·08, gave — 26ᵐ 35ˢ·34 for the longitude of Armagh. The investigation of the longitude by the second method was founded on five phases of the solar eclipses of 1826 and 1836, and twenty-two occultations of stars. The definitive value of the longitude thus deduced is — 26ᵐ 35ˢ·47. The method of moon-culminating stars was in high repute when the author commenced his observations, and he at first gave it much attention; but he soon found that, however useful it might be to a traveller, it is destitute of the precision requisite for a fixed observatory. He, on the whole, assumes as the true value of the longitude, until new determinations are obtained by electric signals, the mean result of the chronometers and occultations, viz.—

— 26ᵐ 35ˢ·74

The Transit Instrument.—This instrument was constructed by Thomas Jones, and was erected in 1827. It has an object-glass of 3ⁱⁿ·8 aperture and a focal length of 63 inches. The author has given valuable details respecting his investigation of the qualities of the different parts of this instrument, especially the pivots, and his mode of observing with it.

Corrections of the Transit.— The existence of the periodic changes of the transit already referred to, and others of a more irregular character, induced the author to reject the system of leaving an instrument out of perfect adjustment, and applying to the observations the necessary corrections, as determined by observations of standard stars. This process, he adds, assumes the permanence of the instrument to an extent which he cannot admit. He, therefore, either kept the transit perfectly adjusted, or determined the error within a few hours of the observations which it affected.

Clock.— The clock is by Earnshaw. The author speaks favourably of its qualities. It was originally furnished with a gridiron pendulum, which was replaced in 1830 by a mercurial pendulum, the work of Mr. Sharp, sen., of Dublin. On the 5th of July, 1832, the author applied the compensation barometers, described by him in a paper published in vol. v. of the *Memoirs* of this Society (*On the dependence of a clock's rate on the height of the barometer*). He has given some interesting tables exhibiting the rate of the clock for each of the three different sorts of pendulum.

Mural Circle.— This instrument was also constructed by Jones. Its diameter is 56 inches, and its telescope a duplicate of the transit. The author refers to vol. ix. of the *Memoirs* of this Society for a detailed description of it, and in his remarks

on the errors of the instrument he refers to another paper by him "On Correcting Errors of the Astronomical Circle" (*Trans. R. I. A.* vol. xv.), wherein he investigates the effect of equidistant readings in correcting ellipticity, the deformation of a circle by its own weight, and the change of figure due to unequal temperature or irregular strength.

Meteorological Instruments. — The thermometer is by Troughton. The barometer is a standard by Newman, $0^{in}\cdot57$ in bore, similar to that of the Royal Society.

Observed Places. — To have published the entire observations in detail would have swelled the work to an enormous and useless extent. The author has, therefore, only given the result of each observation, as the correction of an assumed place, and the mean place deduced from the individual results. On receiving the instruments, he resolved to devote them to a re-observation of Bradley's stars. On the whole, the stars of the *Fundamenta* are tolerably represented. The author's observations in right ascension contain 2703 out of 2933; those in north polar distance, 2562 out of 2954. As Bradley's stars were disposed of, others were selected from Piazzi or Groombridge to employ the vacant time. It was the author's intention that every star should be observed at least five times; but he was unable to carry this plan generally into effect.

With very few exceptions the transit observations till March 1837 were made by the author, and afterwards by his first assistant, Mr. Neil M'Neil Edmondson, to whose accuracy as an observer and computer, and devotion to his duty, the author bears the highest testimony. The circle observations were made by the author till 1849, and subsequently by his second assistant, Mr. W. Hautenville Rambaut.

The Catalogue. — The epoch is 1840·0. The precessions are those of the British Association Catalogue, when the star is found in it, reduced to 1840. For stars not found in that Catalogue the precessions are computed by the same constants.

Proper Motion. — The author does not consider it necessary to discuss the general question of proper motion, which has recently been so thoroughly done by Mr. Main and Mr. Airy. He has, however, given in the Introduction an interesting list of proper motions, as determined by a comparison of his own results with those of Bradley, Piazzi, Lalande, and Groombridge. He is of opinion that differences of $0^s\cdot5$ and $5''\cdot0$ imply a real change of place, even when they result from a single observation. He has, therefore, taken these as limits, and whenever the correction of any Catalogue equals or exceeds them, he has considered the quotient of it by the number of years between the epoch of the Catalogue of comparison and the mean date of his own observations to be the proper motion.

The author concludes his Introduction by remarking that many small stars have large proper motions. He considers the

most probable explanation of this circumstance to be, that they are nearer to us than other stars which, although brighter, have less apparent motion. He cites, in support of this opinion, the case of many physically double stars, the constituent members of which, although unequally bright, are known to be situate nearly at the same distance. The great reflectors of Lord Rosse have also established the existence of a similar difference of brightness in the stars of which clusters are composed.

The Introduction is followed by the individual observations, conveniently arranged in double columns. For each star the division error of the circle is given, with an appropriate symbol indicating whether the observation is one above pole, below pole, or by reflection. This part of the volume occupies 646 pages.

The volume closes with the Catalogue. The right ascensions are given on the left-hand page, and the north polar distances on the opposite page. The numbers on the right-ascension page are arranged in ten columns. The first column contains the number of the star in the Catalogue; the second, its usual designation in those cases wherein it is not anonymous; the third, its magnitude; the fourth, its right ascension; the fifth, the amount of annual precession; the sixth, the secular variation of precession; the next three columns exhibit the differences between the right ascension given by the author and the corresponding results of Bradley, Piazzi, or some other observer. The last column contains the number of individual observations. The numbers on the opposite page of the Catalogue, relating to the north polar distances, are drawn up in only eight columns, the repetition of the name and magnitude of the star being obviously unnecessary. In other respects the arrangement is the same as for the right ascensions.

We shall conclude with expressing an assurance that the readers of the *Monthly Notices* will cordially congratulate the author on having brought his arduous and important labours to so satisfactory a close.

The U.S. Naval Astronomical Expedition to the Southern Hemisphere, during the years 1849-50-51-52. Vol. III., *Observations to determine the Solar Parallax.* By Lieut. J. M. Gillis, LL.D., Superintendent. Washington, 1856.*

Allusion has frequently been made in the *Monthly Notices* to the expedition despatched to Chili by the American Government for the purpose of determining the solar parallax, by observations

* This volume, although bearing the date 1856, was published only in the year 1858.

of *Venus* and *Mars*, in accordance with a method proposed by Dr. Gerling of Marburg in No. 502 of the *Astronomische Nachrichten*. The merit of suggesting this expedition is due to Lieut. Gillis, who offered to execute, at some place in Chili, the observations which would be required, in conjunction with corresponding observations made at other places in both hemispheres, for carrying Dr. Gerling's method into effect. Lieut. Gillis, who was already favourably known as an observer, was accordingly appointed Superintendent of the operations of the Expedition, which sailed from the United States in the autumn of 1849. A temporary observatory was erected at Santiago. The instruments were: an equatoreally mounted refractor of 6·4 inches aperture and 103·7 inches focal length, by Fitz of New York; and a meridian circle reading to half seconds of arc by four micrometer microscopes, and furnished with a telescope of 51 lines aperture and 73 inches focal length. The Expedition returned to the United States towards the close of 1852 after an absence of thirty-nine months.

The first two volumes of the work, containing an account of the operations of this expedition, so creditable to the enlightened enterprise of the Government of the United States, is devoted to the geography, climate, natural history, &c., of Chili. The present volume contains a complete account of the observations for determining the solar parallax, which formed the main object of the expedition, including the subsequent discussion of them by Dr. Gould, and a statement of the definitive results deducible from them.

The introduction to the contents of the volume commences with an account of the origin and progress of the expedition by Lieut. Gillis. This is followed by a description of the instruments and methods of observation. The next subject is the discussion, by Dr. B. A. Gould, of all the observations available for determining the value of the solar parallax. The author of this part of the work has prefaced his labours by a valuable historical sketch of the subject. He then proceeds to a general statement of the problem. The observations of *Mars* and *Venus* at the Santiago Observatory consist of four series of micrometric measures executed on 217 days, including two oppositions of the former planet, and two inferior conjunctions of the latter, besides a large number of corresponding meridian observations of both planets. Dr. Gould regrets that the number of corresponding micrometric comparisons made at the observatories in the northern hemisphere proved totally inadequate to admit of an investigation of the solar parallax conformably to the original plan of procedure. Under these circumstances he considered that "the only course remaining for him was to make use of the largest possible number of observations of every kind in each hemisphere, and to trust to the accumulation of observations for eliminating the inaccuracies due to erroneous assumptions in the adopted positions of the comparison-stars, and to

the unavoidably large mean error of the meridian declinations." He accordingly has given a complete discussion of the subject, adapted to the data available to him. The entire investigation and calculations occupy 228 pages of the Introduction. We have only room for a statement of the final results, which consist of new values of the semi-diameters of *Mars* and *Venus*, and of the solar parallax. They are, for the semi-diameters of the two planets,

$$\text{Semi-diameter of Mars} = 4''\cdot6639 + 1''\cdot9681 = 6''\cdot63$$
$$\text{Semi-diameter of Venus} = 8''\cdot6625 - 0''\cdot3118 = 8''\cdot35$$

These results are derived exclusively from observations made at Santiago and Washington.

The value of the solar parallax is derived solely from the series of observations of *Mars* which include the first opposition of the planet. It is found to be

$$= 8''\cdot5712 - 0''\cdot0762 = 8''\cdot4950$$

The author accordingly concludes that we may assume with advantage

$$\text{The sun's horizontal parallax} = 8''\cdot5000,$$

being less by $0''\cdot07$ than the value commonly adopted, and corresponding to a distance of 24,266 terrestrial semi-diameters, 96,160,000 statute miles, 15,475,000 myriameters.

The investigation of the solar parallax by Dr. Gould closes the introductory part of the volume, which comprises 288 pages. The remainder of the volume is occupied with a detailed statement of the observations made at Santiago by Lieut. Gilliss, and the corresponding observations made by Lieut. Maury at the Washington Observatory, by Mr. Bond at the Harvard College Observatory, and by Mr. Maclear at the Royal Observatory, Cape of Good Hope.

The *Nautical Almanac* for 1863 has recently been published. The contents and arrangement are generally the same as for preceding years. Accompanying it is a Supplement containing ephemerides of the minor planets and of *Neptune* for the year 1860.

Annales de l'Observatoire Royal de Bruxelles. Par le Directeur A. Quetelet. Tome xiv. 4to. Bruxelles, 1859.

In the Introduction to this volume M. Quetelet has given a brief sketch of the history of the Brussels Observatory since its

establishment in 1835. The astronomical operations, which commenced with measures of double stars, were interrupted in the year 1840, by the necessity of attending to the phenomena of terrestrial magnetism and other points relating to the physical constitution of the earth, which it was then proposed to study simultaneously at the principal observatories throughout Europe. In 1848, the astronomical observations were resumed, and have been regularly prosecuted down to the present time. Those for the years 1848–9 have been reduced and published in tome xii. of the *Annales*. The observations for the succeeding five years have not yet been reduced. It is intended that they shall be included in tome xiii., which, consequently, has not yet been published.

The present volume contains observations with the transit instrument and the mural circle, made in the years 1855–6. They consist chiefly of observations of stars, but there are also transit observations of the sun, moon, and principal planets, and also of some of the minor planets. From the star-observations of each year a provisional catalogue is formed, the epoch of reduction being the beginning of the year. The number of stars in each catalogue is upwards of a thousand. It is intended, as soon as the nine annual catalogues subsequent to 1848 have been completed, to construct from the totality of the results a single catalogue reduced to some common epoch. The volume concludes with an account of the meteorological and magnetical observations executed at the observatory during the years 1855–6.

Bericht über die Cometen von den Jahren 975, 1264, *und* 1556. Von P. Augustin Reslhuber, Director der Sternewarte zu Kremsmünster. Linz, 1857.

In this *brochure* the author briefly notices the researches of Dunthorne, Hind, Hoek, and others, who have investigated the subject of the identity of these famous comets. According to Hoek, to whom the most recent examination of the question is due, there exists very little probability of the reapparition of the comet of 1556 in our own day.

Tables of Victoria computed with regard to the Perturbations of Jupiter and Saturn. By F. Brünnow, Ph. Dr., Professor of Astronomy in the University of Michigan, and Director of the Observatory at Ann Arbor, 4to, New York, 1859.

Dr. Brünnow has devoted much attention to the improvement of the theory of this planet, and at length, when the

proper time appeared to him to have arrived, has undertaken the laborious task of computing tables of its motion. In No. 1077 of the *Astronomische Nachrichten* he has given the expressions for the perturbations of the planet produced by *Jupiter* and *Saturn*, together with the elliptical elements as deduced from five oppositions of the planet in 1850, 1852, 1853, 1854, and 1856. Subsequent corrections of the elements, deduced from the opposition of 1857, were found to be of trifling magnitude.

The perturbations were calculated from the formulæ published by Professor Hansen in No. 799 of the *Astronomische Nachrichten*. The tables give, therefore, the perturbations of the mean anomaly, those of the logarithm of the elliptic radius vector, and those of the perpendicular to the plane of the orbit. Subsidiary tables are given for computing the heliocentric rectangular co-ordinates referred to the equator, and the mean equinox of 1857·0.

Dr. Brünnow, by his self-imposed labours in the construction of tables of *Flora* and *Victoria*, has earned the unqualified thanks of all astronomers. It is to be hoped that his example will be speedily followed by others of his contemporaries, and that ere long the computation of ephemerides of the minor planets, or, at any rate, of those most favourable for the purpose, will be effected by the aid of tables constructed beforehand.

Observations of Donati's Comet made at the Observatory of Edward J. Cooper, Esq., with the large Equatoreal.

This pamphlet, which has been printed for private circulation, among astronomers, contains differential measures for determining the position of the comet, executed by Mr. Graham at the Markree Observatory, and extending from September 18, 1858, to October 16 in the same year. Appended to these are a series of physical observations of the comet by Mr. Cooper and Mr. Graham. There are some well-executed drawings, illustrative of the text, which may be advantageously compared with the delineations of the same sort made at other observatories.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *December* 9, 1859. No. 2.

The Rev. R. MAIN, President, in the Chair.

The Rev. Percival Alfred Fothergill, B.A.,

was balloted for and duly elected a Fellow of the Society.

Unexpected Reappearance of U *Geminorum.* By Norman Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

M. Hermann Goldschmidt, in a letter of the 30th ultimo, announces the premature apparition of this singular star, which must have passed its maximum on Nov. 12th, full 27 days before the time indicated by computation. The following extract from his interesting and important communication will show how zealously he *toils* at his celestial recreations:—

"On the evening of Nov. 18th, I looked for it, and it was brighter than a star I estimated 10·11 magnitude, but to which Dr. Winnecke assigned (*h*) 10·3 magnitude. On the next evening I was surprised to find it less bright, and nearly equal to this star. I was obliged to compare with the list of stars given by Dr. Winnecke, for I want the *Monthly Notices* in which you gave the magnitudes of your comparison stars. On the 20th, 10 hours, it was equal to the stars *d* and *e*. On the 21st, it was on the extreme visibility of my four inches: it even diminished one hour after. On the 22d, *it was painfully seen;* 0·1 magnitude less bright than the small star *b*. On the 23d it had quite vanished, and since my instrument shows 12·4 magnitude, after your scale, 12·5 can be obtained by favourable

circumstances. I must say that I was constantly for two hours and more in comparing, and favoured by a beautiful sky. I made use of eye-pieces of 36 and 80 magnifying power. At instants I could see the small stars next the variable; *a* and *b* in Dr. Winnecke's list. You may safely trust these observations *laboriously obtained*. I send a diagram of the comparison stars to avoid all mistake. The scale of Dr. Winnecke is in the mean 0·3 magnitude higher than my estimations."

Now, applying these differences to my own standards (*Monthly Notices*, vol. xvii. p. 202),* for comparison with previous observations, we get, for about 11 hours each evening,—

1859.				
Nov. 18	U Geminorum	0·3 mag. brighter than *e*	= 10·7	magnitude.
19	,,	0·1 mag. less than *e*	= 11·1	,,
20	,,	Equal to *g*	12·1	,,
21	,,	A limit of favourable vision	12·5	,,
22	,,	0·1 mag. less than *l*	13·1	,,
23	,,	Invisible	Under 12·5	,,

Well might M. Goldschmidt say of his comparison on the 22d, that "the star was painfully seen," for I estimate *l* (Winnecke's *b*) 13·0, Mr. Baxendell 13·3; and besides his "two hours and more in comparing, favoured by a beautiful sky," he must possess an exquisite eye and telescope (by Steinheil), to peer so far beyond the limit of his aperture as to have glimpsed it at all! But some of his extremely faint planetary discoveries have proved this long since. I can only say that, ordinarily, the star *l* is the limit of the Hartwell equatoreal, and that, with a larger but inferior telescope at Oxford I never saw it at all!

Knowing that U *Geminorum* was not due till Dec. 9th, and being anxious to push forward other work, I am only able to add to my friend's valuable observations one useful record, viz., that U *Geminorum* was invisible and under 13·0 magnitude on Nov. 7th. Now, whatever other anomalies have been displayed by this truly wonderful star, it has hitherto kept in pretty close conformity with a uniform period. My latest elements of variation, viz., Epoch = 1859, Feb. 19·6; Period = 96·879 days; furnish for the six observed maxima, from which they were deduced, the residual errors (Observation—Calculation), +1·4; +2·5; −2·5; +7·3; +1·2, and +0·4 days respectively, the equation, 7·3 days out, having but small weight in regard to the others; but here is an irregularity of 27 days at once, more than a fourth of the entire period! For the future it must be more closely watched.

* Comparison star *h* is certainly under-estimated in my list, and must therefore be rejected.

Hartwell Observatory, Dec. 8, 1859.

On the Variability of α Cassiopeiæ. By W. R. Birt, Esq.

Some doubts having been expressed relative to the variability of *α Cassiopeiæ*, which I had the honour to discover in the year 1831, have induced me to give particular attention to the star during the preceding summer and autumn, the result of a very careful series of *eye-comparisons* fully confirming my previous conclusions as to the fact of the varying intensity of its light, although within narrow limits.

Sir John Herschel's determination of the magnitudes of γ, α, and β, in the table appended to his Cape Observations are as follows:—

γ 2·52, α 2·57, β 2·63;

In the course of my observations, extending from March 31 to December 1, 1859, I have seen α brighter than γ, and I have also observed it less than β, the most usual estimation of its magnitude being 2·57, that assigned to it by Sir John, this places it nearly midway between γ and β.

It is not my intention, in the *present communication*, either to attempt anything like a re-determination of the period, or to compare any of my observations during the past summer and autumn with those that I made in 1831, or others that are on record by various observers since, my object being to point out for the information of astronomers engaged with variable stars the epochs when I observed the star either brighter than the γ or less than the β. I have, however, attempted in the following tables to exhibit the *great irregularity* of the phases of brilliancy, indicating at the same time, in Table III., the progression from max. to min. and the reverse; the epochs of the *true* maxima, as well as brilliancy, as shown by a curve in most cases differing considerably from those greater brightnesses of the star which appear to be connected with a very remarkable feature which has been presented in the course of these observations: it is the alternation of brightness which the star has exhibited, even in the course of a single evening, nearly the same irregularity appertains to this feature as well as to the increase and decrease of brilliancy. The epochs of the principal instances are as under:—

1859, June 24 to July 7, August 19 to Sept. 1, Nov. 19 to 28.

On these occasions the light of the star has apparently been in a state of great fluctuation, nothing at all settled; one evening bright, the other dim; sometimes settled for a few hours, then suddenly brightening up or decreasing in its brilliancy; while on other occasions the course of the observations has been such as to induce the belief that for some little time, at least, the star has been steady at its brilliancy, very gradually increasing or

decreasing in brightness. During the alternations above alluded to, the γ and β have not appeared to participate, indicating that the alternations have rather occurred in connexion with the star itself than have been produced by fluctuations in our own atmosphere. On one or two occasions, but very rarely indeed, a suspicion has crossed my mind that both γ and β have exhibited a slight change of lustre, but nothing at all in any way comparable with the variations of α, the extent of which I estimate to be in accordance with Sir John Herschel's scale, about 0·3 of a magnitude.

Table I.

Epochs of greater brilliancy, the star nearly as bright or brighter than the γ.

1859.	G.M.T. d h m	Mag.	Interval days.	Remarks.
May	5 13 0	2·53		Sky very favourable.
	5 14 0		49·84	
June	24 10 0	2·50		Strong twilight.
	24 10 15		5·00	
June	29 10 15	2·45		Strong twilight.
			15·98	
July	15 9 45	2·50		Very clear, moon present.
			5·01	
July	20 10 0	2·47		Clear, moon present.
			29·95	
Aug.	19 9 0	2·52		Moon low.
			11·00	
Aug.	30 9 0	2·52		
			22·02	
Sept.	21 9 20	2·50		

Table II.

Epochs of minima or inferior brightness, the star generally less than β.

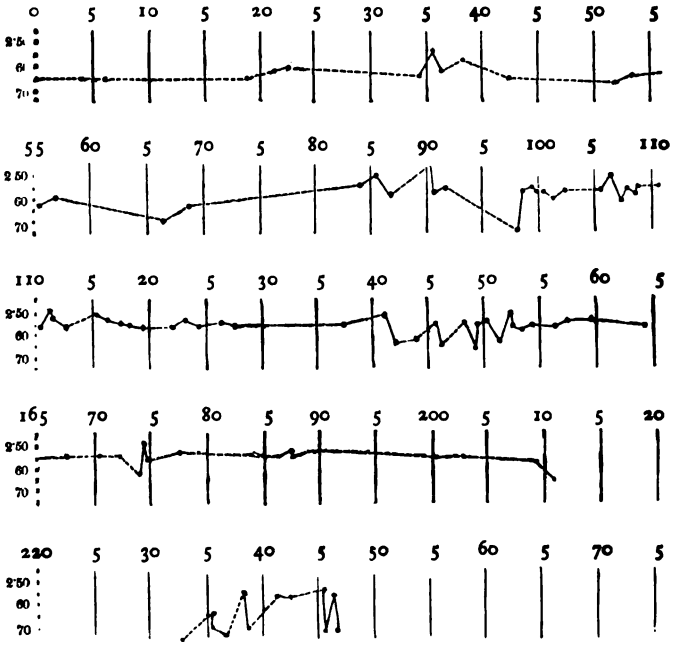
1859.	G.M.T. d h m	Mag.	Interval days.	Remarks.
April	19 10 15	2·65		Very clear.
			32·98	
May	22 9 45	2·65		Fine strong twilight.
			14·11	
June	5 12 30	2·70		Clear.
			31·92	
July	7 10 40	2·74		Moon just set.
			43·99	
Aug.	20 10 20	2·65		
			4·10	
Aug.	24 12 45	2·63		Clear, Milky way bright.
			2·84	
Aug.	27 9 0	2·65		
			25·07	
Sept.	21 10 40	2·65		
			36·02	
Oct.	27 11 0	2·67		
			22·83	
Nov.	19 7 0	2·74		

Table III.

Maxima and minima of α Cassiopeiæ, the maxima determined by the projection of a curve with intervals of increase and decrease.

1859.	Epoch of Min. d h m	Mag.	Interval days.	Epoch of Max. d h m	Mag.	Interval days.*
April	19 10 15	2·65	16·13	May 5 13 30	2·53	16·85
May	22 9 45	2·65	4·10	May 26 12 15	2·60	10·01
June	5 12 30	2·70	23·91	June 29 10 15	2·45	8·01
July	7 10 40	2·74	12·98	July 20 10 0	2·47	35·11
Aug.	24 12 45	2·65	18·08	Sept. 11 14 40	2·57	9·83
Sept.	21 10 40	2·65	14·83	Oct. 6 6 30	2·55	21·19
Oct.	27 11 0	2·67		No observation.		

Curve of the variation of the brightness of α Cassiopeiæ from March 31 to Dec. 2, 1859.



0 = March 31, 1859. Brightness of γ Cass. = 2·52, of β Cass. 2·63.

* Intervals between the maxima and *succeeding* minima.

Ephemeris of the Variable Stars for 1860. By Norman Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

Star.	Probable Mag.	Mean Place, 1860. R.A	N.P.D.	Times of Maxima.	Authority.
		h m s	o ′		
T Piscium	9·5	0 24 46	76 14	May 7, September 27 ...	S.
α Cassiopeiæ	2·0	0 32 35	34 14	Jan. 19, April 8, June 26, Sept. 13, Nov. 31	A.
S Piscium	9·0	1 10 15	81 49	June?	—
R Piscium	7·5	1 23 25	87 51	Early in June	—
R Arietis	8·0	2 8 10	65 36	January 16, July 21	B.
o Ceti	2·0	2 12 17	93 37	September 6·5	A.
R Tauri	8·0	4 20 38	80 9	July 23	W.
S Tauri	10·0	4 21 32	80 22	February 17	W.
R Orionis	9·0	4 51 22	82 5	May 26	W.
α Orionis	1·0	5 47 36	82 37	June, December	—
R Geminorum	7·3	6 58 56	67 5	January 15	P.
R Can. Min.	8·0	7 1 0	79 46	April?	—
S Can. Min.	7·5	7 25 7	81 23	August 22	W.
S Geminorum	9·2	7 34 38	66 13	March 15, and January 3, 1861	P.
T Geminorum	8·9	7 40 54	65 55	October 13	P.
U Geminorum	9·1	7 46 48	67 38	March 13, June 18, September 23, December 29	P.
R Cancri	6·0	8 8 51	77 51	April 8	A.
U Cancri	9·0	8 27 45	70 37	October 29	W.
S Hydræ	8·5	8 46 16	86 24	August 26; not observable ...	W.
T Cancri	9·5	8 48 40	69 37	Uncertain, Minimum (12 mag.), July 1; not observable ...	W.
T Hydræ	6·5	8 48 51	98 37	July 18	W.
R Leonis	5·0	9 40 2	77 55	Feb. 12, and Jan. 2, 1861 (Minimum, Aug. 18; 10 mag. ...	P.
R Urs. Maj.	7·0	10 34 41	20 29	September 2 (Minimum, May 12)	P.
R Comæ	8·0	11 57 4	70 26	October?	W.
R Virginis	6·5	12 31 24	82 14	January 5·6, May 29·9, Oct. 22·2	A.
S Urs. Maj.	7·5	12 37 48	28 8	June 2 (Minima, Feb. 25, Oct. 7)	P.
U Virginis	7·5	12 44 0	83 41	April 8, November 6 ...	W.
V Virginis	7·0	13 20 36	92 28	Possibly in March and November	P.
R Hydræ	4·0	13 22 4	112 33	August 31, irregular ...	B.
S Virginis	6·0	13 25 42	96 28	April 25 (Minimum; 11 mag.; Feb. 21)	P.

Star.	Probable Mag.	Mean Place, 1860. R.A. h m s	N.P.D. ° ′	Times of Maxima.	Authority.
R Boötis	8·0	14 31 1	62 39	May 3, December 22 ...	W.
S Serpentis	8·0	15 15 7	75 11?	April 10	A.
R Coronæ	6·2	15 42 49	61 25	October 15; very irregular ...	B.
R Serpentis	6·5	15 44 15	74 26	September 10	A.
R Herculis	8·5	15 59 38	71 15	April 7	B.
S Scorpii	9·5	16 9 17	112 33	{April; maximum brilliancy very uncertain ... ...}	P.
R Scorpii	9·0	16 9 19	112 36	July; uncertain	P.
S Ophiuchi	9·3	16 26 12	106 52	January 19, September 7 ...	P.
S Herculis	7·5	16 45 32	74 49	March 16	B.
R Ophiuchi	7·6	16 59 44	105 54	January 4, October 31 ...	P.
α Herculis	3·0	17 8 16	75 27	March 11, May 16, July 22, Sept. 26	A.
T Herculis	7·9	18 3 48	59 0	January 6, June 14, November 21	K.
R Scuti	5·0	18 40 1	95 50	April 2, June 14, Aug. 24, Nov. 3	A.
13 Lyræ	4·3	18 51 4	46 14	{Jan. 19, March 6, April 21, June 7, July 23, Sept. 8, Oct. 24, Dec. 10}	B.
R Aquilæ	6·5	18 59 38	81 59	June 8	W.
R Sagittarii	8·2	19 8 28	109 33	Not until February, 1861 ...	P.
R Cygni	8·0	19 33 4	40 7	August 3; Minimum (14·0) Mar. 1	P.
χ Cygni	5·0	19 45 11	57 27	March 10	A.
U Capricorni	10·5	20 40 22	105 18	Not until February, 1861 ...	P.
R Vulpeculæ	8·0	20 58 10	66 44	February 15, July 11, December 5	W.
T Capricorni	9·0	21 14 17?	105 45	April 25	S.
β Pegasi	2·0	22 57 0	62 41	{Jan. 19, April 14, May 28, July 10, Aug. 23, Oct. 5, Nov. 17, Dec. 31}	Au.
R Pegasi	8·5	22 59 37	80 14	April 2	A.
R Aquarii	7·0	23 37 15	106 3	June 24	A.
R Cassiopeiæ	6·0	23 51 18	39 23	June 25; Minimum (13 mag.) Jan. 9.	P.

Minima of the Short Period Variable Stars during 1860 *in G.M.T. and for the Earth's Mean Distance from each Star.*

λ *Tauri.*

Max. 4·0; Min. 4·5. Variation rapid.

	d h m		d h m		d h m		d h m
Jan.	31 13 8	Feb.	16 8 37	Oct.	6 13 56	Oct.	22 9 25
Feb.	4 12 0		20 7 29		10 12 48		26 8 17
	8 10 52	Sept.	28 16 12		14 11 41	Dec.	24 15 20
	12 9 44	Oct.	2 15 4		18 10 33		28 14 12

Algol.

Maximum 2·3; Minimum 4·0. Whole variation completed in seven hours.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	2	16	35	Mar.	11	12	8	July	18	12	47	Nov.	1	14	55
	5	13	24		14	8	57		21	9	36		4	11	44
	8	10	13		31	13	50	Aug.	7	14	29		7	8	33
	11	7	2	April	3	10	39		10	11	18		10	5	22
	22	18	17		20	15	32		30	13	0		21	16	37
	25	15	6		23	12	21	Sept.	2	9	49		24	13	26
	28	11	55		26	9	10		19	14	42		27	10	15
	31	8	44	May	13	14	3		22	11	31		30	7	4
Feb.	14	16	48		16	10	52		25	8	20	Dec.	11	18	19
	17	13	37	June	5	12	34	Oct.	9	16	24		14	15	8
	20	10	26		8	9	23		12	13	13		17	11	57
	23	7	15		11	14	16		15	10	2		20	8	46
Mar.	8	15	19		28	11	5		18	6	51		23	5	35

ζ Geminorum.

Max. 3·8; Min. 4·5. Increasing $5^d\,6^h$.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	30	5	48	Feb.	19	13	24	April	20	12	12	Dec.	20	7	24
Feb.	9	9	36	April	10	8	24	Oct.	30	12	24		30	11	12

S *Cancri.*

R.A. $8^h\,35^m\,56^s$; N.P.D. 70° 28′.

Max. 8·0; Min. 10·5. Variation rapid.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	2	13	5	Feb.	28	10	46	Sept.	14	14	41	Nov.	10	12	23
	21	12	18	Mar.	18	10	0	Oct.	3	13	55		29	11	37
Feb.	9	11	32	April	6	9	14		22	13	9	Dec.	18	10	51

β Lyræ.

Principal Minimum (4·3); First Max. (3·4), $3^d\,2^h$ after.
Second Min. (3·8), $6^d\,9^h$ after.; Second Max. (3·4), $9^d\,10^h$ after.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	26	5	11	May	8	11	12	Sept.	14	12	44	Oct.	23	6	0
Apr.	12	15	42		21	8	58		27	10	30	Dec.	26	18	46
	25	13	27	Sept.	1	14	59	Oct.	10	8	15				

η Aquilæ.

Max. 3·6; Min. 4·4. Increasing $2^h\,9^d$.

	d	h	m		d	h	m		d	h	m		d	h	m
Feb.	16	18	9	June	10	13	55	Aug.	21	8	15	Nov.	8	6	50
Mar.	23	15	20	July	16	11	5		28	12	29	Dec.	14	4	
Apr.	28	12	30		23	15	19	Oct.	3	9	40				

δ Cephei.

Max. 3·7; Min. 4·8. Increasing $1^d\ 14^h$.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	4	14	33	Mar.	30	11	15	July	26	12	44	Oct.	20	9	27
	15	8	8	April	15	13	38	Aug.	11	15	7	Nov.	5	11	50
	20	16	56	May	1	16	1		22	8	42		16	5	25
	31	19	31		12	9	37	Sept.	7	11	5		21	14	13
Feb.	16	12	54		28	12	0		23	13	28	Dec.	2	7	48
Mar.	3	15	17	June	13	14	23	Oct.	4	7	4		7	16	36
	14	8	52	July	10	10	21		9	15	51		18	10	11

The minima of all the short-period variable stars, except λ *Tauri*, are computed from Prof. Argelander's elements. The corrections to be applied to the observed times, before comparing with the ephemeris, may be found by the formulæ given in the *Monthly Notices*, vol. xvii. p. 27.

The initials in the last column of the first table indicate the authorities for the elements of variation employed, viz. Argelander, Auvers, Baxendell, Krüger, Pogson, Schönfeld, and Winnecke. When no authority is given, the month specified must be regarded as little better than a mere conjecture as to the time of return. I am under especial obligation to the valuable information contained in Dr. Winnecke's paper, in *Ast. Nach.* No. 1224; as also to Mr. Baxendell, for his communication of the dates for λ *Tauri* and the other stars bearing his initial.

For stars given under my own signature I have deduced fresh elements, from all observations up to the present time. The total number of stars of undoubted variability now amounts to ninety-nine; sixty-two of which are sufficiently under control to be given in an ephemeris.

V *Virginis* is an interesting variable star, discovered by M. Hermann Goldschmidt, but at present necessarily very uncertain both as regards time and light. The position of S *Scorpii* is a mere estimation, though probably not a bad one; that of its neighbour R *Scorpii* is more trustworthy; both have unfortunately been incorrectly given on several previous occasions. The N.P.D. of S *Serpentis* differs 20′ from that given by Dr. Winnecke, and I cannot say which is correct. The R.A. of T *Capricorni* is also doubtful by some few seconds.

Hartwell Observatory, December 20, 1859.

Ephemerides of the Telescopic Appearances of the Lunar Craters Geminus, C, and Bernouilli, March and April, 1860. By W. R. Birt, Esq.

Geminus.

A walled crater (20) on Rev. T. W. Webb's index-map, 54 miles broad; eastern wall, 12,300 feet, western, 16,700 feet high, on the authority of Beer and Mädler. At about 4·14 days of the moon's age this crater is seen as a deep excavation below the surface. Under the morning illumination it presents a clear elliptical outline; at mid-day it undergoes a remarkable change, the northern portion being brightly luminous, the southern dusky, which is separated from the northern by a narrow rille. In the evening the elliptical form again comes out, by the strong contrast of light and shadow.

Morning Illumination.

G.M.T.	Moon's Age.	Form.	South Wall.	North Wall.
March.				
d h m	d			
26 8 0	4·25	Elliptical.	Terraced.	
28 8 0	6·25	,,	,,	Rim distinct.
29 8 0	7·25	,,	Rille across the floor.	
30 8 0	8·25	Divided.		

Mid-day Illumination.

G.M.T.	Moon's Age.	Form.	North Wall.
March.			
31 8 0	9·25	Half lucid; half obscure. Rille across the floor forming the southern boundary of the northern lucid portion. The crater is bounded by a dark ribbon-like fringe on the eastern wall.	Bright.
April.			
4 10 0	13·34		

Evening Illumination.

G.M.T.	Moon's Age.	Form.	Eastern Wall.	Western Wall.
April.				
5 10 0	14·34	Elliptical.	In shadow.	Bright.
6 10 0	15·34	A small crater, extreme south.	,,	,,
7 10 0	16·34	A spot on north terrace.	Terraced.*	Terraced.*
7 15 0	16·54		Shadows advancing.	

* These terraces are on the S.W. and N.E. parts of the wall.

C.

A small crater, situated *close* to the south-western terrace of *Geminus*, becoming rather *intensely* bright as the lunar day advances, and exhibiting a streak of light at first rather broad, but narrower and *forked* under the mid-day illumination. It is marked on Beer and Mädler's map, but not noticed by Webb.

Morning Illumination.

G.M.T.	Moon's Age.	Greatest Libration.*	Aspect.
March.			
d h m	d	d	
26 8 0	4·25	−2·05	Southern rim in shadow.
28 8 0	6·25	−0·05	Streak *broad*.
29 8 0	7·25	+0·95	,, ,,
30 8 0	8·25	+1·95	Crater brightly luminous.

Mid-day Illumination.

31 8 0	9·25	+2·95	Apparently some distance from Geminus.
April.			
4 10 0	13·34	+7·04	Very bright; the streak *narrow* and forked.

Evening Illumination.

5 10 0	14·34	+8·04	Eastern wall in shadow.

The crater afterwards becomes very *insignificant*, as compared with its mid-day aspect.

Bernouilli.

A crater north-west of *Geminus* (21) in Webb's map, and characterised by a *dark* floor, offering, in this respect, a fine contrast to the *lucid* portion of *Geminus* under the mid-day illumination. This dark floor appears in some way to be connected with a *dark* space seen to great advantage under the mid-day illumination, and therefore more of the character of a selenological "marking" than an appearance dependent on hypsometrical relations. This dark space extends to the margin of the crater *Messala*, and appears to be remarkably persistent during the three illuminations.

* Epoch of greatest libration, N.E., March 28 d 9 h 10 m
,, ,, ,, N.W., April 9 19 26

Morning Illumination.

G.M.T.	Moon's Age.	Aspect.
March.		
d h m	d	
26 8 0	4·25	Outline of wall distinct; a dark spot at the S.W. point.*
28 8 0	6·25	The dark spot distinct; the N.E. wall bright.
		Mid-day Illumination.
31 8 0	9·25	Floor very dark; ring bright, and generally visible.
April.		
4 10 0	13·34	
		Evening Illumination.
5 10 0	14·34	Eastern part of floor *darker*; two ridges extend from the southern part of the wall.
6 10 0	15·34	Eastern wall in shadow; western luminous, ridges very apparent.
7 10 0	16·34	Strong contrast of light and shadow; lucid spot at S.W. point.*
7 15 0	16·54	Shadow extending over floor of crater opening in the wall between the two ridges.

For indicating the most favourable epochs for catching *minute details* the distance in time from the epoch of perigee is here added. This should be given with every observation, as well as the data pointed out by Mr. Webb.

Epoch of Perigee, April $4^{d}\cdot0^{h}$.

G.M.T.				Perigee.	G.M.T.				Perigee.
	d	h	m	d		d	h	m	d
March	26	8	0	−8·67	April	4	10	0	+0·42
	28	8	0	−6·67		5	10	0	+1·42
	29	8	0	−5·67		6	10	0	+2·42
	30	8	0	−4·67		7	10	0	+3·42
	31	8	0	−3·67		7	15	0	+3·62

* The dark and lucid spots are the same.

The following notices of the Great Comet of 1858 and of Occultations have been communicated to the Society by the Board of Trade through Admiral Fitzroy.

The Great Comet of 1858.

Captain J. W. Callow, commanding the "Charles" in the Southern Ocean, reports as follows:—

"October 11. At noon, lat. 36° 17′ S., long. 22° 52′ W. At $7^h\ 15^m$ P.M. saw a comet, and at $7^h\ 25^m$ P.M. found it bore by compass N.W. by W. ½ W., its altitude at the same time being 12° 21′. At 7^h 42′ its altitude was 6° 47′; and its angular distance from *Venus* at $7^h\ 53^m$ was 26° 23′: the length of the tail being 8 or 9°. At setting it bore W. 10° N. (by compass?). Variation by observation 9° 42′ W. The nucleus of the comet was about the size of a star of the first magnitude, of a yellowish light, and distinctly visible through cumulo-stratus clouds, through which the stars in its neighbourhood were not seen. The tail, which traversed the constellation of *Scorpio*, made an angle of about 15° with the horizon. Barometer $30^{in}\cdot17$, thermometer 55°·0.

"October 12. At noon, lat 35° 47′ S.; long. 21° 40′ W. At $7^h\ 18^m$ P.M. in lat.* 35° 50′ S. and long. 21° 8′ W., saw the comet bearing N.W. by W. ¼ W. at an altitude of 13° 57′; its angular distance from the moon was 37° 47′; from *Venus*, 22° 40′; and the distance of *Venus* from the moon was 19° 13′. Two stars, one of which belonged to the bow of *Sagittarius*, were visible through the luminous vapour of the tail; one of these stars was 2° 0′ distant from the nucleus, as near as could be observed with the aid of a sextant. The nucleus was very bright, and visible after sunset before any stars in the same region of the heavens; being also seen through cumulus and cumulo-stratus clouds. Barometer $30^{in}\cdot12$, thermometer 56°.

"October 13. This evening being cloudy, the comet was only seen for a short time, and there was no opportunity for taking observations.

"October 14 and 15. Cloudy on both evenings, comet not visible.

"October 16. At $7^h\ 25^m$ P.M. in lat. 35° 22′ S. and long. 13° 51′ W., the comet's distance from *Antares* was 9° 28′; its altitude being 22° 18′; that of *Antares*, 29° 0′. The distance of *Antares* from *Venus* was 1° 19′; I have given the distance between *Venus* and the star, because about this time they are nearly in conjunction at Greenwich; and the distance may be some guide as to the difference of parallax between that at the ship's position and at Greenwich. The comet very

* The latitudes and longitudes given at the times of observation are derived from those given at noon; supposing the latitude and longitude to vary as the time.

luminous to-night; its nucleus is in size and appearance like *Saturn*, the tail very distinct and fully 10° long. Barometer 29^{in}·53, thermometer 61°·0.

"October 17. At 7^h 30^m P.M. in lat. 35° 25′ S. and long. 10° 9′ W., the distance of the comet from *Antares* was 6° 56′; its altitude, 24° 0′; that of *Antares* being 26° 12′. The tail of the comet appears longer than it was last night, and its nucleus is very large and bright. Barometer 29^{in}·70, thermometer 58°.

"October 18. The comet seen for a short time to-night; it appears to be still increasing in brightness.

"October 19. The comet very distinct; its tail now forms an angle of about 60° with the horizon, and its nucleus appears to be approaching *Venus*.

"October 20 and 21. The comet only visible for a short time on these nights.

"October 22. Comet distinctly visible, its position has greatly changed, and the tail is now nearly perpendicular to the horizon.

"October 24. Comet seen for a short time; the nucleus does not appear to have diminished in brightness yet.

"October 26. Comet distinctly visible for a short time; the tail appeared longer and larger, though not so luminous as before. The nucleus is very bright.

"October 28. To-night the comet was seen very brilliantly for a short time. The nucleus was close to the tail of *Scorpio*, and the tail of the comet terminated in the bow of *Sagittarius*.

"October 29. Comet not so distinct to-night, though it does not appear to have diminished in size.

"November 1. Comet seen only for a short time.

"November 4. Comet visible for a short time this evening, it is now in the constellation *Corona Australis;* its shape is somewhat altered, the head being more pointed, and the tail like a double bow.

"November 6, 7, and 8. The comet was seen each evening; it has now greatly decreased in brilliancy.

"The comet was seen very indistinctly with the naked eye on the 11th, and was not visible afterwards without the aid of a telescope; it was seen for the last time with the assistance of a 2·5-inch glass on November 16th."

This comet was also seen by Captain Trivett of the Hudson Bay Company's steamer, "Labouchere," for the first time on the 5th of October, in lat. 5° 20′ S. and long. 29° 55′ W.; and (its last appearance) on November 1st, in lat. 38° 45′ S. and long. 55° 10′ W. The following positions* are given by Captain Trivett, as deduced from sextant observations.

* It is not stated what corrections were applied to the observations used in deducing these positions.

	G.M.T	Comet's R.A.	Comet's Decl.
	h m	h m s	° ′ ″
October 7	9 7	14 35 55	+13 41 35
8	9 8	14 47 15	10 7 38
9	9 11	14 59 27	+ 6 41 43
13	9 16	15 47 11	− 4 3 41
14	9 17	15 56 56	−10 34 51

On board the Arctic discovery yacht "Fox," Captain M'Clintock, the comet was seen on September 13th and 23d, and on October 8th and 9th, in (about) lat. 72° N. and long. 94° W.

Captain James Clarke of the ship "Marco Polo," likewise reports having seen the comet (for the first time) in lat. 53° 0′ S. and long. 56° 46′ W., on October 11 about 8 P.M., at which time its tail was 2° long. The comet afterwards increased in size, but the weather being very unsettled it is uncertain when it began again to decrease; it was seen for the last time on November 5th, in lat. 5° 57′ S. and long. 27° 17′ W. The following are the observations for position.

"October 15. $10^h\ 35^m\ 0^s$ G.M.T. The comet bore W. by S. by compass from the ship; its altitude was 17° 40′; the distance from the moon (query? centre, or bright limb) 59° 50′, and from *Venus* 12° 32′. The tail this night was 3° long. Barometer $29^{in}\cdot80$, thermometer $42^\circ\cdot5$ in air. Variation 6° E.

"October 22. $9^h\ 24^m\ 0^s$ G.M.T. The comet 3° 4′ distant from *Venus*, and 8° 51′ from *Antares;* its altitude 29° 55′, and that of *Venus* 26° 51′. Barometer $30^{in}\cdot14$, thermometer $63^\circ\cdot0$.

"November 5. $9^h\ 58^m\ 0^s$ G.M.T. The comet now much fainter than when first seen, its distance from *Venus* is 17° 46′, its altitude 16° 25′, that of *Venus* being 8° 37′. Barometer $30^{in}\cdot04$, thermometer $78^\circ\cdot0$."

Occultations.

Captain David Smith observed the following occultations on board the ship "Hanover," the first on the outward passage to India, and the second on the return voyage to England.

"1858, July 23. In lat. 34° 14′ S. and long. 77° 12′ E., observed the star φ *Sagittarii* disappear at the moon's dark limb at $17^h\ 5^m\ 0^s$ apparent time.

"1859, May 8. In lat. 50° 33′·5 N. and long. 0° 28′ E., observed the planet *Saturn's* remote limb disappear at $8^h\ 27^m\ 17^s$ apparent time at ship."

Occultation of the Pleiades, 1859, December 8, observed at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

Phenomenon.	Moon's Limb.	Greenwich Mean Solar Time of Observation. Hour and Minute. h m	Seconds Observed by M	D	C	T
23 Tauri, disapp.	Dark	4 7	3·8	4·5	4·3	
16 Tauri, reapp.	Bright	4 10	...	...	46·8	
17 Tauri, reapp.	Bright	4 25	60·7	60·7	58·8	
η Tauri, disapp.	Dark	4 32	54·5	54·9	55·1	
23 Tauri, reapp.	Bright	4 48	...	48·0	44·6	
28 Tauri, disapp.	Dark	5 18	16·1	17·0	16·7	
η Tauri, reapp.	Bright	5 24	36·0	31·1	26·4	
27 Tauri, 1st disapp.	...	5 34	47·4	35·4	...	
27 Tauri, 2d disapp.	...	5 35	...	30·3	...	
27 Tauri, reapp.	...	5 38	...	31·8	...	
28 Tauri, reapp.	Bright	5 59	...	...	40·9	38·8

Notes by Mr. Main [M]. Observed with the Sheepshanks' equatoreal. The reappearance of 17 *Tauri* uncertain to a second or two; that of η *Tauri* was observed with a very low power, and the star was exceedingly faint when first seen, but it appeared to be in contact with the limb. 27 *Tauri* appeared to run along the limb at a part where it was slightly unilluminated, and to vanish at the central of three flat detached patches of illumination. Returning to the telescope two minutes after the disappearance, the star was again visible.

By Mr. Dunkin [D]. Observed with the Altazimuth. The disappearance of 27 *Tauri* was a most singular phenomenon; the star appeared to move a considerable time along the moon's limb, and disappeared behind a prominence at the first time noted; in a few seconds it reappeared, and finally disappeared at the second time noted. The other observations were generally satisfactory.

By Mr. Criswick [C]. Observed with the North Equatoreal. 16 *Tauri* was separated from the moon's limb when first seen, and the observation is not, therefore, satisfactory. 27 *Tauri* was not occulted at all, though it passed so close to some of the illuminated peaks of the dark limb as hardly to be distinguishable from them.

By Mr. Talmage [T]. Observed with the Sheepshanks' equatoreal. The star was very faint; it was slightly separated from the limb when first seen.

Occultations of the Pleiades observed at Highbury, December 8, 1859. By T. W. Burr, Esq.

		Local Sidereal Time. h m s	
„ Tauri (Alcyone)	Immersion	21 40 13·5	Dark limb.
	Emersion	22 31 56	Bright limb.
23 Tauri (Merope)	Emersion	21 56 26	Bright limb.
27 Tauri (Atlas)	Immersion	22 40 55	Bright limb.
	Emersion	22 45 7	

The disappearance of *Alcyone* was instantaneous, and an excellent observation as to time. The reappearance is a little doubtful, the eye not having been directed to the exact point. The phenomena attending the occultation of 27 *Tauri* were very interesting. It approached the upper rugged limb of the moon, and continued to graze along the mountain ridges, and to look like a double star with several isolated illuminated peaks, but at last vanished, and was hidden for the time stated. 20 *Tauri* also grazed the lower limb of the moon, and I lost sight of it for a few seconds; but this may have arisen from the brightness of the lunar surface with which it was in contact, and my attention being drawn off to watch *Alcyone*, which was approaching immersion.

Telescope employed, $3\frac{3}{4}$ inches aperture, by Ross, focal length 52 inches, power 128. Latitude 51° 33′ 45″·1 N.; Longitude 23ˢ·8 W.

Dec. 15, 1859.

Occultations of Stars by the Moon. Observed by Captain Noble (previously unreported).

1859, Wednesday, April 13.

37 *Sextantis.*

	h m s	
The star disappeared instantaneously at	10 10 39·2	L.S.T.
and reappeared also instantaneously at	10 58 41·0	L.S.T.

Saturday, July 16.

ι *Capricorni.*

	h m s	
The disappearance took place at	20 38 57	L.S.T.
and the reappearance at	21 42 13	L.S.T. (subject to an error of 1ˢ)

The star entered into contact in a remarkably sluggish manner; the moon's limb apparently exerting a repulsive

power upon it, as though, so to speak, the moon and star were *similarly electrified:* but although an inordinate time elapsed before the disappearance took place, yet nothing in the nature of a projection of the star upon the moon's limb was seen.

Wednesday, September 21.

μ^2 *Cancri.*

	h	m	s	
The star disappeared at	1	31	32·9	L.S.T.
reappearance not seen.				

The atmospheric tremor was very great.

Tuesday, November 8.

101 *Piscium.*

	h	m	s	
The star disappeared instantaneously at	1	14	17·7	L.S.T.

The emersion is untrustworthy on account of the overpowering brightness of the moon.

Thursday, December 8.

The *Pleiades.*

	h	m	s		
27 Tauri disappeared instantaneously at	21	40	10·6	L.S.T.	
and reappeared at	22	31	53?	L.S.T.	emersion very doubtful.

27 *Tauri* was not occulted at all at this station, the moon's limb passing within a few seconds of the star, but never covering it.

	h	m	s	
28 Tauri disappeared instantaneously at	22	25	52·5	L.S.T.
and reappeared about	23	6	33·0	L.S.T.

The telescope employed was my Ross Equatoreal of 4·2 inches aperture, with a power in all cases of 115, adjusted on the respective stars.

Observatory, Forest Lodge, Maresfield, December 8, 1859.

New Double Stars discovered by Mr. Alvan Clark.

(*Communicated by the Rev. W. R. Dawes.*)

(*Second series.*)

Curr^{t} No.	Designation.	R.A. 1860. h m s	N.P.D. 1860. ° ′	Mag.	Dist. ″	Date of discovery.
13	Lalande 1980	1 0 51	45 32·4	8, 8	0·45	1858, Nov. 29
14	Lalande 2634	1 20 7	47 56·6	8, 9	0·8	1858, Oct. 13
15	99 Herculis	18 1 43	59 27·4	5½, 10½	1·7	1859, July 10
16	* Vulpeculæ	19 52	63 7	7½, 8	0·4	1858, Aug. 30
17	Cygni 153 B / B.A.C. 6959	20 8 38	38 57·4	6, 11½	3·8	1859, July 19
18	44 Cygni	20 25 41	53 32·0	6½, 11½	2·8	1859, July 6
19	A.Z. xxiv. 11	21 10 50	26 10·2	7½, 7½	+0·9	1859, July 8
20	75 Cygni	21 34 41	47 21·6	5½, 11	2·9	1859, July 19

All the double stars in this series may be considered as good test-objects for telescopes of from 6 to 8 or 9 inches aperture. But it is not merely as such that they are interesting: they become especially so from the fact that they are all situated in the northern hemisphere, and all but one at more than 30° from the equator. They consequently attain a good altitude at Pulkova; notwithstanding which they have escaped the acute search of M. Otto Struve with the 15-inch refractor, in addition to the previous one by his father with the Dorpat telescope of 9·6 inches aperture. Either, therefore, they have recently undergone considerable change; or, if not, it appears that objects of great difficulty and delicacy may be detected with very perfect telescopes of smaller size, which have escaped the most diligent scrutiny with far larger instruments. The whole of these objects were discovered by Mr. Clark with his own object-glasses of 8 and 8¼ inches aperture, and five of them in my own observatory, during his visit to me last summer; since which I have met with *seven others*, of similar character and situation; and it should be mentioned that none of the objects recorded are below Struve's eighth magnitude for the larger star; all smaller, of which many have been found, having been systematically rejected. It appears, therefore, that there is still much occupation for telescopes of moderate dimensions, even in this department of astronomy, which might reasonably have been supposed to have been long since exhausted. The *distances* stated in the list are from my own recent measurements.

Haddenham, Thame,
November, 1859.

Remarkable Solar Spot. By the Rev. W. R. Dawes.

Having turned my 8½-inch refractor upon the sun, about noon on the 22d of October, I noticed that near the south-eastern edge there was an unusually large mass of bright streaks, or *faculæ*. With the whole aperture and power 234 the edge was at times extremely quiet and very sharply defined. During the best views I satisfied myself that *a bright streak, which formed the very edge, projected irregularly beyond the circular contour of the edge;* reminding me of a ridge of low hills often seen at the enlightened limb of the moon. Nearly parallel to this, and about two or three seconds from it, ran another streak nearly as bright; and the space between them was observed to be darker than is the ordinary limb of the sun, but not so black as the *umbra* of a spot. I supposed it to be the *penumbra* of a large spot just entered on the disk, the umbra of which was as yet hidden by the inner bright streak.

At 2^h p.m. I went again to the telescope. The state of the air was less favourable; but during the most tranquil moments I satisfactorily made out an excessively narrow *black line*, a little broken in two or three places, as if by irregularities in the inner bright streak, the top or outer edge of which was projected upon it. Clouds soon covered the sky, and an extraordinary storm of hail, sleet, and snow, came on.

The 23d was cloudy. On the 24th a fine spot was visible, with a remarkably extensive penumbra. At about 1 o'clock I made a sketch of it. On the 26th I saw it again; and finding the *umbra* had changed its form in a very extraordinary way, I carefully sketched it under tolerably favourable circumstances. I subsequently saw it well, and sketched it carefully, on the 27th, 29th, and 31st of October, and on the 2d and 3d of November. The changes were more extraordinary than I ever before observed, and some of the forms which the umbra assumed were curiously grotesque. The accompanying tracings from my sketches are as faithful representations of their outlines as my eye and hand could produce.*

The last transient view which I had of the spot was on November 4th, in the afternoon; and it passed off in the night of that day.

It appeared to me very remarkable, that in so violent and extensive a disturbance of the luminous envelopes, I could not find in any part of the *umbra* a *really black nucleus*. In spots of not one-tenth part of the area I have, in numerous instances, perceived a decidedly *black* portion, not always central, but comparatively well defined, and totally devoid of any light visible through the darkening glass rendered necessary by the

* These delineations were exhibited at the meeting, and attracted much attention.—Ed.

solar illumination of our own atmosphere. The umbra of this large spot was, however, of various depths of shade in different parts. It appears probable that the upward force by which the luminous envelopes were thrown aside must, in this instance, have acted upon them *very obliquely*.

To obtain ocular demonstration of the bright streaks being really *elevated ridges or waves* of the exterior luminous envelope, is, of course, a very rare occurrence; but in the present case the evidence was as complete as could be desired. The combination of circumstances was most fortunate; a bright streak of unusual size being precisely at the sun's edge, and the state of the air permitting the use of a large aperture and high power with full advantage.

The same spot has now come round again; but beyond the richness of its vicinity in *faculæ*, there is nothing remarkable in the group into which it has resolved itself.

Haddenham, Thame,
21 November, 1859.

Physical Observations of Jupiter's Satellites.

By W. Lassell, Esq.

I take the liberty of drawing the attention of telescopic observers to the fourth satellite of *Jupiter*, which, after a considerable interval, has this year begun to pass across the disk of the planet. I saw it last night from about $11^h\ 30^m$ to $12^h\ 30^m$ P.M. near the following and south limb. At first I took it be a shadow of one of the other satellites; but from the peculiarity of its aspect, was induced to refer to the *Nautical Almanac*, which showed it to be the body of the fourth satellite. On viewing it again, I thought it scarcely so *black* as the shadows ordinarily appear, yet so very dark that a cursory glance would certainly have taken it for a shadow; and it was difficult to conceive that any object so dark in its present aspect could ever appear as a bright one. I remember, many years ago, having noticed it as a dusky spot; but I fancy never so dark as now. The atmosphere was not fine enough to see anything new upon the face of the planet; but reflecting upon the very different phenomena now presented from those which marked the disk twenty years ago, one cannot but be filled with wonder at the stupendous chronic changes which must be taking place, even though they should be wholly or principally confined to the atmosphere of the planet.

I had an opportunity, a few minutes after midnight, of noticing the striking contrast between the bodies of the fourth

and first satellite, the latter having then caught my attention as a very brilliant spot near the preceding limb. The observation of this contrast prepares one for the belief that the darkest solar spots might, if removed from the insupportable splendour of the sun's disk, appear as very bright and shining objects.

Bradstones, Sandfield Park, Liverpool, Dec. 8, 1859.

Physical Observations of Jupiter. By Sir W. Keith Murray.

"I enclose a few diagrams of *Jupiter*,* selected out of twenty-one taken since Sept. 26th between 15^h and 19^h G.M.T. The atmosphere has seldom been very favourable until this morning, when several conspicuous white spots were seen on the second belt, two of them being nearly vertical.

"The principal belt had quite a flocculent cloudy appearance; indeed, it presents a very mottled muddled aspect this season; more so than I have noticed before. I hope other observers who possess sufficient means may have been favoured with more propitious weather than I have, as the thermometer has generally ranged from 25° to 33° Fahr. at the hours of observation.

"I have reason to think a small star has been occulted by *Saturn*, as I saw it near the planet on several successive mornings to the apparent east, but cloudy weather prevailed; it being seen till this evening, where I observe it now to the apparent west, and in such a position that I think the planet must have occulted it. It was less bright than *Dione* or *Tethys*.

"*Ochtertyre, Crieff, Nov.* 14, 1859."

Extract of a Letter dated Dec. 9th, from W. S. Jacob, Esq., to one of the Secretaries.

"Adverting to p. 15 of the *Monthly Notice* for November, I beg to refer you . . to my drawings of *Jupiter*, as seen at Madras in Dec. 1857 and Jan. 1858, copies of which were sent to the Society at the time. . . You will there see, I fancy, the same thing as M. Schwabe describes,—the shading of the belts in fine dark nearly parallel streaks, very different from anything I have seen described or delineated by any one else, whether by Dawes, Lassell, or Smyth (?); indeed it takes first-

* These diagrams were exhibited at the meeting.—Ed.

rate definition to bring out such fine markings, such definition as is rather rare in this climate, I believe. Dawes's round beads I have also never seen, but only occasional irregular blotches, more or less elongated (like cirrus clouds), here also agreeing with M. Schwabe."

On the Position of Venus during the Total Eclipse of July 18, 1860. By Prof. Chevallier.

"I have not observed any notice that *Venus* will be within a very few hours of her inferior conjunction with the sun during the total eclipse on the 18th of July, 1860. This will afford an excellent opportunity, within the track of the moon's shadow, of making observations on the character of the cusps of *Venus*, as affected by her atmosphere, and possibly of her diameter too.

"And, as the eclipse of the sun will be large over the whole of western Europe and in part of North America, the partial darkness may facilitate observations of *Venus* at the time, even in fixed observatories.

"I do not know whether it is worth while to notice that *β Leonis* will have very nearly the same N.P.D. as *Venus*, and nearly four hours greater Right Ascension.

"*Durham, Dec.* 8, 1859."

On the Physical Constitution of Comets. By O. G. Downes, Esq.

The author proposes to inquire how far the appearances and motions of comets may be due to the operation of known laws on a body similar in constitution to the earth. It is argued that if our earth were to move in a long elliptical orbit, the degree of cold to which it would be exposed in aphelion would be sufficient to solidify not only the aqueous matter on the surface, but the atmosphere itself; and the author proceeds to consider how the redissolution of the atmosphere, as the body again approaches the sun, would give rise to phenomena such as are presented by a comet. The author remarks that his hypothesis indicates that comets belong to the same family as planets, and that with continuously contracting orbits they will, in the fulness of time, take their place with the other planets in the solar system.

Description of an Equatoreal recently erected at Hopefield Observatory, Haddenham, Bucks. By the Rev. W. R. Dawes.

My observatory was furnished, in May last, with an equatoreally-mounted telescope by Messrs. Alvan Clark and Sons, of Boston, U. S., which in several important points differs from any other in this country; and I therefore hope that a brief description of it may not prove uninteresting to the Royal Astronomical Society.

The accompanying picture of it by Mr. Alvan Clark shows its general form, which combines great firmness and compactness with considerable elegance of design. The massive part of its structure is of cast-iron, the base of which is firmly bolted down to a stone pier. The semicircular form of the upper part affords a secure position for most of the wheel-work of the driving-clock, of which the going-weight descends in a groove on the east side of the pier, and is not seen in the drawing. The space between the polar axis and the semicircular bed-piece is occupied at its lower part by the hour-circle. Immediately above this is a sector, which clamps on to the axis, and the wheel-work of the clock occupies the upper portion. The sector has a radius of rather more than 9 inches, and an arc of 30°, or two hours of right ascension. This arc has a face of an inch and a half in breadth, between which and a cylinder 7 inches in circumference there is just room enough for two thin bands of sheet-brass, each of about three fourths of an inch in width, to pass side by side. These bands are both keyed by the end into one groove in the cylinder, at such a distance that they cannot overlap or interfere with each other. They are then bent round the cylinder in opposite directions, the end of one being fastened to one extremity of the arc of the sector, and the end of the other at the other extremity of the arc to a piece of brass which is acted upon by a screw and nut, for giving to both the bands a due degree of tension. The sector and cylinder thus move together without friction, irregularity, or lost time.

Upon the same arbor with the cylinder is the wheel, 15 inches in circumference, in the racked edge of which the driving-screw works. This arrangement gives the screw about the same driving-power as if it acted on the edge of a wheel nearly 40 inches in diameter, fixed on the polar axis.

I have every reason to be satisfied with the going of the driving-clock; and the cylindrical bob of the pendulum being screwed on to its steel rod, the rate is capable of adjustment to the greatest nicety. Great care has been bestowed by the makers upon the accurate dividing of the wheel-work; and I have much pleasure in acknowledging that its performance fully bears out my expectations, founded on the character

given by the Messrs. Bond of the clock-work applied by the same makers to the great Munich equatoreal in Harvard Observatory, which has been so successfully employed for the purposes of telescopic photography. While the speed of the clock is regulated by the vibrations of the half-seconds pendulum, the action of the pendulum on the wheel-work is rendered smooth and equable by an ingenious application of Bond's *Spring-governor;* and so perfectly successful is this contrivance, that with the thread of the micrometer bisecting a star, and a power of 800 or 1000 on the telescope, no interruption or jerk from the escapement is perceivable.

For producing a slow motion in right ascension, the driving-screw is mounted on a brass frame, which, being carried by a fine screw under the observer's control, acts as a slipping-piece through nearly five minutes of time.

A firm clamp, close to the cradle of the telescope, fixes the declination-axis, and is accessible to the observer both at the eye-end, and also during the setting of the declination-circle. A slow-motion screw acts on an arm extending from the clamp to the bottom of the cradle to which the screw is attached.

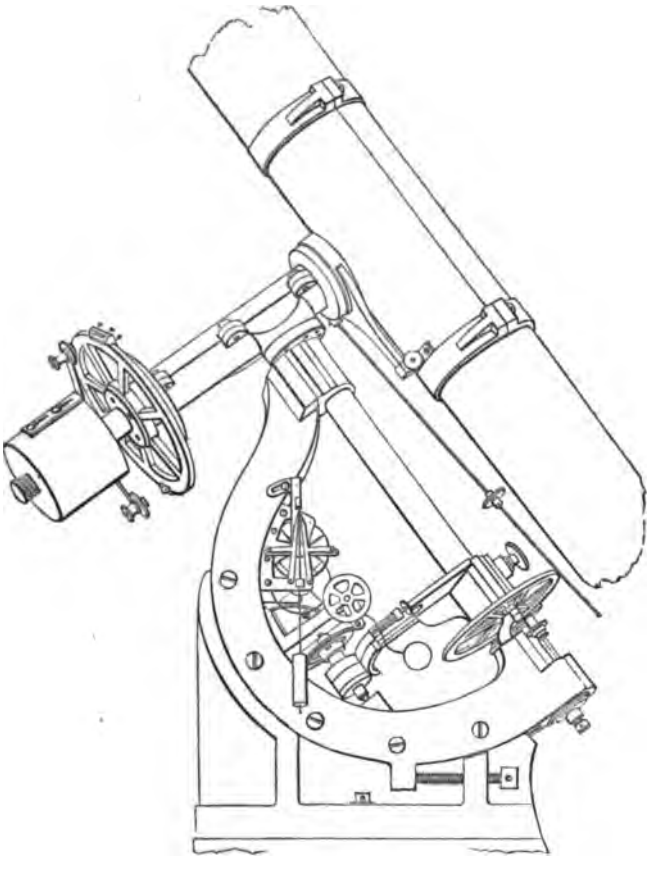
To permit the adjustment of the polar-axis to the latitude and meridian of the place, the upper part of the cast-iron bed-piece is made with a groove which receives loosely a projecting keel on the portion bolted down to the pier. The form both of the groove and of the keel being semicircular, the upper portion is moved upon the lower by the stout screw which is seen in the drawing, and the polar-axis is thus easily raised to the required angle. The adjustment to the meridian is performed by the screws on each side of the groove in the upper piece pressing against the keel in the lower, which has play enough in the groove to allow of a moderate degree of azimuthal motion.

To facilitate the finding of objects in Mr. Clark's "Two-eye-piece Micrometer," when their distance exceeds the field of one of the eye-lenses, the finder is furnished at its eye-end with a small position-circle divided into degrees. The thick wires of the finder being placed in the direction of the objects to be measured, the reading of the position-circle indicates the approximate setting for the micrometer, whereby the two objects may be immediately found by their respective eye-lenses. The aperture of the finder being two inches, it will show a star of the $9\frac{1}{4}$ magnitude of Struve's scale.

The object-glass of the telescope has a clear aperture of $8\frac{1}{4}$ inches, and a focal length of about 110 inches. The materials were furnished by Chance and Co., of Birmingham. The figure is excellent to the circumference, and the dispersion but little over-corrected. Its performance fully supports the character of Mr. Alvan Clark's object-glasses, and I believe it to be capable of everything which can be performed by such an

aperture. It clearly divides γ^2 *Andromedæ*, and shows the smallest companions among the stars of the Pulkova Catalogue.

Haddenham, near Thame,
November, 1859.



Equatoreal Mounting of a Telescope,
aperture 8¼ inches, focal length 110 inches,
by Alvan Clark and Sons, Boston, U.S.;
Erected in the Observatory of the
Rev. W. R. Dawes, Haddenham, Bucks.

The Astronomer Royal called the attention of the Society to the approaching total solar eclipse of 1860, July 18. His remarks were suggested, partly by the circumstance that the Superintendent of the *Nautical Almanac* had that evening presented to the Society copies of his new map of the path of the shadow through Spain and Algeria, and partly by the appearance in the French *Comptes Rendus* of suggestions by M. Faye as to the apparatus which might be used with advantage in the observations.

In the first place, a selection is to be made of the place of observation. The extreme facility now given by the French railways for the journey as far as the southern frontier of France (either at Bayonne or at Perpignan), and the continuation of the Spanish royal road, on which it is understood that the travelling is very good, through Vittoria and Burgos (both in the band of total obscuration), would probably determine amateurs in general to fix on one of these cities. But it ought to be considered that the heat in these places, at the season of the eclipse, would be very great; and the accommodation for travellers in Spain is said to be most uncomfortable to Englishmen. Those who might prefer the eastern coast of Spain would find it easy to go from Perpignan to Barcelona; but there would remain a long journey (on which he could give no information) to the point crossed by the eclipse-shadow. To himself it appeared that, if conveyance by sea could be obtained, it might be best to adopt the port of Santander, which is nearly in the centre of the shadow-path.

Adverting then to the phenomena of the eclipse, it was at once assumed that the appearances specially requiring attention are the "red prominences." Upon the locality and origin of these there is much obscurity. Illustrating his remarks by a small model, the Astronomer Royal endeavoured to point out what is known and what is not known. He could assert with the utmost certainty that, in the total eclipse of 1851, as the eclipse proceeded, red projections were lost on one side of the moon's black disk, and new red projections appeared on the other side, exactly as if they were attached to the sun, and were covered on one side and uncovered on the other side by the motion of the moon. On the other hand, it was not remarked by any of the observers that the red prominences which were visible throughout the totality (of which the most remarkable was that known as the "boomerang") altered their elevation in a degree corresponding properly to the motion of the moon. Moreover the appearance of the "boomerang" was not exactly the same at all observing stations; at some, instead of red, it was white; at some it was partially connected with the floating balloon, while at others no such connexion was seen. Apparently this had led some philosophers to conceive that, though the light which formed these prominences came in the first instance from the sun, the

appearances remarked might in reality derive their peculiarities from some action of the surface of the moon upon the light which grazed over it. Cosmically, it is of the utmost importance to determine whether these appearances are attached to the sun or to the moon. This suggested the remarks which followed.

The second point, then, which requires the observer's attention is the mode of defining the angular position of any prominence upon the moon's edge at least twice during the obscuration. The telescope being firmly planted, if its motion were equatoreal the polar point or equatoreal point would be zero; if its motion were altazimuthal the vertical point would be zero. These would be equally good, and all that is then wanted is a mode of referring any prominence to the vertical line or the polar line passing through the centre of the moon's disk. M. Faye, in treating of this, had condemned all attempts at determining the angular position by anything like a toothed circle in the field of view surrounding the moon; first, because observations so made had hitherto been discordant, and, secondly, because they implied the use of a power so low that the whole disk could be seen. He proposed, instead, to use a wire or straight bar in the field, which could be made a tangent to the moon's limb at the point where the prominence is seen. By means of the model it was shown that if a prominence were seen on these parts, which are nearly 90° from the points of first and last totality-contact, its angular position, if it belonged to the moon, would not be altered; but if it belonged to the sun, it would be altered about 6° in the course of the obscuration. M. Faye conceived that this could be well determined by a tangent-wire. The speaker suggested for consideration whether, if the field of view were armed with six intersecting wires, dividing its circumference into twelve parts of 30° each, such a change of angle as 6° might not be very well estimated by the eye.

The third point is, the method of measuring the elevation of the prominences. By means of the model it was shown that the prominences to which the second head alludes would not sensibly change their elevation during the totality; but that those situated near the points of first and last totality-contact, if they belonged to the sun, would greatly change their elevation. Observers must prepare themselves with some system of measuring these elevations, at least approximately. Probably it would be very difficult to apply anything like screw-motion; but perhaps an estimation might be made, by the use of parallel wires at a known distance, or some other contrivance.

The fourth point requiring the observer's consideration is the mode of making a record. The difficulties to be overcome here are of three kinds: first, the shortness of time; secondly, the extreme darkness, which makes it difficult to read anything without artificial light; thirdly, the difficulty of keeping an assistant to his duty during the strange and appalling appear-

ances of a total eclipse. The speaker recounted a history which he had heard from M. Arago of the preparations made in detail at one station for the eclipse of 1841, but which failed totally from the impossibility of maintaining discipline. In regard to the use of artificial light, the Astronomer Royal believed that, in the eclipse of 1851, he had lost much time and important results from attempting to refer to it, and he would earnestly dissuade observers from any use of a lamp. If an assistant were writing, or reading a chronometer, he must have a lamp. But without relying on an assistant, some arrangements like the following may be considered. M. Faye had proposed to register the position of a rotating wire (for angular position of prominences) by scratches upon a circle of soft tin. This gives no record of the order of succession of the registers. The man of cool temperament and good memory might trust to his recollection of the estimations of angle, &c. Or it might be advisable to acquire the habit of writing in the dark, and to devote one leaf of a small pocket-book to every record, however brief.

The fifth point is the method of registering time. The aid of a trustworthy assistant, with a chronometer and lamp before him, would remove all difficulties from this and other parts of the operation. Even in those parts where the observer and the chronometer-reader had both to make records, the use of a numerical reference (No. 5, No. 6, &c.) would connect both without risk. Or some mechanical device like the following might be used. The totality occupies less than 4^m of time. Suppose, then, a barrel to be made to revolve in about 5^m or 6^m of time (no accuracy is required). An impression might be made on this, without any sensible friction, by the touch of a camel's-hair pencil carrying oily ink (watery ink might dry up in the arid atmosphere). Upon this barrel, then, the time of every observation might be registered without confusion; and, the 60^s of chronometer last preceding and first following the totality being recorded in the same way, the zero and scale of time would be obtained.

The Astronomer Royal acknowledged his obligations to Mr. Carrington for remarks long since made by him on the importance of attempting to secure accuracy in these observations.

Finally, the Astronomer Royal urged strongly on the members of the Society that he had no intention to recommend to them the exclusive adoption of any of the methods which he had sketched out. The experience of two total eclipses had made him acquainted with the phenomena and with the difficulty of observing them properly; and his whole intention now was, after conveying some idea of the points to be observed, to impress strongly upon intending observers that they must have carefully considered the apparatus for observation, and must have fitted themselves with it, some months before the occurrence of the eclipse.

Dr. Lee read to the meeting and illustrated by large diagrams a paper of an interesting character by Mr. Pogson on the variable stars R *Ursæ Majoris*, S *Ursæ Majoris*, R *Cygni*, R *Cassiopeiæ*, and U *Geminorum*. The paper was read originally before the Mathematical Section at the Meeting of the British Association in September last, and was fully printed in the *Aberdeen Free Press* of September 16; but as many of the members had not had the advantage of hearing or seeing the paper, Mr. Pogson's results were looked upon with great interest.

MISCELLANEOUS INTELLIGENCE.

Le Verrier's *Bulletin* of the 8th December, 1859, contains some remarks by M. Faye on the recent experiments of M. Fizeau, considered in relation to the movement of translation of the Solar System. M. Fizeau had announced that in his experiments a variation of 30 kilometers per second in the velocity of a refracting apparatus placed in the path of a ray of polarised solar light was shown by a displacement of nearly a degree in the position of the plane of polarisation, when observed alternately in the direction of the ray and in the contrary direction. The experiments are published in the *Comptes Rendus*, 14th November, 1859. M. Faye considers that the experiments (although the author doubts their sufficient exactness for the purpose) ought to indicate the motion of translation of the solar system; but on discussing them in reference to this end, he finds that the influence of the annual motion of the earth is alone sensible, and that the introduction into the question of the motion of translation of the solar system causes in some of the observations discordances too great and too regular to leave any room for doubt; and he infers that, if the experiments of M. Fizeau are as exact as they appear to be, it must be concluded that the movement of translation towards λ *Herculis* does not exist; or if, on the contrary, this motion is to be taken as proved, and M. Otto Struve's velocity is not much too great (the recent researches of the Astronomer Royal on the proper motion of the Solar System seem to show that the velocity is to be increased rather than diminished), it must be concluded that the experiments of M. Fizeau are affected by some systematic error, or that there is some important lacuna in the theory adopted by him.

The Editor is requested to notify that the records of appearance seen on the sun by Mr. Pastorff, which are now in the custody of the Society, terminate abruptly in the year 1833,

and that at present no reference is possible respecting the statement of Mr. Wartmann, that in the years 1834, 1836, and 1837, Pastorff saw little bodies form over the disk in comparatively short spaces of time,—a result quoted by Mr. Herrick, of Newhaven, U. S., in a letter to M. Leverrier. The statement will receive attention.

Humboldt Foundation.

The Committee, consisting of Prof. Magnus and others, state that their desire is to establish a fund called the Humboldt Foundation: a memorial of gratitude designed to promote scientific talent wherever it appears, in all those branches in which Humboldt developed his scientific energy, viz. in works of natural history and distant travels. The application of the fund to be intrusted to the Royal Prussian Academy of Sciences at Berlin, who have agreed to plan, in conjunction with the Committee, and according to the sum collected, the statutes of the foundation, and to superintend its proper administration. Contributions to be sent to the house of Mendelsohn and Co., Berlin, or to any member of the Committee.

Wanted to purchase, a good second-hand telescope with a maximum power of about 300 or 350, and two or three lower powers and a terrestrial eye-piece; apply to the Assistant-Secretary.

ERRATUM.

Vol. XX., page 24, line 19, *for* "that the *Comet* of 1846 may be shortly *expected* at the other extremity of the planetary system," *read*, "that the *event* of 1846 may be shortly *repeated* at," &c.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *January* 13, 1860. No. 3.

Rev. R. MAIN, President, in the Chair.

The Rev. Percival Alfred Fothergill, B.A., H.M.S. Melpomene, was balloted for and duly elected a Fellow of the Society.

Evening Illumination of Fracastorius (*Lunar Crater*).
By W. R. Birt, Esq.

I obtained a fine view last evening, at $11^h\ 10^m$ G.M.T., of the crater *Fracastorius* described by Webb as an *imperfect* ring forming a bay of the *Mare Nectaris*, and marked on his index-map (372). There are several similar objects on the lunar surface demanding close attention, from the probability of portions of their walls having been destroyed since the formation of the craters. The termination last evening just grazed the *outer* portion of the ring on the western side, and the angle of illumination was such as to show very distinctly every inequality of the floor and its surrounding *enclosure*. The ring was seen to be *complete*. The following is the entry I made at the time of observation :—"The north-eastern edge of the crater (northern ?) is perceptible *as a series of low points or peaks, casting very short shadows.*"

These low peaks, seen under such an illumination as I had last evening, completely separate the crater from the mare, and, combined with the difference of level, the floor of the crater appearing as if slightly depressed below the level of the mare, would indicate that whatever force might have broken down the portion of the wall *towards* the mare (if such has been the case), the floor in this instance *has not been altered* so as to produce the appearance of a filled or partially filled crater, giving rise to a bay. Several instances of partially

filled craters may be found on the moon. The shadows of the eastern peaks were long, sharp, and well defined, one being interrupted by the brilliant point of a projecting crag. The unevenness of the floor was well seen. Epoch 1860, Jan. $12^{d}\cdot47$ G.M.T. Moon's age, $19^{d}\cdot73$. Libration, N.W.—$3^{d}\cdot76$. Epoch Jan. $16^{d}\ 5^{h}\ 34^{m}$. Perigee $+2^{d}\cdot84$. Epoch Jan. $9^{d}\ 15^{h}\ 0^{m}$.

Letter from the Rev. Father Secchi to the Astronomer Royal, received 1860, *January* 11.

"I take this opportunity to inform you that I have been progressing in my researches on the polarisation of light reflected by the moon. The principal result deserving attention is the almost perfect equality of polarisation which takes place in all the smooth parts of the moon, and that under any inclination of the plane touching the reflecting surface. The strongest indeed is in the *Mare Serenitatis*, but the difference compared with the other *maria* is quite insignificant; on the contrary, the tops of the mountains are not polarised at all in the neighbourhood of the Caucasus, Riphœi, and Apennini. In the neighbourhood of Tycho, where there is a great alternation of flat and mountainous region, the light is more polarised than at the above-mentioned mountains, and less than in the *maria*. The means of measuring the quantity of polarisation is by a pile of glass-plates as a polarometer in combination with a very sensible polariscope of *Arago*. The error in the angle is seldom one degree; but as I have not yet graduated my pile, I can only tell you the more or less, and I will give you afterwards the absolute numbers of the quantity of polarised light. This seems to me a very remarkable thing, as I have said in the *Comptes Rendus*, that we find equally polarised light on the whole smooth surface of the moon, whatever be the inclination of the incident and reflected rays. The only thing which practically gives the same result is the glass paper used in the arts. Can this kind of surface have influence in the *corona* seen during eclipses? this is not improbable. I have made this experiment:—Through a telescope I have looked at the image of the sun made in the focus of an object-glass, and have imitated the eclipse, putting in the way of the luminous rays two objects: 1st, a smooth cylinder; 2d, a cylinder covered with glass (or emery) paper, and I have seen great difference in the outline of the termination of the edges; the latter seemed to have a bright fringe and some scattered rays starting from the edges. If the moon is so constituted, no wonder if a similar effect be produced and the *corona* so generated. But as I have never seen the *corona*, I cannot say whether the appearance observed by me was equal to it or not. The experiment might be repeated by those who have seen it and can judge better.

"*Jupiter* now has a very remarkable appearance: the principal belt is decidedly red, and several bands of green and white alternate in such a way that I have never seen such colours before. I will send you some drawings."

On Dr. Sœmmerring's Observations of the Solar Spots in the Years 1826, 1827, 1828, and 1829. By R. C. Carrington, Esq.

When I visited North Germany in the year 1856, one object which I had in view was to obtain personal information of the observations of the solar spots made by Dr. von Sœmmerring; as I thought it probable, from the account given of them by Professor Thilo, in a dissertation published in the year 1828, that records made by a man of Sœmmerring's eminence would exhibit a degree of accuracy which would repay the labour of reduction; and, when reduced, would put me in immediate possession of an ancient series which might enable me to obtain a more exact value of the time of rotation of the sun on its axis. I started under the impression that these records extended over a much longer period than I have since found to be the case, and that they were deposited in the Library of Göttingen,—a supposition also erroneous, as, on my arrival at Göttingen, I found they were at Frankfort, but could not learn with sufficient certainty in whose immediate possession, to induce me to change my plan of travel. On my return home, I wrote respecting them to Dr. Argelander of Bonn, who very obligingly communicated my wish to Dr. Wilhelm Sœmmerring, the son; and, by his intervention, the volumes were in the course of a few months transmitted to me in England, with the permission to retain them till I had extracted from them the desired information. My time and that of my assistant were so fully taken up in the preparation for the press of my Circumpolar Star Catalogue to the end of the year 1857, that I took no immediate steps with the volumes entrusted to me; and when that work was off my hands, it appeared to me to be desirable that the reductions of my own solar observations should first be brought up to date, and that the additional acquaintance with the subject which would then be gained would probably be of advantage in the proposed treatment of the older and less exact records. I was still thus engaged when, in July 1858, my astronomical plans were interrupted by an event which has been previously alluded to, and which prevented my doing more at the time than giving the Society an account in November 1858 of some results thus derived from my own records. In the past year, 1859, I was first able to make and to get made by Mr. Henry Criswick a transcript of Sœmmerring's diagrams, and subsequently to effect their reduction by the assistance of Mr. Carl Moritz von Bose. The foregoing expla-

nation of the long detention of his father's valuable MSS. is mainly due to the present Dr. Sœmmerring, and will, I hope, be pardoned by the readers of this paper.

Some account of Dr. Sœmmerring's observations to the year 1827 may be found in the tract above referred to, which was published at Frankfort on the occasion of Sœmmerring's attaining his Jubiläum, in which also are given two plates exhibiting the distribution of the spots during the last six months of 1826 and the first six months of 1827, and an account of the process of reduction followed by Dr. Thilo. He finds his results to be generally confirmatory of those of Lalande respecting the comparative paucity in the neighbourhood of the equator and the general limits north and south of the principal zones of action. But I do not find in this tract, which, indeed, was intended to be complimentary, any material addition to the subject, or any indication that a period of maximum energy was approaching, as the subsequent labours of Schwabe showed to have occurred. This publication gave rise to another by Dr. Schweigger of Halle in the year 1829, "Uber die Natur der Sonne mit Beziehung auf v. Sœmmerring's Sonnenbeobachtungen," but to which I need not now refer.

The records of Sœmmerring I found to consist of diagrams of the sun's disk, contained in two thin stitched books, apparently put together by himself, and two larger bound volumes, the drawings in which could scarcely be other than copies of those made at the telescope, to judge by the size of the volumes and the nicety of the execution of details. Excepting a few diagrams on a larger scale (and not always the same) taken principally in April, May, June, and July, 1826, and which I rejected entirely from the insufficiency or total want of indication of the direction of diurnal motion; the disk of the sun was uniformly represented by a circle of the diameter of 90 millimeters, crossed by two parallel vertical lines and two parallel horizontal lines, the intersection of which formed a square in the centre whose sides were each 30 millimeters, and 30 millimeters distant from the zenith, nadir, and E and W horizontal points. Fig. 2 of Table II. in Dr. Thilo's tract may be taken as a faithful specimen with the correction that for N and S (north and south) should be read N and Z (nadir and zenith), for the evidence of the record is conclusive that the upper point of the circle in all the drawings is the nadir point of the disk, allowing for such error as the representations are liable to. I conclude that the telescope employed had a motion round axes intended to be vertical and horizontal, and that in its field were cross-wires dividing the disk roughly into vertical and horizontal third parts, and that the positions recorded were estimated by the eye, with the assistance of the square of wires screwed approximately into the centre of the visible disk of the sun. Apart from the excessive trouble of this method of observation, especially when the sun is high in the heavens,

it is liable among other grave objections to this, that the fixed square divides the disk into inexact thirds by reason of the variation of the sun's diameter, even supposing the observer could, by the mechanism of the instrument, place it and keep it in the desired position. The errors of this class are, however, insignificant in comparison with those of estimation and of that of the north point deduced from the given nadir point and the time, for, during the two first years, the time of observation only purports to be given to the nearest hour or half-hour, and for the two last years to the nearest quarter. In naming this last defect as decidedly the weakest part of the records, and which leads me to doubt if a record of position was a material part of the observer's plan, I may name as a set-off that the records are for the most part so continuous, and the positions of the same spots on successive days so numerous, that a computer at once sees that the method of means will afford him considerable aid, and reduce the error of the concluded position of a group very materially below that of a single observation. This circumstance, in fact, principally induced me to undertake the reduction of these observations, coupled with the belief, founded on some research, that from the general inattention to the subject by more practised micrometrical observers I could not expect to find elsewhere any series offering more precision.

After rejecting about 80 diagrams in the first half of 1826, there remained suitable for my purpose about 80 others taken between August 3 and Dec. 31 of that year, and about 160 between January and July 31, 1827, after which, for a period of seven months, ending with Feb. 1828, no observations occurred. From the 1st of March, 1828, the record was resumed with increased care and with a little more precision in the recorded time of observation, and continued to December 31, 1829, each of the years 1828 and 1829 affording about 240 diagrams. Of these 720 disks in all, Professor Thilo's tract comprises the first third part only; and so far as I know no one previously to myself has instituted an examination of the last and more valuable portion, which happens to extend (as I have marked on the map) over a period of maximum frequency, as ascertained by Prof. Wolf of Zurich from the observations of Schwabe and Staudacher.

For the double reason that I decided on performing the reduction by graphical process, for which I had materials already prepared for disks of 12 inches, and that I desired to possess a duplicate of the records, I employed Mr. Criswick to copy the whole of the 720 drawings for me on a scale increased about 3·4 times from 90 millimeters to 12 English inches. This was done very expeditiously, in about three weeks, by the use of a pentagraph constructed to work in two different planes; and although the instrument employed, being of home-manufacture, did not possess perfect accuracy, its errors of indication were ascertained by trial, and so allowed for in transcription as not

to introduce any material additional error of position. The copies thus made are bound in three volumes, which were exhibited at the evening meeting of the Society in January 1860.

For the reduction by graphical process of the positions of the spots at any one time, three elements have to be computed.

1st. The angle from the zenith and nadir points given on the record, at which the projection of the sun's axis cuts the circle.

2d. The angle of inclination to the plane of the sun's equator of the line joining the centres of the earth and sun, which is sensibly the same as the direction of vision.

3d. The meridian on the sun, which at the time of observation passed through the centre of the disk.

When these elements are known, and a set of projections of the principal meridians and parallels of latitude has been previously prepared for different convenient values of the second element, the position of any spot for any day may readily be seen on inspection by superposing on the record at the suitable angle (1) the suitable projection (2), and reading off the longitudes by their differences from (3). Angle (1) is the sum (with the proper signs) of three other angles, S the angle at sun's centre between the pole and zenith of Frankfort, β the angle by which the earth's axis would appear inclined to a vertical on the ecliptic to an observer in the sun, and γ the angle by which the sun's axis would appear inclined to a similar vertical to an observer at the earth's centre. Of these β is readily computed from the sun's longitude as argument, and γ from the sun's longitude less that of the node of the sun's equator (the elements employed for the sun were $N=74^\circ\,8'$ and $I=7^\circ\,10'$); but S depends on the hour-angle or time of observation, and is liable to considerable error in certain positions from insufficient precision in this datum. Angle (2) readily follows from the element I and the argument $\odot - N$; and angle (3) is found from the given time by the synodic period of rotation (taken as 27·275 days) from any meridian taken as the primary. In my reduction the values at G.M. noon on Jan. 1 are, for 1827, .. 241° 27′; for 1828, .. 103° 52′; and for 1829, .. 313° 3′: and the following extract will show the process followed and the degree of accuracy considered sufficient:—

1828.	F.M.T.	λ	N.P.D.	$\odot$	$\odot - N$	β	γ	S
	h m	h m	° ′	° ′	° ′	° ′	° ′	° ′
Mar. 1	8 0	4 13 E.	97 33	340 42	266 34	+22 16	−0 27	97 9
2	12 0	0 12 E.	97 6	341 52	267 44	+22 25	−0 17	97 9
3	8 45	3 27 E.	96 47	342 44	268 36	+22 31	−0 10	97 10
4	10 0	2 12 E.	96 22	343 47	269 39	+22 37	−0 3	97 10
5	8 45	3 27 E.	96 1	344 44	270 36	+22 43	+0 4	97 10
6	16 0	3 49 W.	95 31	346 2	271 54	+22 50	+0 14	97 10

	Day.	S	ΔS	S+β+γ	Sheet.	M	Rotation.
1828.		° ′	′	° ′	° ′	° ′	
Mar. 1	60·309	−35 38	+ 41	−13 49	7 20	34 27	22
2	61·475	− 2 21	+114	+19 47	„	19 4	22
3	62·340	−31 55	+ 59	− 9 34	„	7 38	22
4	63·392	−23 1	+ 84	− 0 27	„	353 45	23
5	64·340	−32 1	+ 58	− 9 14	„	341 14	23
6	65·652	+34 5	+ 51	+57 9	„	324 3	23

In the above, h is the hour-angle from apparent noon at Frankfort; the numbers in column "Day" are the days of the year to three decimals, in which Frankfort times have been converted into Greenwich mean civil times; and the quantities "ΔS" are the increments of "S" for 10 minutes' increment of the time of observation. Similar quantities to the above were computed by Mr. von Bose, under my direction, for the whole of the 720 disks, and the results subsequently checked throughout.

In the application of these elements, it was a question in what way the errors of observation, individually large, and the error of North Point, should be treated. The application of equations of condition, or any elaborate treatment, was out of the question altogether in results of this character; and it appeared to me a method of approximation based on the general principle of taking means would best serve my purpose. Accordingly I made trial of the following scheme, of first drawing in lightly the projected principal meridians and parallels resulting from the given time strictly; then assembling all the individual positions of the same spots, and adopting mean places of the first approximation; next by comparing the positions of each day with the previously taken means to ascertain by what allowable alteration of the angle S the discordances of the day could most be reduced; and so for each day in succession: finishing by finally drawing in the principal meridians and parallels with the corrected values of S, and taking concluded mean positions of the spots and groups. In my own hands this method presented no difficulty; but I perceived that the concluded means, for the most part, differed so little from those of the first approximation when a little judgment was used in the rejection of any manifestly bad subject, that the labour of repetition was hardly compensated. In the hands of a comparatively unpractised assistant I found it, on the other hand, difficult to lay down satisfactory rules for his guidance in my absence from home; and, finally, I fell back to the principle of simply taking mean results, rejecting manifest errors, and depending as little as possible on positions near the limb, in which the errors of S had full effect. It is desirable that I should give a single example, in order to show the degree of uncertainty attaching to the concluded places.

		12	
	355	18	
	354	14	
	2	19	1
	357	21	2
	0	20	3
Mean	1	16 S.	2

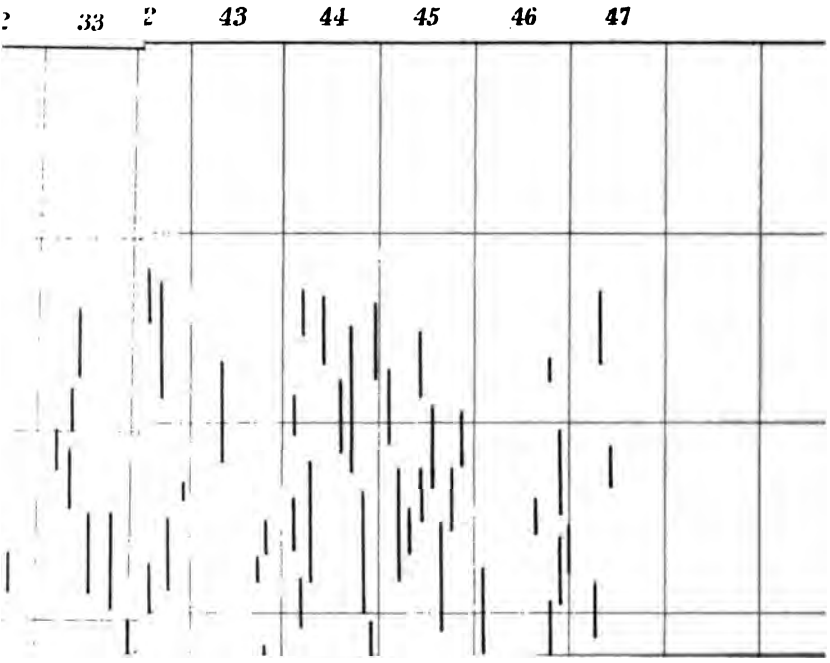
In this case no result was rejected a ordinary error, and though the process final result for the group to be nearly as that could be obtained by more refined me groups thus obtained were as concluded l of sheets (exhibited at the meeting) with cating the general limit of the observed each sheet comprising half a rotation of th

In the present state of the subject I do be justified in asking the Society to publis results *in extenso*, and I limit myself to ma results are so far extracted from the obs my possession and open for use to any stu whose researches they may be of service. give a graphical summary of the distributi four years, similar to that printed with November 1858. The distribution in lati of the last minimum presented a remarka liarity; and it was with some curiosity th years of the sun's history a quarter of a prising a period of maximum energy, I lai map to see if any peculi-

The pre-

from the at Frankfort.

y.



year 1858; but with this exception I now have before me reduced records of nearly 10 years of the sun's history without a similar or nearly a similar case; and I think I shall not be judged hasty in saying that I discredit the oft-quoted assertion that spots have been seen in a latitude of 70°, and that I believe the statement has arisen by error in reduction.

It was stated above that I sought for the records of Sœmmerring with the view of obtaining the possible means of arriving at a more correct value of the period of the sun's rotation; and I have now to add that hitherto I have been baffled in every effort to recognise any indications of true recurrence which might enable me to arrive at this much-desired result, and that against my will I seem almost forced to the conclusion that every meridian of the sun within the bounding latitudes is indifferently visited by these singular phenomena, still wanting an adequate theory. In common with many others I have sought to identify the recurrence of individual spots and groups by peculiarities of size or form, and have gone about the inquiry prepared for a very different true period than that at present assumed. Failing in this way, I have grouped the outbreaks of successive batches of 4 or 5 rotations, with the object of recognising any preponderance of action which might exist in any quadrant, or any absence of outbreaks in corresponding regions, or again any tendency in outbreaks to reach higher latitudes in some longitudes rather than in others on the whole; but as yet the clue fails me. The problem is undoubtedly of high importance for the physical theory of the sun; and though the siege operations may be protracted, I have no doubt that a conclusion will in due time be arrived at. It need hardly be pointed out that whenever an indication of recurrence shall be ascertained, the existence of a body of observations such as the present will enable any computer in an hour to step at once to a period of rotation true to several places of decimals of a day.

On the Latitude and Longitude of the Sydney Observatory. By W. Scott, Esq., Director of the Observatory.

"I send my determination of the latitude and longitude of the Sydney Observatory. The instrument employed is a meridian-circle by Jones, diameter of circle 42 inches, focal length of telescope 63 inches, aperture $3\frac{1}{4}$ inches.

"The circle has been recently repaired, considerably modified and redivided, by Messrs. Troughton and Simms. A comparison of the latitude results obtained from different stars will show how far the accuracy of its division can be relied on.

"A second report on the observatory is now in the press.

Latitude of Sydney Observatory.

Star.	No. of Obs.	Approx. Z.D. North.	Mean of Latitudes.	Greatest error from mean.
		° ′	° ′ ″	″
ε Bootis	3	64 51	33 51 41·16	2·4
α Bootis	7	53 46	42·07	2·6
η Bootis	9	52 58	42·45	2·8
α Herculis	7	48 25	39·25	3·9
α Serpentis	9	40 44	40·60	2·2
ζ Virginis	12	33 58	41·70	3·9
ϑ Ophiuchi	5	30 32	40·88	1·6
ι Virginis	8	29 4	40·82	1·5
β Libræ	4	24 59	40·82	1·5
α Virginis	9	23 26	42·15	2·3
α_2 Libræ	8	18 24	40·69	1·7
α Canis Major.	9	17 20	42·13	2·8
β_1 Scorpii	13	14 26	40·36	2·0
β Corvi	3	11 14	40·43	2·1
δ Ophiuchi	13	9 0	40·13	2·8
α Scorpii	9	7 44	41·52	3·1
		South.		
α Argûs	8	18 45	41·86	1·8
α Pavonis	7	23 19	41·05	1·1
β Centauri	8	25 50	42·48	3·0
β Chamæleon	4	44 40	40·85	2·6
β Hydri	4	44 10	43·22	0·8
α Triang. Aust.	13	34 54	41·29	2·5
σ Octantis	13	55 25	39·05	2·7
β Hydri S.P.	7	68 6	39·01	2·1

	° ′ ″
Mean of northern stars	33 51 41·07
Mean of southern	41·10
Adopted latitude	33 51 41·1

Longitude from Moon-Culminating Stars.

Limb I.

Assumed Approximate Longitude $10^h\ 4^m\ 50^s$

Date	1859, June 9	June 10	June 13
Stars	η Virginis q Virginis	α Virginis	ϑ Scorpii β_1 Scorpii
Observed R.A.	12° 15′ 40″·07	13° 5′ 39″·07	15° 45′ 2″·71
Computed R.A.	12 15 40 ·47	13 5 39 ·40	15 45 3 ·04
Difference	+ 0 ·40	+ 0 ·33	+ 0 ·33
Correction to Long.	+ 11ˢ·98	+ 9ˢ·76	+ 8ˢ·79

Date	June 14	July 7	July 8
Stars	ω Scorpii τ Scorpii	γ Virginis ψ Virginis	83 Virginis 89 Virginis
Observed R.A.	16° 41′ 51″·84	12° 49′ 42″·18	13° 40′ 48″·33
Computed R.A.	16 41 52 ·49	12 49 42 ·50	13 40 48 ·56
Difference	+ 0 ·65	+ 0 ·32	+ 0 ·23
Correction to Long.	+ 16ˢ·98	+ 9ˢ·41	+ 6ˢ·64

Date	July 9	11	13
Stars	·5 Libræ α_2 Libræ	σ Scorpii α Scorpii	θ Ophiuchi d Ophiuchi δ & λ Sagittarii
Observed R.A.	14° 33′ 17″·20	16° 23′ 26″·21	18° 16′ 54″·06
Computed R.A.	14 33 17 ·50	16 23 36 ·29	18 16 53 ·96
Difference	+ 0 ·30	+ 0 ·08	− 0 ·10
Correction to Long.	+ 8ˢ·39	+ 2ˢ·11	− 2ˢ·68

Date	August 6	August 11	August 12
Stars	20 & ι Libræ α Scorpii	h_2 Sagittarii f Sagittarii	f Sagittarii σ_2 Capricorni
Observed R.A.	15° 9′ 42″·75	19° 47′ 10″·62	20° 37′ 21″·99
Computed R.A.	15 9 43 ·27	19 47 10 ·49	20 37 21 ·81
Difference	+ 0 ·52	− 0 ·13	− 0 ·18
Correction to Long.	+ 14 ·05	− 3ˢ·76	− 5ˢ·49

Date	August 13	Sept. 7	Sept. 9
Stars	ι Capricorni γ Capricorni	σ Sagittarii χ_3 Sagittarii	θ Capricorni ι Capricorni
Observed R.A.	21° 24′ 58″·44	19° 29′ 37″·17	21° 9′ 5″·84
Computed R.A.	21 24 58 ·25	19 29 37 ·06	21 9 5 ·69
Difference	− 0 ·19	− 0 ·11	− 0 ·15
Correction to Long.	− 6ˢ·08	− 3ˢ·13	− 4ˢ·72

Date	Oct. 5	Oct. 7	Oct. 8*
Stars	ω Sagittarii A Sagittarii	θ Capricorni γ Capricorni	θ Aquarii
Observed R.A.	20° 1′ 55″·89	21° 38′ 15″·61	22° 23′ 30″·79
Computed R.A.	20 1 55 ·85	21 38 15 ·49	22 23 30 ·31
Difference	− 0 ·04	− 0 ·12	− 0 ·48
Correction to Long.	− 1ˢ·17	− 3ˢ·88	−15ˢ·95

Date	Oct. 9*	Oct. 10*	Oct. 11*
Stars	κ Piscium	κ Piscium 22 Piscium	ω Piscium
Observed R.A.	23° 7′ 59″·62	23° 52′ 42″·05	0° 38′ 41″·06
Computed R.A.	23 7 59 ·19	23 52 41 ·86	0 38 41 ·31
Difference	− 0 ·43	− 0 ·19	+ 0 ·25
Correction to Long.	−14ˢ·37	− 6ˢ·24	+ 7ˢ·90

* Assumed Longitude $10^h\ 5^m\ 0^s$

Limb II.

Date	June 15	June 20	June 21
Stars	θ Ophiuchi d Ophiuchi	μ Capricorni	67 Aquarii λ Aquarii
Observed R.A.	17° 41′ 31″·57	21° 57′ 45″·54	22° 42′ 21″·41
Computed R.A.	17 41 32 ·26	21 57 45 ·59	22 42 21 ·46
Difference	+ 0 ·69	+ 0 ·05	+ 0 ·05
Correction to Long.	+ 18ˢ·63	+ 1ˢ·64	+ 1ˢ·67

Date	June 22	July 18	July 22
Stars	π Piscium λ Piscium	σ Aquarii π Aquarii	η Piscium
Observed R.A.	23° 26′ 31″·55	22° 26′ 58″·50	1° 28′ 2″·12
Computed R.A.	23 26 31 ·50	22 26 58 ·81	1 28 2 ·83
Difference	— 0 ·05	+ 0 ·31	+ 0 ·71
Correction to Long.	— 1ˢ·67	+ 10ˢ·39	+ 21ˢ·35

Date	July 23	Aug. 17	Aug. 19
Stars	α Arietis η Arietis	45 Piscium	α Arietis
Observed R.A.	2° 19′ 44″·61	0° 25′ 22″·79	2° 1′ 20″·50
Computed R.A.	2 19 45 ·17	0 25 23 ·15	2 1 21 ·22
Difference	+ 0 ·56	+ 0 ·36	+ 0 ·72
Correction to Long.	+ 14ˢ·92	+ 11ˢ·79	+ 20ˢ·89

Date	Aug. 21	Aug. 22	Sept. 13
Stars	η Tauri A_1 Tauri	β Tauri	45 Piscium
Observed R.A.	3° 53′ 16″·04	4° 56′ 16″·28	0° 11′ 6″·89
Computed R.A.	3 53 17 ·02	4 56 17 ·30	0 11 7 ·36
Difference	+ 0″·98	+ 1 ·02	+ 0 ·47
Correction to Long.	+ 24ˢ·23	+ 23ˢ·65	+ 15ˢ·43

Date	Sept. 17	Sept. 18	Sept. 19
Stars	17 Tauri	ν^1 Tauri	β Tauri ζ Tauri
Observed R.A.	3° 34′ 22 ′·61	4° 34′ 49″·00	5° 38′ 26″·76
Computed R.A.	3 34 23 ·37	4 34 49 ·99	5 38 27 ·73
Difference	+ 0 ·76	+ 0 ·99	+ 0 ·97
Correction to Long.	+ 19ˢ·51	+ 23ˢ·90	+ 22ˢ·58

Date	Sept. 20	Oct. 12 *
Stars	μ Geminorum	ε Piscium η Piscium
Observed R.A.	6° 43′ 22″·66	1° 29′ 11″·10
Computed R.A.	6 43 23 ·64	1 29 11 ·71
Difference	+ 0 ·98	+ 0 ·61
Correction to Long.	+ 22ˢ·74	+ 18ˢ·20

* Assumed Longitude $10^h\ 5^m\ 0^s$

Mean of results	Limb 1	+ 3'.26	21 observations
	Limb 2	+ 16'.46	17 ,,
	Mean	+ 9'.86	

Resulting Longitude $10^h\ 4^m\ 59^s.86$

Apparent Right Ascension of the Moon's Limb, and of Moon-culminating Stars, observed with the Transit Instrument of the Observatory, Washington.

(*Communicated by Lieut. Gillis.*)

Date.	Name of Object.	No. of Wires.	Apparent R.A. Observed.	Apparent R.A. Computed.
1859.			h m s	s
Aug. 6	☽'s I. Limb	25	15 45 2·915	3·01
	δ Scorpii	15	52 3·338	
	β' Scorpii	25	57 17·806	
	σ Scorpii	25	16 12 40·854	
	α Scorpii	25	20 49·610	
	τ Scorpii	15	27 10·213	
8	A^1 Ophiuchi	15	17 6 44·566	
	θ Ophiuchi	25	13 24·977	
	d Ophiuchi	25	18 25·192	
	C^2 Ophiuchi	15	22 52·715	
	☽'s I. Limb	25	38 27·997	28·37
	4 Sagittarii	15	51 15·009	
	γ Sagittarii	15	56 49·283	
	μ' Sagittarii	25	18 5 23·898	
10	φ Sagittarii	25	36 55·067	
	σ Sagittarii	25	46 35·535	
	τ Sagittarii	15	58 12·453	
	χ' Sagittarii	15	19 16 45·835	
	☽'s I. Limb	25	28 7·479	7·74
	f Sagittarii	15	38 12·317	
16	ι Piscium	25	23 32 45·666	
	21 Piscium	25	42 18·107	
	☽'s II. Limb	25	0 8 53·401	53·37
	d Piscium	25	13 24·287	
	45 Piscium	25	18 29·507	
17	d Piscium	25	13 24·164	
	45 Piscium	25	18 29·423	
	δ Piscium	15	41 25·667	
	☽'s II. Limb	25	54 33·444	33·42
	e Piscium	15	1 1 9·840	

Date. 1869.	Name of Object.	No. of Wires.	Apparent R.A. Observed. h m s	Computed.
Aug. 17	η Piscium	25	1 23 59·843	
	ϖ Piscium	15	29 40·888	
18	η Piscium	25	23 59·921	
	ϖ Piscium	25	29 40·936	
	☽'s II. Limb	25	42 48·392	48·40
	β Arietis	15	46 54·733	
	ι Arietis	15	49 42·405	
	α Arietis	25	59 17·147	
Sept. 2	☽'s I. Limb	25	15 23 47·935	48·10
	δ Scorpii	15	52 2·924	
	β' Scorpii	25	57 17·471	
	α Scorpii	25	16 20 49·127	
3	δ Scorpii	15	15 52 2·889	
	☽'s I. Limb	25	16 21 13·021	13·09
	τ Scorpii	10	27 9·954	
6	ϕ Sagittarii	25	18 36 54·665	
	29 Sagittarii	15	41 21·803	
	σ Sagittarii	25	46 35·339	
	ζ Sagittarii	15	53 42·415	
	☽'s I. Limb	25	19 10 14·053	14·51
	χ^3 Sagittarii	25	17 1·566	
	h^3 Sagittarii	25	28 11·573	
7	h^3 Sagittarii	25	28 11·646	
	f Sagittarii	15	38 12·137	
	c Sagittarii	15	54 3·410	
	☽'s I. Limb	25	20 2 18·750	19·14
	σ Capricorni	25	11 19·438	
	ϱ Capricorni	15	20 53·020	
8	σ Capricorni	25	11 19·485	
	ϱ Capricorni	25	20 53·106	
	ν Capricorni	25	32 5·453	
	☽'s I. Limb	25	51 39·785	40·01
	θ Capricorni	15	58 5·333	
	ν Aquarii	15	21 1 58·832	
	ι Capricorni	25	14 27·837	
9	θ Capricorni	15	58 5·291	
	ι Capricorni	25	14 27·818	
	γ Capricorni	15	32 20·846	
	☽'s I. Limb	25	38 37·681	37·92
	μ Capricorni	25	45 40·638	
	ι Aquarii	25	58 53·406	
	θ Aquarii	25	22 9 27·663	

Date. 1859.	Name of Object.	No. of Wires.	Apparent R.A. Observed. h m s	Computed.
Sept. 10	ϑ Capricorni	25	21 39 19·668	
	ι Aquarii	15	58 53·439	
	θ Aquarii	25	22 9 27·603	
	☽'s I. Limb	25	23 51·791	52·00
	κ Aquarii	15	30 31·320	
	λ Aquarii	25	45 19·592	
12	γ Piscium	25	23 9 55·464	
	κ Piscium	25	19 46·392	
	ω Piscium	15	52 8·489	
	☽'s II. Limb	25	54 38·876	38·93
	d Piscium	25	0 13 24·645	
	45 Piscium	25	18 29·967	
	12 Ceti	25	22 54·626	
13	35 Piscium	15	7 47·232	
	d Piscium	25	13 24·672	
	45 Piscium	25	18 29·907	
	☽'s II. Limb	25	40 8·067	8·14
	ε Piscium	25	55 41·709	
18	υ′ Tauri	25	4 17 56·268	
	τ Tauri	25	33 50·787	
	ι Aurigæ	25	47 52·899	
	ι Tauri	15	54 43·778	
	☽'s II. Limb*	25	5 14 53·466	52·83
	β Tauri	15	17 26·502	
	ζ Tauri	25	29 16·500	

The observations of September 2d were made by Professor Major; the others by Professor Yarnall.

The computed place of the moon is taken from the American Ephemeris.

December 12, 1859.

Results of Meridional Observations of Small Planets; Occultations of Stars by the Moon; and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

Leda (38).

Mean Solar Time of Observation. h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1859, Nov. 1 11 59 14·4	2 42 11·39	62 35 42·17

* Moon's limb very jagged.

Hebe (6).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, Nov. 9	10 18 18·1	1 32 30·96	108 10 26·90
15	9 52 32·2	1 30 20·18	107 46 34·94
19	9 35 54·8	1 29 26·23	107 22 55·71
21	9 27 46·6	1 29 9·85	107 9 0·35
Dec. 1	8 48 55·5	1 29 37·89	105 40 39·90
5	8 34 13·6	1 30 39·85	104 58 1·34
7	8 27 2·8	1 31 20·97	104 35 23·33
8	8 23 30·4	1 31 44·53	104 23 48·37
13	8 6 11·6	1 34 5·73	103 23 0·12
21	7 39 50·3	1 39 12·52	101 38 17·89

Parthenope (11).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, Nov. 15	13 9 34·0	4 47 54·35	74 4 39·04
21	12 40 13·3	4 42 8·20	74 14 3·68
30	11 55 32·1	4 32 48·61	74 25 56·87
Dec. 5	11 30 39·6	4 27 34·80	74 30 51·13
7	11 20 45·0	4 25 31·72	74 32 25·27
8	11 15 48·8	4 24 31·28	74 33 6·89
19	10 22 24·6	4 14 20·45	74 35 42·96
28	9 40 35·4	4 7 53·41	74 30 24·14

Polyhymnia (33).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, Nov. 21	12 4 45·2	4 6 34·22	66 25 58·27
Dec. 5	18 55 37·4	3 52 26·86	67 2 6·62
28	9 10 18·1	3 37 31·17	67 54 49·40

Alexandra (54).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, Dec. 27	10 23 57·0	4 47 25·59	54 6 16·15

Mnemosyne (57).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1859, Nov. 18	7 56 26·4	23 45 44·98	89 16 13·24

Eclipses, Occultations, and Transits of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
1859. Nov. 13	I	Ecl. disapp.	11 51 47·6	C.
15	I	Occ. reapp. (*a*)	9 46 50·7	C.
21	I	Ingress, 1st cont. (*b*)	12 1 2·4	D.
21	I	,, central bis. (*b*)	12 3 2·1	D.
21	I	Egress, central bis. (*b*)	14 21 39·3	D.
21	I	,, last cont. (*b*)	14 24 38·8	D.
24	III	Ecl. disapp. (*c*)	9 4 11·7	T.
24	II	Egress, last cont.	9 28 44·2	T.
Dec. 1	II	,, central bis. (*b*)	11 45 58·0	D.
1	II	,, last cont. (*b*)	11 47 57·7	D.
7	IV	Ingress, central bis.	11 6 36·6	M. D.
7	I	Egress, central bis. (*d*)	12 23 24·0	M. D.
8	I	Occ. reapp. central bis. (*f*)	9 30 26·8	C.
8	I	,, last cont. (*f*)	9 31 56·7	C.
8	II	Ingress, total ingr.	11 19 39·0	C.
8	II	Egress, last cont.	14 4 12·1	C.
15	I	Occ. reapp. first cont.	11 13 46·3	M. D.
15	I	" last cont.	11 15 31·0	M. D.
15	II	Ingress, first cont.	13 28 54·1	M. D.
15	II	,, last cont.	13 34 23·2	M. D.
15	IV	Ecl. disapp.	13 38 58·5	M. D.
21	I	Ingress, first cont.	13 30 0·4	M. D.
21	I	,, central bis.	13 32 59·9	M. D.
21	I	,, last cont.	13 36 59·2	M. D.

(*a*), Planet very low and extremely tremulous; time uncertain to 2 or 3 minutes. (*b*), Unsatisfactory; planet very tremulous. (*c*), *Jupiter* very low; satellite exceedingly faint. (*d*), Cloudy; very unsatisfactory. (*f*), Image of *Jupiter* extremely bad.

The initials D., C., M. D., and T., are those of Mr. Dunkin, Mr. Criswick, Mr. Dolman, and Mr. Talmage.

Occultations of Stars by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
1859, Nov. 8	101 Piscium disapp.	(*a*) Bright.	9 54 59·0	J.C.
15	ϑ Cancri disapp.	(*b*) Bright.	18 14 45·8	T.
15	ϑ Cancri reapp.	(*c*) Dark.	19 23 55·7	T.

(*a*) Not very accurate; star faint.
(*b*) Somewhat doubtful, owing to a passing cloud.
(*c*) Satisfactory.

The initials J.C. and T. are those of Mr. Carpenter and Mr. Talmage.

The observations of the occultation of the *Pleiades* by the moon, on December 8, have been already published in the preceding number of the *Monthly Notices*.

Comparison of the Probable Error of a Transit of a Star Observed with the Transit-circle by the "Eye and Ear" and Chronographic Methods. By Edward Dunkin, Esq., of the Royal Observatory.

The probable error of a transit of a star has frequently been considered of sufficient interest to occupy the attention of astronomers, some of whom have discussed the subject in considerable detail, generally with the desire of testing the accuracy of meridional observations; but in the present communication it is not my intention to enter very fully into the question, or to offer any critical remarks on the Greenwich transits, but solely to confine myself to an investigation of the relative difference between the original method of observing transits and that which has superseded it since the introduction of galvanism as an aid in the registration of astronomical observations. The transits made at the Royal Observatory with the transit-circle furnish ample materials for such an investigation.

In these reductions I have, for the comparison, used two methods; that which gives the more direct result will be explained first. In this method a certain number of stars were selected without any partiality, whose total number of observations in each series amounted to about one hundred. The separate complete transits were then extracted from the *Greenwich Observations* for 1853, this being the last year of observation by "eye and ear," and from the volume for 1857 for the transits recorded by the chronographic method, taking care in each case to reject all when any wires had been omitted. Each transit was then treated as follows,—for the observations made in 1853, to the clock-time observed at each separate wire, has been applied the value of the interval, expressed in time, between that wire and the mean of the complete system of wires; and for those made in 1857, to each recorded clock-time has been applied the interval between the wire observed and the centre-wire, according to the values published in the *Introduction to the Greenwich Observations* for the respective years. Each clock-time is thus reduced to a mean, and ought, supposing no error to exist, to consist of a series of numbers absolutely identical. This theoretical perfection is never practically the case, therefore for our purpose it is necessary to find the difference between each individual observation and the mean of the whole series, thus obtaining a number of errors equal to

the number of wires observed. The square of each error was then taken, and the remainder of the reduction treated for each observation according to the formulæ explained in considerable detail by M. Liagre in his *Calcul des Probabilités et Théorie des Erreurs*, Sections 105 to 113.

The following are the results obtained:

Transits observed by "Eye and Ear."

		No. of Obs.	Probable uncertainty of result.
Probable error of an observation at one wire	$= \pm 0^s.074$	707	$\pm 0^s.001$
,, of a complete transit	$= \pm 0^s.028$	101	$\pm 0^s.001$

Transits recorded by the Chronographic Method.

		No. of Obs.	Probable uncertainty of result.
Probable error of an observation at one wire	$= \pm 0^s.051$	909	$\pm 0^s.001$
,, of a complete transit	$= \pm 0^s.017$	101	$\pm 0^s.001$

It must be borne in mind that the probable error in this instance consists of a combination of the error of observation with the error of the determination of the intervals of the wires, though there is every reason to believe that the effect of the latter is nearly, if not altogether, insensible.

In the second investigation, I have taken the observation of every star whose N.P.D. is greater than 50°, which had been observed at least three times during the year over the whole number of wires. The separate results are given in the *Greenwich Observations* for 1853 and 1857 respectively. The limit of 50° was adopted in consequence of the scarcity in 1853 of complete observations of stars having less N.P.D.; and as the object of this paper is simply a comparison of the two observing methods in *ordinary* transits, it was considered prudent to commence from that point.

The process of reduction in this determination is slightly different from the preceding. In the section of the *Greenwich Observations* from which the numbers are extracted, the right ascensions are reduced to January 1 in each year. These results are affected by error of observation, errors of the position of the instrument, error of the correction applied to each observation on account of clock-error, &c. There are therefore so many errors affecting the result in this determination, which have no place in the preceding, that it would be impossible to discover what amount is due to the observer. But in an instrument of such stability as the transit-circle, it may be assumed that the instrumental errors in different years are determined with equal accuracy; consequently the difference obtained between the "eye and ear" and chronographic methods

from this investigation may be considered as representing the superiority of one method over the other.

The method of reduction consists in first taking a mean of all the observations for each star, then the difference of each observation for the mean, giving a series of errors equal to the number of observations; these errors being squared were treated by the same formulæ as previously mentioned. The results are given in the following table:—

N.P.D.	Probable Error of each Observation. Eye and Ear.	Chrono-graphic.	Number of Observations. Eye and Ear.	Chrono-graphic.	Probable Uncertainty of each Result. Eye and Ear.	Chrono-graphic.
° °	s	s			s	s
50 to 60	0·044	0·037	46	61	±0·003	±0·002
60 to 70	·049	·035	237	165	± ·002	± ·001
70 to 80	·051	·029	313	210	± ·001	± ·001
80 to 90	·049	·032	312	278	± ·001	± ·001
90 to 100	·046	·032	169	122	± ·002	± ·001
100 to 110	·048	·035	106	167	± ·002	± ·001
110 +	·050	·043	76	189	± ·003	± ·001
Means	0·048	0·034	1259	1192	±0·0007	±0·0005

By inspection of the preceding table, the general agreement of the resulting errors at different N.P.D. is easily seen, and tends to give some degree of confidence to the mean result. The comparatively large error in the chronographic method obtained from observations of stars below 110°, is probably on account of an unusual number observed in 1857 near the south horizon for the determination of the constant of refraction. These objects are often seen with considerable difficulty, and their images under the best circumstances are generally very diffused.

The result of this investigation may be stated as follows:—For stars whose N.P.D. is greater than 50° in a transit over all the wires observed by "eye and ear," the probable error is ±0ˢ·028, and by the chronographic method ±0ˢ·017; and in a transit observed by "eye and ear" combined with the instrumental errors, &c., the probable error is ±0ˢ·048, and by the chronographic ±0ˢ·034, the differences of 0ˢ·011 and 0ˢ·014 respectively being in favour of the chronographic method.

Greenwich, 1860, *January* 4.

Suggestions connected with the Carrington-and-Hodgson Solar Phenomenon of 1st Sept. 1859. By Prof. C. Piazzi Smyth, Astronomer Royal for Scotland.

At the conclusion of the very interesting account, given at pp. 24–26, in the *Monthly Notices* for November, of M. Le

Verrier's late remarkable investigations, resulting in a high probability for the existence of one large planet, or a zone of small ones, revolving round the sun at rather less than half the mean distance of *Mercury*,—a hint is appended, that with the above may be usefully read, "Prof. W. Thomson's ideas respecting a zone of meteors in the neighbourhood of the sun, thrown out as a possible source of supply of the solar heat and energy, in a paper published in the *Philosophical Magazine*."

It is, however, proper to remark, where so valuable a thing as an original idea from the brain of man is concerned, that the first suggestion of the meteoro-dynamic origin of solar light and heat, was made by Mr. Waterston. His idea was afterwards taken hold of by Prof. W. Thomson, and worked out by him and extended in the most admirable manner, with invaluable numerical conclusions, in a paper first published to the world in the *Transactions of the Royal Society of Edinburgh*, which therefore should rather be quoted than the magazine in London which merely copied the printed article.

I trust that, without incurring a charge of needless criticism, I may express a doubt whether that solar-heat theory has any particular connexion with the results of Le Verrier's researches.

It is quite true, that upon the dynamical explanation of heat a planet or a planetoid falling into the sun, would, from the conversion of its previous mechanical energy, give out an awful blaze of light, such a blaze as, according to Prof. W. Thomson, would certainly scorch up in a moment all the inferior planets, and probably the earth also with everything upon it.

But until the planet does so fall, or approach so near the sun as to have its orbital motion mechanically interfered with by resistance of the solar atmosphere (supposed, by the way, to extend very little beyond the actual surface), no dynamic heat would be given out.

The question, therefore, of the connexion of Le Verrier's zone of asteroids with Waterston's and W. Thomson's meteoric solar fuel, resolves itself into as the first step,—how soon will the mean heliocentric distance of the former, at present some 18 millions of miles, be reduced to half a million only?

Now so far as the results of physical astronomy, founded on the theory of gravitation alone, may speak, supported too by observation on all the known planets, no such decrease will ever take place; for all the comparatively small periodic perturbations are secularly restored. And though from other branches of science we may conclude the existence of causes which must introduce a new element of change into the calculations; and in the end, even cause all the planets to fall successively to the sun,—yet, the interval of time that must elapse before the first of these casualties can occur, is so vast, as, for all practical purposes, to be infinity itself; and similar

periods will separate the falls of all the other planets, as they come in, in their order. Hence it is surely obviously impossible for Le Verrier's supposed zone of asteroids (planetoids?) with a mean distance still of not less than half that of *Mercury*, to contribute towards solar heat, before countless ages shall have passed away. Not therefore from them, or any succession of *planet*-falls, as that word is usually understood, can come the nearly constant amount of light and heat which the sun has thrown out during the whole human period on earth, without a single moment's cessation, up to and including the present time. And it cannot even arise from a "zone" of meteors (*i.e.* meteoric stones) as long as their mean distances from the sun are anything at all approaching the planetary radii vectores.

What is wanted for keeping up, according to the dynamic theory, the observed amount of regular solar light and heat, is an infinity of meteors, with their orbits at all distances, and specially increasing in number with proximity to the sun, up to his very surface; as indeed we may almost fancy we can see them, on making an attentive examination of the steady increase in density of the zodiacal light, with every step of its apparent approach to the solar centre.

I am aware that some writers, even a few observers also, speak of the "ring" of the zodiacal light, but it has never so appeared to me; and I searched in vain when on the Peak of Teneriffe in 1856, for any symptom that could be so construed. Everything, on the contrary, tended so certainly to a continual increase in density, with every approach to the solar neighbourhood, (the opinion also, I believe, of so eminent an authority as Sir John Herschel,) that the frequent remarks met with in books, of the "ring" of the zodiacal light, appear to me to depend on some peculiar definition of the word quite different from the usual one, and therefore not imposing, as otherwise would be the case, a necessity for rejecting the idea of the zodiacal light being a visible representation of the present supply, and earnest of regular future supply, of meteoric heat-material to the sun.

Taking for granted, however, that the sun is fed by meteors, from wherever derived, partly in solid lumps of meteoric stone, as Mr. Waterston imagined, and partly by the same distilled away into smaller particles, as added by Prof. W. Thomson,—does not such an idea indicate a very possible interpretation of that strange solar phenomenon so fortunately witnessed, and well observed, by Mr. Carrington, and also by Mr. Hodgson, on Sept. 1st, 1859? Or, taking their observation (*Monthly Notices*, pp. 13–15) as our standing point, does not that seem greatly to confirm the Waterston and W. Thomson theory?

It appears, indeed, from what is printed in the *Notices* quite certain, that two bodies, elongated in the direction of their motion, moved rapidly along above the actual surface of the sun, suddenly bursting into view by means of their emitted light,

and then dying gradually out after having described an arc nearly parallel with the ecliptic, and at the rate of about 7000 miles in a minute.

Now here we have a velocity such as nothing we know of, short of orbital motion in the neighbourhood of the sun, can give. But, as in the immediate neighbourhood of the sun, or close to his surface, where we may presume the observed bodies to have been, the true orbital velocity is about 16,000 miles in a minute, we have a destruction, from some mechanical resistance, of 9000 miles of motion in a minute; which multiplied into the mass of the bodies so retarded, would at once indicate the heat they ought to have given out; something so great, doubtless, as to be necessarily accompanied by the intense light which was testified to by either gentleman.

In all this we may see, *mutatis mutandis*, precisely what has often been observed on the earth, when a meteorite, in the act of falling to it, shines out brilliantly in the clear blue sky at noon-day, like a speck of the sun itself; and describes thus luminously, in our upper atmosphere, the last part of its orbital motion.

Not the most ardent admirer of the theory had once apparently expected that human eye would ever be able to distinguish an individual meteor falling into the sun, and visibly adding to his fires; yet here does really seem to have been an instance. That it was so remarkably visible, evidently implies that its size must have been immensely greater than the average supply according to Waterston; or that, by its superior mass, it had successfully resisted Prof. W. Thomson's process of distillation of its material by heat, while revolving for ages in near proximity to the sun. This, however, presents no natural difficulty, when we judge of the meteorites that fall into the sun by those which fall to the earth; for numerous records show, that while the generality of the latter class of meteoric stones measure but a few inches, a particular one seen by many able observers in Scotland, England, and France, on August 18th, 1783, was estimated at more than half a mile in length.

There is still another point of view from which we may regard Messrs. Carrington and Hodgson's unique observation; for we have now, by its means, been made aware of a distinct class of solar phenomena, hitherto quite unexpected, and to the detection or record of which the usual methods of observing are accordingly not the most efficient that can be imagined; should not therefore something be done to follow up the opening thus made by an almost providential occurrence of circumstances?

A communication has been received from M. Chacornac, "Note sur l'intensité lumineuse du centre du Soleil comparée à celle des bords." Part of the note has been published in the *Comptes Rendus* of the 21st Nov. 1859, but the author's observations have been carried on to a later date, and the results of the entire series, extending from the 12th May to the 11th Dec. 1859, are as follows,—

Dist. from Circum.	Azimuths.	Ratio of Intensities.	Primitive Intensities.
	° ′		
1·000	34 11	0·461	1·000
0·717	34 11	0·461	1·000
0·483	35 05	0·494	0·935

where the last column, containing the deduced values of the primitive intensities, shows that the central part of the sun's disk has a sensibly uniform brightness for a distance from the centre of about three tenths of the radius, and that the brightness of the centre surpasses by about one fifteenth part of itself, that of a point at a distance from the centre of fifty two hundredth parts of the radius. The author shows that these results as to the diminution of the brightness are consistent with those obtained by supposing that the sun (assumed to be of uniform brightness) is surrounded by an atmosphere, which treated as homogenous would be of a thickness $= 0{\cdot}55$ of the radius, and such that the coefficient of extinction P is $= 0{\cdot}511$, viz. brightness at centre: brightness at any other point $= P : P^{\iota}$ if ι, ι, denote respectively the thickness of atmosphere traversed by the ray at the centre and by the ray at the other point.

The President announced, at the December meeting, that Mr. Grant, having been appointed to the Chair of Astronomy in the University of Glasgow, would be compelled to resign the editorship of the *Monthly Notices*. He was happy to say that Mr. Arthur Cayley had consented to undertake the editorship; and whilst he congratulated the Society on their periodical being continued by so distinguished a man as Mr. Cayley, he thought that the Society would feel pleasure in acknowledging their high appreciation of Professor Grant's services for the last seven years, during which period the journal had increased in value and popularity. Mr. May followed, at the suggestion of the President, and proposed that the thanks of the meeting be given to Professor Grant. This motion was seconded by the Treasurer, and carried by acclamation.

The Approaching Solar Eclipse.

(*Letter from Dr. Lamont to the Astronomer Royal.*)

"Having just received the *Monthly Notices* of the Astronomical Society, I find that at the last meeting you called the attention of the Society to the eclipse of the sun on the 18th of July, adverting to the necessity of previously collecting information about the places that might be selected for observing the eclipse. Now, as I myself visited in 1857 nearly all the places within the band of total obscuration, I thought it might, perhaps, be useful to communicate the following particulars:—The place most easily arrived at is Santander, there being between that place and Nantes a regular communication by steam-vessels. At Santander there is only one good inn, kept by a Frenchman, and generally crowded with travellers during the hot season, when the people of Madrid and other principal towns in the interior are accustomed to resort to the sea-coast. From Santander there is a railroad to Reynosa (where, I was told, there is tolerable accommodation for travellers), and a small steam-boat goes every week to Portugalete and Bilboa. Both places would be proper stations for observing the eclipse; and the inn at Bilboa will be found sufficiently comfortable. From Bayonne there is a very good road to Vitoria and Burgos, and the diligence that goes to Madrid passes there every day; the only difficulty is to get a place in the diligence. A traveller may have to stay at Bayonne at least eight days in July, and three weeks in September, to get a good place inside. At Vitoria there is a good inn, and it will be possible at all times to find accommodation there. In Spain the climate of Vitoria is accounted cool,—which only means that it is less hot than at Madrid. From Bayonne there is another road to Madrid passing through Pamplona and Tudela, both in the band of total obscuration. This road is less frequented, and it will, generally, be found much easier to get to Pamplona than to Vitoria. A kind of omnibus goes daily from Pamplona to Logroño, and another from Logroño to Vitoria. Logroño must be very near the central line of the eclipse. The inn at Pamplona cannot be said to be comfortable, and that at Logroño still less.

"Those who intend to station themselves further to the south ought to take the way of Barcelona. There are French and Spanish steam-vessels to convey passengers from Marseilles to Barcelona, and a diligence passes from Perpignan across the Pyrenees. From Barcelona it is easy to get by the diligence (if the railroad that was constructing in 1857 is not yet finished) to Saragossa and Calatayud (very near the central line). At Saragossa there is excellent accommodation for travellers; the inn at Calatayud is a very inferior one. From

Barcelona steam-vessels are going regularly to Valencia, and from thence there is a diligence to Castellon de la Plana and Oropesa. The two latter places are near the central line; Valencia (according to Mr. Encke's calculation) lies near the western extremity of the band of total obscuration. At Valencia travellers will find excellent accommodation; as for the two other places I can give no account of them, not having visited them myself.

"My own intention is to take up my station at Oropesa or Castellon de la Plana. The heat will be excessive, but there is no fear of the observation being frustrated by clouds. Now and then some clouds may be seen in the morning near the horizon, but they are always dissolved by the sun as soon as it attains a considerable altitude. If the cholera should break out in the south, as it did last year, I will probably go to Catalayud. I prefer a station in the south, because it is to be expected that the greater number of foreign astronomers will resort to the northern stations to avoid the heat: besides, I conceive that the climate of the south, near the sea-coast, will be especially favourable for deciding the points that I wish particularly to attend to. You may have seen the explanation of the red prominences that I have given in my 'Jahresbericht' for 1854 and 1858: I suppose them to be produced by thin clouds or masses of vapour floating in our atmosphere, and condensed by the depression of temperature in the shadow of the moon. All the phenomena that have been recorded seem to me to agree perfectly with this explanation, and I have no doubt that in a hot climate, where the air contains a great quantity of vapour, it will be possible, by using a telescope of small magnifying power and considerable aperture, to trace these clouds to a much greater distance than has been done yet, and to prove by their motion and changes that they do not belong to the sun."

The following account of the new equatoreal at the Royal Observatory, Greenwich, is extracted from the Report of the Astronomer Royal read at the Annual Visitation, 1859, June 4:—

"The new south-east equatoreal is now far advanced, and though I am not yet able to exhibit it in a complete working state, I trust to be able, by referring to the parts or models of parts which are visible, and by description of the principles which have guided their introduction, to make it generally in-

telligible. It is known to the Visitors that on three occasions of erecting large equatoreals which were entirely intrusted to me, namely, at Cambridge, at Liverpool, and at Greenwich, I have thought it my duty carefully to examine the construction of the three principal classes of equatoreals (the old English or Shuckburgh's, the long polar axis with telescope on one side, and the German form). And I have never put out of sight the consideration that, in observatories like this, an equatoreal cannot be considered as a mere commodious support for a gazing-telescope, but must be regarded as a firm graduated instrument, with which right ascensions and polar distances can be determined with considerable accuracy. Remarking, then, the diameters of the graduated circles, but still more the diameters of the racked-circles, and the clamp-circles on which the steadiness of clock-movement or the firmness of clamps must depend; remarking also the engineering objections, as regards weakness and friction, to the support of an instrument which projects beyond its two bearings; I prefer the English form, and I have accordingly adopted it here, with no novelty of principle, except that the declination-axis is so advanced in front of the centre of the polar-axis, and the upper part of the polar frame is so cut away, that the telescope commands the meridian without interruption to and a little way beyond the pole. Each cheek of the polar-axis is a skeleton prism; and, as I have shown in the description of the Northumberland telescope, this form is braced against torsion if each of its slides is braced. The bracing here is effected by a series of diagonal tension-bars and transversal thrusting-bars (each being drawn or thrust by its appropriate screw-motion); the long pillars and the bars being of wrought-iron. The upper and lower ovals which carry these are of cast-iron. On the spindle of the lower oval turns freely the hour-wheel, 6 feet in diameter, on which the clock-movement acts: which hour-wheel can be clamped at pleasure to the oval, and can also (when transits are required with an unmoved telescope) be clamped to the foundation-plate. A fixed microscope views a graduation of the hour-wheel, which, in its ordinary motion, will always exhibit in the microscope the sidereal time: and a microscope carried by the lower oval views another graduation, which in the same circumstances will exhibit the right ascension of the celestial body under view of the telescope. These microscopes are not directed perpendicularly to the graduated limb, but at an angle of nearly 10°, the illuminator throwing its light at the same inclination on the opposite side of the normal to the limb, so that the light is specularly reflected up the microscope: a principle of illumination which I think important. The two microscopes of the declination-circle spread out nearly in the same way as those of the meridional transit-circle, so that though they view opposite graduations on a 5-feet circle, the two eye-pieces are only a few inches

apart. The illumination of the divisions is effected as in the eye-piece for viewing the wires of the transit-circle by reflexion; the light enters a hole in the side of the eye-tube, where it is reflected downwards by diagonal plates of transparent glass; it falls upon the limb, whose surface is turned to a concave or dished conical form so that the axis of the microscope is perpendicular to the portion of the limb under view; and the light which has been thrown down that axis is therefore reflected specularly up the axis to the eye. For the observer's ascent to these microscopes, a swinging ladder is provided, of which the lower end rests on the floor, while the upper end is supported by a rope of adjustable length fastened to the top of the dome. There is a 5-foot clamp-circle attached to the opposite cheek of the polar-axis, whose clamp-screw and slow-motion are acted on by long handles from the eye-end of the telescope. For convenience of setting, and for easy reading of differences of polar distance, there is a radial bar at the side of the telescope-tube, turning on a pin near the centre of motion of the telescope, and having a graduation at its end near the eye-piece of the telescope; and this radial bar is bridled by an adjustable graduated sliding rod, of which the distant end is carried by a pin on one cheek of the polar-axis. The lights for all these purposes are gaslights; the upper pivot of the polar-axis is perforated, and a gas tube (connected with fixed gas tubes by a rotatory joint) is passed through the perforation, and distributes its branches where lights are required on the polar-axis; one branch passes by means of a rotatory joint through the declination-axis (which is perforated), and brings gas to a burner on the eye-end of the telescope. The communication of time is made by a small clock with 10-inch pendulum beating seconds, near the fixed microscope of the hour-circle, and by a galvanic chronometer carried by the eye-end of the telescope; but these clocks do not give independent times: the wire of a galvanic battery, whose circuit is completed at 59 seconds of every minute of the transit-clock, is carried through a coil into which swings a bar-magnet carried by the 10-inch pendulum, and is also carried through the coils of the galvanic chronometer, (being led from the fixed support to the polar frame by means of insulated springs on the fixed support, touching insulated rings on the polar frame, and being then led in a similar manner to the telescope,) and thus the 10-inch clock is regulated, and the chronometer is moved, in exact accordance with the transit-clock. The seconds' wheel of the chronometer has only 59 teeth, and the seconds' dial has only 59 divisions, one of which counts for the two successive seconds (28 and 29): this is necessary in consequence of the loss of one current per minute in the transit-clock, as before remarked. In the 10-inch clock, which is merely regulated by the current on the principle introduced by Mr. R. L. Jones, there is no such anomaly.

"Another pair of galvanic wires is brought, by insulated rings and springs, to a touch-piece at the eye-end of the telescope: these wires are connected with the pricker of the chronographic barrel, so that times may be registered on the same sheets as the times of observations with the transit-circle and the altazimuth.

"The clock-movement of the hour-circle is effected essentially by the same mechanism as that which I introduced at Liverpool. By transferring a water supply from a lower to a higher level, I have obtained a fall for driving a reaction machine (Barker's mill), revolving four times in a second. This, acting through two worms, drives the hour-circle. For its regulation, it gives motion to an axis revolving in two seconds, which acts upon a conical pendulum (carried by two pairs of springs that do the duty of a universal joint), by the remarkable contrivance called Sieman's chronometric governor, of which the effect is that, if there is a tendency to acceleration, the throttle-valve of the water-pipe is immediately contracted. It is necessary that the pendulum suffer a retarding force, which ought to be strictly tangential; to effect this, I have so connected a small spade with the rotatory apparatus, that the pendulum by enlarging its cone dips the spade into a trough of water. I anticipate from this a very smooth and uniform motion.

"Every part of the apparatus is provided with the proper means of gearing and ungearing, oiling, &c., according to the usages of the best modern engineering. The instrument may now be considered a splendid piece of mechanism; I hope that it will ultimately be judged to be a splendid equatoreal.

"The engineers' part of the work is executed by Messrs. Ransomes and Sims of Ipswich; the graduations and the optical parts generally (except the object-glass), including also the work of the eye-end, the radial bar and bridles, and the mounting of the object-glass, by Mr. Simms of Fleet Street; the object-glass by Messrs. Merz and Son of Munich. The pendulum of the clock movement, and the 10-inch clock, were furnished and mounted by Mr. Dent of the Strand. The galvanic chronometer was made by Mr. Shepherd of Leadenhall Street.

"The observing chair I propose to mount as a slider upon a vertical quadrant revolving round a point near the centre of the room, the part of the floor near the south pier being furnished with theatre seats, as for the Northumberland telescope. For enabling the observer to observe objects near the pole, without leaning upon the polar frame, I have surrounded the lower oval with a curb, for supporting a board that will pass between the cheeks of the frame, on which board the observer will rest.

"It is proper to mention that the focal length of the object-glass furnished by Messrs. Merz is considerably less than the length originally stated. The whole apparatus therefore is somewhat larger than is absolutely necessary. It might with

some inconvenience be made to carry a telescope of above 20 feet focal length; and, if the regions near the pole were given up, it might carry one still larger."

MISCELLANEOUS INTELLIGENCE.

A supposed New Interior Planet.

The conclusions arrived at by the distinguished Director of the Imperial Observatory at Paris in his recent investigations of the corrections required in the theory of *Mercury*, and which he communicated in so striking a manner to the world by his letter addressed to M. Faye, appear to have already received signal confirmation. An abstract of this remarkable letter was printed in the first number of the present volume, and it may now be acceptable to the readers of the *Monthly Notices* to be made acquainted with the circumstances of the observation recently announced.

From various accounts received from Paris it appears that not long after the publication of M. Le Verrier's assertion of the probable existence of a planet or planets interior to *Mercury* it began to be rumoured in Paris that more than one person had already seen *the* planet, but that, as is usual, the rumour assumed different forms and was coupled with different names. Towards the close of the year, however, a communication was made of so definite a kind to M. Le Verrier that he considered his official position required that he should at once probe it to the bottom, and take such steps as should prevent the success of any attempt to palm off a fraud on the public. Noting the name and address of the asserted observer he proceeded on the last Saturday of the year to the nearest railway station to Orgères, in the department Eure et Loire, and accompanied by a friend whom he took as a desirable witness of his proceedings, made his way direct and unannounced to the house of M. Lescarbault, residing at Orgères and practising as a country physician. A very graphic account of what here passed is given by the Abbé Moigno in a recent number of his useful publication, *Cosmos*, and is stated to be given nearly as recounted by M. Le Verrier to an assembly of friends on his return. M. Lescarbault was subjected to a severe cross-examination by his unknown visitor, who pressed him hard from step to step till he had obtained such material and verbal evidence as no longer permitted him to doubt the reality of the observation or the good faith of the observer. Not content to leave the question of good faith, where so much rested upon it, dependent on the replies of the individual concerned, he was careful to obtain collateral

evidence of the high character and worth of Dr. Lescarbault, from such other persons of station in the neighbourhood as should at once satisfy himself and others; and we believe that the precautions thus taken have been accepted as sufficient by all concerned.

At the first sitting of the Academy of Sciences after his return M. Le Verrier announced that on the afternoon of the 26th of March, 1859, a small dark body had been observed to transit a portion of the sun's disk by M. Lescarbault, and which bore all the appearance of being a new interior planet. And at the same time he stated that the observer had made such observations as led to the conclusion that the supposed new planet's distance from the sun was about 0·1427, its period less than 20 days, its ascending node situated at about 13° of longitude, and its inclination between 12° and 13°.

M. Lescarbault has since addressed a written account of his observation to M. Le Verrier which has been published, in which he states that having witnessed the transit of *Mercury* in 1845, he had himself at once inferred that the body was an interior planet, and that, living in retirement, he had kept his discovery to himself, in the hope of being able to come to some conclusion respecting its distance from the sun, by calculation, or by being so fortunate as again to observe its transit; but that being only a moderate geometer, and much pressed by professional engagements, the problem had baffled him, and he had at last been induced to break silence on reading an account of M. Le Verrier's theoretical conclusions, printed in the publication of Abbé Moigno, to which he was accustomed to subscribe.

M. Lescarbault first detected the body when a little way advanced on to the disk, and inferred the time of first contact by noting the interval which elapsed while it advanced over what he estimated to be an equal space. He then repeatedly measured the angle from the zenith or nadir of the point of the limb to which the body was nearest and its distance* from the limb, and watched its passage off the sun. Correcting his measured angles for the angle between the pole and zenith, it would appear that the first contact took place at 19° 4′ and the last contact at 52° 42′ from the sun's north point measured towards the west, the greatest distance from the limb being 41″·3, and the times $4^h\,0^m$ and $5^h\,47^m{\cdot}2$ Orgères local time, little more or less. The estimated diameter was about one-fourth that of *Mercury* when last on the sun; and as this was 11″·6, the inference from the statement would be about 3″ for the new body, if so small a quantity can be admitted as probable.

At the meeting of the Society on January 13, Mr. Carrington

* There is a shadow of doubt on this point, which it is desirable to have removed. The distance (if measured) singularly accords with that inferred from the angles and radius.

exhibited two diagrams in illustration of the original observation and of the inferences to be made from it, and pointed out in detail the very simple process required in such a case for arriving at the approximate elements of the orbit supposed circular. The concluded elements were not sensibly different from those given by M. Le Verrier, except in the case of the inclination, which was concluded to be about 11° 51′. The inclination to the orbit of *Mercury* was inferred to be about 7°.

The remark of principal immediate importance was that, inasmuch as the observation of March 26, 1859 was made about $7\frac{1}{2}$ days before the earth passed through the calculated line of nodes, and an equal space thereafter was similarly available, it followed that for the space of 15 days at each conjunction the orbit was projected on the sun's disk; and that as the revolution was performed in less than 20 days, it was more probable than not that, unless the original observation were more in fault than supposed, the body might be re-observed in the spring of this year. The attention of observers was particularly called to this circumstance. The days referred to are

March 25 to April 10 at descending node,
and Sept. 27 to Oct. 14 at ascending node.

The singular merit of M. Lescarbault's observation will be recognised by all who examine the attendant circumstances; and astronomers of all countries will unite in applauding this second triumphant conclusion to the theoretical inquiries of M. Le Verrier. The redetection of this new member of the solar system must next engage the co-operation of observers; and it is hoped that the astronomer at Madras will unite his efforts with those of European and American observers for the purpose.

In the 10th number of Professor Wolf's *Mittheilungen über die Sonnenflecken* several cases are quoted of the observation of planetary bodies in transit over the sun, some of which are evidently of another character, but the following deserving of attention:—

On January 18, 1798, Dangos saw a round sharply defined spot traverse the sun in about 2 hours.

On February 12, 1820, Stark saw about noon a singular and well-defined circular spot with indications of an atmosphere, which was not visible in the evening of the same day.

At the end of February, 1762, Staudacher saw a black round spot, which he missed the next day, and of which he made a note, "Might this have been a new planet?"

At the beginning of May, 1764, Hoffmann remarked a large round black spot which appeared to have independent motion.

On June 6, 1761, Scheuter at Crefeld believed that he saw traverse the sun a round and well-defined spot, one-fourth the size of *Venus* and as black (as that planet when in transit?).

On October 9, 1819, Stark saw a well-defined and truly circular spot about the size of *Mercury*, which he could not find again in the evening.

On October 10, 1802, Fritsch saw a small round spot pass over the sun.

On November 19, 1762, Lichtenberg soon after sunrise saw a large round black spot, which manifestly moved before the sun, and made its exit after traversing a chord of 70°.

The observations of Stark on Oct. 9, 1819, and of Fritsch on Oct. 10, 1802, may prove to be transits at the ascending node of Lescarbault's planet as the recorded dates fall within the days of visibility. Professor Wolf quotes also an observation of Julius Schmidt on Oct. 11, 1847, but Schmidt himself remarks that the *rapidly* moving point seen by him was probably an insect or distant bird, and there are no sufficient grounds for retaining his note as of further value.

[R. C. C.]

Solar Spots.

Hofrath Schwabe has published his usual annual census of the groups of solar spots observed by him at Dessau in the year 1859. Referring to the *Astron. Nach.* (No. 1230, p. 95) for the monthly numbers, it may be sufficient here, as an abstract, to state that the sun was viewed on 343 days, on none of which was the disk found to be blank, and that 205 groups were registered. In comparing this number with those of the years preceding, it will be perceived that the expected maximum is nearly, if not already, attained, and that the arrested progress of the curve seems to indicate that the present maximum of energy will fall far short of that in the year 1848.

M. Otto Struve writes from Pulkowa, under date of December 31st, 1859, to the effect that M. Oblomicosky has been appointed to be director of the new observatory ordered by the Emperor of Russia to be erected at Tiflis, with a special

view to the department of "mountain-astronomy;" and that the Report on the Teneriffe astronomical experiment of 1856 will form the basis of his future proceedings, in the annual visits which he expects to make from his permanent station at Tiflis to the summit of Mount Ararat or Mount Elburz.

A letter was read from Major Tennant, B.E., announcing that he had assumed the charge of the Madras Observatory on the 13th of October, 1859, under his appointment by the Secretary of State for India.

Sir John Herschel communicated further delineations of the solar spots by the Rev. F. Howlett.

The *Compte Rendu* of the French Academy for 1860, January 2, contains letters by M. Rico y Sinobas and M. Verneuil, on the selection of stations in Spain for the observation of the total eclipse of July 18. It is stated that there are frequent clouds in the valleys of the mountains which separate the Asturias, Santander, and Biscay, from the rest of Spain, and that there is also a vaporous stratum in the upper regions of the atmosphere. The annual rain-fall in these provinces is about 140 inches.

Even in the valley of the Ebro and on the mountains of Pancorbo, though there are fewer clouds in the hot season, there is a dry fog after noon called the *caliña*.

The most favourable positions, in respect of clearness of atmosphere, appear to be the mountains of Gorbea near Vittoria, the plateau of Alaba, the peaks of Pancorbo near Burgos, the peaks of Urbion (Cameros), the mountains of Treviño, but above all the mountain of Moncayo near Soria, 2340 mètres high.

Aragon and the eastern counties of Spain appear less favourable, as there are always cirrus clouds after noon.

M. Verneuil describes the best approach to the Moncayo as to be made by taking the diligence from Bayonne to Madrid, *viâ* Soria, as far as Agredo, from which a five hours' walk leads to the hermitage of Virgen at 1600 mètres elevation, where lodging might be obtained.

The Peña Golosa in Valentia, and Oropesa, are also mentioned as advantageous stations.

Other stations, generally on mountains, are also described, as advantageously situated near the northern and southern limits of the eclipse.

RECENT PUBLICATIONS.

Untersuchungen über die Lichtstärke der Planeten Venus, Mars, Jupiter, und Saturn verglichen mit Sternen, und über die relative Weisse ihrer Oberflächen, Nebst einem Anhange enthaltend die Theorie der Lichterscheinung des Saturn. Von L. Seidel. 4to. Munich, 1859. (Forms a part of the *Monumenta Sæcularia*, published by the Royal Bavarian Academy of Sciences on the 100th anniversary of the foundation.)

The author refers to a paper in the *Memoirs of the Academy* for 1852, and to some results published in the *Gelehrte Anzeigen*, Sept. 1853, in relation to the planets *Venus*, *Mars*, and *Jupiter*. The observations were made by him and Prof. Leonhard by means of Steinheil's objective or prism photometer. The memoir contains formulæ for the reduction of the results to standard positions of the planets under consideration; and the author finds numerical values for the quantities of light of the four planets respectively, in comparison with the star *Wega*.

He infers from the observations that the changes (if any) in the quantity of light within the period of the observations (August 1845 to June 1858) are so small that they are concealed by the necessary uncertainty of the separate observations, and no change is indicated by a comparison of the results with two observations by Olbers as to *Mars* and *Saturn* in the years 1801 and 1803. The author defines the "Albedo" of a planet to be the proportion diffusedly reflected by an element of surface, of the solar light incident on such element; and he obtains from the observations numerical relative values (Alb. other planet: Alb. *Jupiter*) for the four planets, the result being, that the Albedo is nearly the same for *Venus*, *Mars*, and *Jupiter*, but only about one-fifth for *Saturn*. The addition contains a mathematical investigation as to the quantity of light reflected by *Saturn*, taking account of the ring.

Annales de l'Observatoire Impériale de Paris. Publiées par U. J. Leverrier, Directeur de l'Observatoire. Observations, t. II. Paris, 1859.

This volume contains the reductions of Arago's observations for the years 1837 and 1838. The processes of reduction are explained in an introduction, and the concluded right ascensions and polar distances of the centres of the Sun, Moon, and Planets, are given at the end of the volume.

The *Connaissance des Temps* for 1861 has been published. The additions contain a memoir by M. Delaunay, "Nouvelle Méthode pour l'intégration des équations différentielles du Mouvement de la Lune autour de la Terre": the most detailed exposition he has as yet given of the method employed in his researches in the lunar theory, some of the results of which, in relation to the acceleration of the mean motion, have been already published in the *Comptes Rendus*.

Johannis Kepleri Astronomi Opera Omnia. Ed. Ch. Fusch, Frankfort and Erlangen.

Volumes i. and ii. of this important publication have appeared.

Report on the Teneriffe Astronomical Experiment of 1856, *Addressed to the Lords Commissioners of the Admiralty.* By Professor C. Piazzi Smyth, F.R.SS.L. and E., &c., H.M. Astronomer for Scotland.

This paper has been printed at the expense of the Admiralty. Some of the principal results recorded in it have been already published in the Annual Report of the Council of the Society for the year 1857. The paper is divided into eight chapters.* The first chapter is devoted to an account of the circumstances connected with the origin of the expedition. In the second chapter the author describes the improvement of astronomical vision, depending on the increase of height above the level of the sea. The third chapter is occupied with an account of the astronomical observations made by the author during his residence on the Peak. The fourth chapter contains the physical observations. The fifth chapter is devoted to the meteorological observations; the sixth, to the geology of the mountain; the seventh, to the botany; the eighth, to the miscellaneous observations. Appended to the paper are a series of valuable drawings illustrative of the facts recorded in it.

By favour of General T. F. de Schubert, Honorary Member of the Imperial Academy of Sciences at St. Petersburg, copies of his "Essai d'une Détermination de la véritable Figure de la Terre" (printed in the *Mém. de l'Acad. Imp. des Sciences de St. Pétersbourg*, vii Série, Tome i. No. 6) have lately been received in this country. The meridian arcs used by the

* The first five chapters form a paper which is printed at the expense of the Admiralty in Part II. of the *Philosophical Transactions* of the Royal Society, t. cxlviii. (1858).

author are the following, in which the longitudes are measured eastwardly from an imaginary meridian 20° west of Paris:—

The Russian Arc	Longitude	45	20	to	70	40	Longitude	44 23
The Indian Arc	,,	8	10	to	29	31	,,	95 20
The French Arc	,,	38	40	to	51	2	,,	20 0
The Cape Arc	,,	−29	44	to	−34	21	,,	36 9
The Peruvian Arc	,,	0	3	to	− 3	5	,,	298 44
The Prussian Arc	,,	54	13	to	55	43	,,	38 10
The English Arc	,,	50	37	to	53	28	,,	17 40
		(in first calculations)						
		50	37	to	60	50		
		(in final calculations)						
The Pennsylvanian Arc	,,	38	27	to	39	56	,,	300 10

The author first exhibits the elliptic formulæ which he has used, and which appear well adapted to give results of great accuracy and with much convenience. He then applies them to the comparison of each of these eight arcs with every other, and thus obtains 28 systems of elements, presenting great discordances. For the earth's polar semiaxis, expressed in toises, the largest result is 3274069, and the smallest 3245754; for the earth's equatoreal semiaxis, the largest result is 3279418, and the smallest 3259832; for the denominator of the fraction expressing the oblateness, the largest number is 14501, and the smallest 116. These discrepancies appear to the author to show that a different method of treatment is required; and he proposes and applies the following:—

First, he remarks that we have assumed in the preceding calculations "that the terrestrial meridians are ellipses," and "that their minor axis is, for all, the same as the axis of rotation of the earth," which assumptions our observations do not enable us to disprove; but we have also assumed "that the earth is a solid of revolution, or that all the meridians are equal," an assumption which he conceives the discrepancies above mentioned show to be untenable, if the two preceding assumptions are maintained. As soon as this is given up, all comparisons of arcs of meridian in different longitudes become illusory. The next inquiry is, What element is common to all the ellipses? and this, it is evident, is the minor semiaxis. [We have been very much astonished to see that in several late discussions this important principle has been entirely forgotten.] It is clear that this minor axis cannot be determined from a combination of arcs in different meridians, but must be found from a comparison of arcs in the same meridian, or (which amounts to the same), by dividing a long arc into two segments, and comparing one segment with the other, or comparing each segment with the whole arc. Thus the author uses the three following arcs (the longest of the series):—

The Russian arc, divided into two parts at Dorpat, latitude 58° 23′, and giving for the minor semiaxis 3261429.

The Indian arc, divided into two parts at Damargida, latitude 18° 3′, and giving for minor semiaxis 3261547.

The French arc, divided into two parts at Carcassonne, latitude 43° 13′, and giving for minor semiaxis 3260365.

The author then expresses his unwillingness to combine this French result with the others, on account of its discordance; but remarks that this discordance would be removed by supposing an error of 2″ in the latitude of Carcassonne. [We conceive that this explanation brings to light what is really a weak point in the investigation. The curvature of the meridian changes more rapidly in the arc divided in latitude 43° than in any other arc; and if an error of 2″ is fatal to that determination, an error of 1″ might be fatal to either of the others.] Using the Russian arc with weight 2, and the Indian arc with weight 1, he adopts for polar semiaxis 3261468.

Using this polar semiaxis as a known quantity, in the formulæ applying to the three arcs of Peru, Russia, and India, (the selection of which is determined in part by their difference of longitude), the author finds the following values for the major semiaxes of the meridians of those arcs:—

Major Semiaxis of Peruvian Meridian,	Longitude	298° 44′,	3272383
Major Semiaxis of Russian Meridian,	Longitude	44 23,	3272650
Major Semiaxis of Indian Meridian,	Longitude	95 20,	3272581

The question now arises, What is the form of the terrestrial equator? The author assumes it to be an ellipse. The three numbers just found are the values of three radii of that ellipse; and are sufficient to determine its elements. They give,—

Major Semiaxis	3272671;	its Longitude	58° 44′	or	238° 44′
Minor Semiaxis	3272303;	its Longitude	148 44	or	328 44

(The combination of these with the polar semiaxis above gives for the oblateness of the two principal meridians $\frac{1}{299}$ and $\frac{1}{302}$.)

With these semiaxes he computes the radii of the equatoreal ellipse corresponding to the different arcs (which are, in fact, the major semiaxes of their meridians), and finds them as follows:—

Major Semiaxis of Peruvian Meridian	...	3272395
Pennsylvanian	...	3272388
English ...	...	3272513
French ...	...	3272528
Cape of Good Hope	...	3272618
Prussian ...	...	3272626
Russian ...	...	3272650
Indian ...	...	3272540
Also, in Meridian of Warsaw ...	...	3272628
Pulkowa ...	...	3272659

With the arcs computed from these elements and the astronomical differences of latitude, the geodetic measures are compared, and the residual discordances are as follows:—

Geodetic Amplitude — Astronomical Amplitude.

Peruvian Arc	...	...	+0·077
Pennsylvanian	...	...	−6·687
English (entire arc)	...	...	+0·736
French	...	...	−1·607
Cape of Good Hope	...	...	−0·442
Prussian	...	...	+1·267
Russian	...	...	−1·289
Indian	...	...	+1·619

The Pennsylvanian arc, as is well known, deserves no *à priori* confidence, and has been introduced only as a matter of curiosity.

In the Indian arc no calculation is made of the effects of the attraction of mountains.

The author then applies his elements to compute the longitudes of Warsaw and Dorpat, as referred to Pulkowa. On comparison with the geodetic measures of parallel, it is found that the discordances are reduced as follows:—

Astronomical Diff. Long. — Geodetic Diff. Long.

For Warsaw, is reduced from −124·7 toises to +58·8 toises.
For Dorpat, ,, from −57·9 ,, to − 7·8

We think that this investigation merits the most careful attention of geodetists. In repeating or extending it, we would suggest for consideration whether the determination of the value of the polar semiaxis would not be more satisfactory if it were left to be decided, with the other semiaxes, by a general process combining the consideration of the lengths of all the arcs, introducing also the latitudes of middle stations.

Referring to the mechanical explanation of the existence of a fluid ellipsoid with three unequal axes (two nearly equal), it appears that it can be explained by supposing the existence of an ellipsoidal nucleus of three unequal axes. Should the existence of the former be established, the mode of original formation of the latter may be a problem for speculators in cosmogony. [G. B. A.]

ERRATUM.

Page 58, line 12, *dele* the word "muddled."

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *February* 10, 1860. No. 4.

THE Annual General Meeting of the Society, Rev. R. MAIN, President, in the Chair.

George Frodsham, Esq., Change Alley;
John Pope Hennessey, Esq. M.P., of the Inner Temple; and
Geo. Johnstone Stoney, Esq. M.A., Secretary to the Queen's University, Ireland,

were balloted for and duly elected Fellows of the Society.

At the Meeting in January, Thomas Heelis, Esq., Manchester, was balloted for and duly elected a Fellow of the Society.

Report of the Council to the Fortieth Annual General Meeting of the Society.

The Council present their usual Annual Report.

The Report of the Auditors, subjoined, will show the state of the finances:—

RECEIPTS.

	£	s.	d.
Balance of last year's account	316	7	9
By dividend on £1700 Consols	24	19	5
By ditto on £109 8*s.* 9*d.*	1	12	9
By ditto on £3500 new 3 per Cents	51	8	2
Carried forward	£394	8	1

	£	s.	d.
Brought forward	394	8	1
By dividend on £1900 Consols	27	18	2
By ditto on £3500 new 3 per Cents	49	13	2
£109 8s. 9d. 3 per Cents reduced, sold at £94, commission, &c.	102	14	8
On account of arrears of contributions	58	12	0
102 contributions (1859–60)	214	4	0
7 compositions	144	18	0
25 admission-fees	52	10	0
20 first year's contributions	37	16	0
Sale of Publications	57	7	0
	£1140	1	1

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Mr. Grant, 1 year as Editor of the Society's Publications	60	0	0			
Mr. Williams, 1 year as Assistant-Secretary	100	0	0			
Ditto commission on collecting £420 9s.	21	0	6			
				181	0	6
Investments:—						
Investing £200 Consols				190	15	0
Taxes:—						
Income Tax, 1 year	1	0	10			
Assessed Taxes, ditto	5	7	6			
				6	8	4
Bills:—						
Geo. Barclay, printer	313	17	0			
J. Rumfit, bookbinder	10	4	0			
Malby and Son, lithographers	24	12	0			
W. Rutter, Esq. (living of Stone)	5	0	0			
Sundry payments out of Turnor Fund	9	14	6			
				363	7	6
Miscellaneous items:—						
Power of attorney to transfer stock	1	1	6			
Charges on books, and carriage of parcels	3	10	5			
Postage of *Monthly Notices*, letters, &c.	41	13	3			
Porter's and charwoman's work	22	15	8			
Tea, sugar, biscuits, &c. for evening meetings	15	12	0			
Waiters attending meetings	4	8	0			
Expenses of meeting, June 1858 (omitted 1858)	2	10	0			
Coals, &c.	12	0	0			
Sundry disbursements by the Treasurer	29	2	4			
Banker's clerks	1	1	0			
				133	14	2
Balance at Banker's				264	15	7
				£1140	1	1

Assets and present property of the Society, Feb. 8, 1860:—

	£	s.	d.
Balance at Banker's	264	15	7
13 contributions of 2 years' standing 54 12 0			
63 ——— of 1 ditto 132 6 0			
On account of arrears 3 18 0			
	190	16	0
Due for publications of the Society	0	14	6

£3500 new 3 per Cents.
£1900 Consols (including the Lee Fund).
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.
Balance of Turnor Fund (included in Treasurer's account) ... 31 2 10

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	28	IX.	195	XXI. Part 1 (separate).	315
I. Part 2	70	X.	207	XXI. Part 2 (separate).	100
II. Part 1	87	XI.	218	XXI. (together).	125
II. Part 2	52	XII.	224	XXII.	220
III. Part 1	112	XIII.	241	XXIII.	220
III. Part 2	132	XIV.	425	XXIV.	235
IV. Part 1	127	XV.	211	XXV.	242
IV. Part 2	140	XVI.	233	XXVI.	262
V.	160	XVII.	211	XXVII.	541
VI.	180	XVIII.	211		
VII.	203	XIX.	224		
VIII.	189	XX.	221		

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patronesses and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1859 ...	153	186	39	5	383	52	435
Since elected	6	17	...	...	23	...	23
Deceased	−5	−4	−7	−1	−17	−1	−18
Resigned	...	−5	...	...	−5	...	−5
Removal	1	−1	...	...	...	...	...
February 1860 ...	155	193	32	4	384	51	435

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock,
The *Wollaston* telescope,
The *Lee* circle.

The *Sheepshanks*' collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. 4-$\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters.

4. 3$\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. 2$\frac{1}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. 2$\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch

theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and $2\frac{1}{4}$-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Wollaston* telescope, to Mr. Rees.
The *Lee* circle, to Mr. Burr.

The *Sheepshanks* instrument, No. 1, to Mr. Lassell.
Ditto ditto No. 2, to Mr. De La Rue.
Ditto ditto No. 3, to Rev. C Pritchard.

The *Sheepshanks* instrument,		No. 4, to Mr. Carrington.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 8, to Prof. Wheatstone.
Ditto	ditto	No. 16, to Admiral Bethune.
Ditto	ditto	No. 18, to ditto.
Ditto	ditto	No. 32, to ditto.
Ditto	ditto	No. 19, to Mr. Dayman.
Ditto	ditto	No. 25, to ditto.
Ditto	ditto	No. 41, to Rev. C. Pritchard.

Nothing definite has yet been ascertained respecting the other *Beaufoy* clock, the two invariable pendulums, and the quadrant (said to have been *Lacaille's*), reported for many years past as being in the possession of the Royal Society.

The Council have awarded the Gold Medal to Professor Hansen, for his Tables of the Moon, constructed on the principles of universal gravitation. The President will detail the grounds of this award in the usual way at the close of the Meeting.

The twenty-eighth volume of the *Memoirs* of the Society has passed through the press and will shortly be ready for publication. It contains several papers of great importance. The volume commences with a series of communications from Capt. Jacob, embodying the results of his recent labours at the Madras Observatory. Among these will be found a catalogue of 317 stars which were selected for observation on the ground of the large proper motions assigned to them in the British Association Catalogue. It appears, from the new places of the southern stars contained in this Catalogue, that the large proper motions which had been in many instances indicated by anterior observations are in a great measure unfounded. Another paper by Capt. Jacob, which is eminently deserving of attention, consists of micrometrical measures of 120 double or multiple stars. Appended to these results is a valuable disquisition on the orbits of several stars whose binary character has been conclusively established. The remaining papers by the same author relate chiefly to the Saturnian system. It would be out of place here to enter into any details, but the Council cannot refrain from directing the attention of the Fellows of the Society to a very elaborate discussion of the elements of the orbits of *Saturn's* satellites and to the author's researches on the mass of the planet, an element of so much importance in physical astronomy. The Council feel assured that the Society, while applauding the energy and ability which Capt. Jacob has exhibited in his various communications, will cordially unite with them in offering their sincere con-

gratulations to him on the improved state of his health consequent upon his return to England.

The President, to whom astronomy is so deeply indebted for his researches on the subject of the proper motions of the stars, has communicated a valuable paper on the proper motions of the stars in the recently published Greenwich Catalogue of 1576 stars. The results are deduced by a comparison of the modern determinations with the corresponding places derivable from Bradley's observations as reduced by Bessel in the *Fundamenta*. This paper by Mr. Main combined with another by him on the same subject which is printed in the nineteenth volume of the *Memoirs* of the Society, and of which it may be regarded as a continuation, will be found to embody the most important results relative to the proper motions of the stars, which are deducible from the observations made at the Royal Observatory, Greenwich, during the period of eighteen years comprised between the years 1835 and 1853.

The Astronomer Royal has contributed two papers to the volume of the *Memoirs* for the past year. The first relates to the important problem of the motion of the solar system in space. The Council would direct the special attention of the Society to this paper, inasmuch as the method of investigation is totally different to that employed by the author's predecessors in the same field of inquiry. Instead of assuming the apex of solar motion as approximately known, and then deducing a more correct result by a combination of observations having reference solely to the angular motions of the sun and stars, Mr. Airy treats the problem independently of any previous results, basing his researches on linear motions, which he expresses in terms of rectangular co-ordinates referred to the sun as the origin. He then forms equations of condition which, besides the unknown quantities of the problem, involve also possible errors in the constants which enter into the formulæ for precession in right ascension and north polar distance, and also a quantity denoting the possible error of Bradley's observations in north polar distance. The equations are then solved on two extreme suppositions with respect to the irregularities of proper motion. According to one of these suppositions the irregularities are wholly due to chance observations of the stars; according to the other they arise entirely from the peculiar motions of the stars. Laying aside all consideration of the existence of possible errors in the constants of precession, and of a possible error of Bradley's observations, the results obtained for the co-ordinates of the apex of solar motion agree pretty nearly in right ascension, but differ a good deal in declination, from the mean of the results arrived at by previous inquirers.

The second paper contributed by the Astronomer Royal relates to the curious phenomenon of the apparent projection of a star on the moon's disk. The author first considers the hypothesis of the effect being due to the unequal refrangibility

of the rays proceeding from the moon and star, but this view of the subject is found to be untenable. He then shows that the phenomenon admits of an intelligible explanation founded on the undulatory theory of light, in which case it must be regarded as an effect of irradiation. This solution of a very obscure point of inquiry is perhaps the most satisfactory that has hitherto been proposed.

Mr. Cayley, who is so well known for his powers as an analyst of the highest order, has contributed two papers. The former of them contains the first part of an investigation of the development of the disturbing function in the lunar and planetary theories. The author refers to a fundamental formula of Hansen's, which gives the expression of the coefficient of the general term of the reciprocal of the distance. A new investigation is given of this expression and its form is somewhat simplified. The object of the paper is to exhibit in as complete a form as possible the preliminary development in multiple cosines of the true anomalies; and to indicate the process of obtaining the ulterior development in multiple cosines of the mean anomalies. The latter of them, supplementary to the author's memoir on the Problem of Disturbed Elliptic Motion, shows how the formulæ contained therein are deduced by means of the general equations of the *Mécanique Analytique*, from the expression of the *Vis Viva* function in terms of the co-ordinates of the body in its instantaneous orbit, and completes the development which he was anxious to give of the dynamical problem.

The volume concludes with three short papers by Capt. Tennant, on subjects connected with the determination of the latitude by observations of transits of stars in the prime vertical or its vicinity. To astronomers in general, and more especially to persons engaged in geodetical researches, these communications will be found to offer considerable interest.

The nineteenth volume of the *Monthly Notices*, which has been published some time since in the usual octavo form, has been also printed in quarto sheets, and will be bound up with the *Memoirs* of the Society, conformably to a resolution of the Council submitted to the Society, and approved at the Annual Meeting of last year.

The Council have much pleasure in announcing the appointment of Mr. Grant to the Professorship of Astronomy in the University of Glasgow, vacant by the death of Dr. Nichol. The appointment is in the hands of the Home Secretary. While rejoicing at the acknowledgment which the author of the *History of Physical Astronomy* has thus received, the Council are fully sensible of the loss which their body has sustained in the retirement of Mr. Grant from the editorship of the *Memoirs* and *Monthly Notices* — a post the duties of

which he has performed with singular assiduity and ability. But they are happy to state that Mr. Cayley has accepted the vacant office, and has relieved the Council of all feeling of anxiety about the performance of duties on which, more than any others, the utility and honour of the Society depend.

By the death of Miss Elizabeth Coxe Baily, the sister of Francis Baily, which took place on the 6th of last September, the house in Tavistock Place in which the pendulum experiments and the repetition of the Cavendish experiment were performed, falls by bequest into the possession of Sir John Herschel. It will be remembered that Miss Baily presented to the Society the marble bust of her brother which stands in our entrance-hall. For his sake she always felt a strong interest in the Society, and for his sake she desired that the house which he had so closely connected with the astronomical history of his time should still retain its old associations. It would have been impossible to point out a better mode of carrying the intention into effect.

The past year has been gladdened to many of the older Fellows of the Society by a visit to England of Mr. Maclear, who has been for twenty-five years pursuing his astronomical duties at the Cape, without any such vacation as he has at last allowed to himself. This visit was chiefly for recreation, and there is therefore not much to be said of it astronomically; though, from the attention which Mr. Maclear paid to what has been doing in England and on the Continent, the fruits of it will probably be apparent. He takes out a magnetic electrical train for the observatory, electrical triggers for the time-ball, and meteorological instruments, and copies of the standard weights and measures for the Colonial Government; and he hopes to get up a time-ball at Simon's Bay. His old friends were happy to see that his health and energy continue unbroken; and should he be continued in their possession, they feel confident that he has a successful career still before him.

The fellows are aware that a part of the munificent gift of 10,000*l.* to the University of Cambridge by the representative of the late Mr. Sheepshanks for astronomical purposes, has been devoted to the maintenance of an astronomical exhibition. The first appointment under this endowment was competed for in October last, and the examination papers were constructed so as to demand a very high union of theory and practice. Each candidate was also examined *vivâ voce* by the Astronomer Royal on the construction and use of some astronomical instruments, the other examiners being Professor Adams and Mr. Mathison. The number of candidates was six. The exhibition was gained by Mr. Stirling, of Trinity College, who has since

that time gained the place of Senior Wrangler; thus giving the Sheepshanks' Exhibition the best possible introduction to public notice. A copy of the examination papers is placed on the table for the inspection of the Fellows.

About four-and-twenty years ago our frequent benefactor, Dr. Lee, gave 100*l.* to the Society as the commencement of a fund, the proceeds of which should be devoted to the succour of such widows and orphans of Fellows of the Society as might be in a condition to claim it. Though this little fund has as yet received no accession, the Council have had fit opportunities of devoting the yearly income to its intended object. Looking at the enormous sums which are bequeathed for such benevolent purposes in this country in every year which passes over our heads, the Council draw the attention of the Fellows to this *Lee Fund*, as it has always been called, in the hope that, by making its existence known, they may possibly be the means of procuring some accessions from those who delight to leave the memory of good deeds behind them.

The Council regret to have to report the loss by death of our valued Associate, Mr. W. C. Bond, of the Cambridge (U. S.) Observatory; Gen. Sir T. M. Brisbane, Bart.; I. K. Brunel, Esq.; Major-Gen. Blanshard; Samuel Cooper, Esq.; Earl De Grey; E. Hughes, Esq.; Rev. Dr. Inman; M. J. Johnson, Esq.; the Earl of Minto; Dr. Nichol; J. A. Nicholls, Esq.; Dr. Ogle; R. Stephenson, Esq., C. E.; Robert Stevenson, Esq., of Edinburgh; Admiral Trotter; and the Rev. W. W. Walton.

William Cranch Bond, M. A., Director of the Astronomical Observatory of Harvard College, was the youngest son of William Bond, who came from England in 1784, and settled as a merchant and shipowner at Falmouth (now Portland), Maine, where the subject of this notice was born in September, 1789. His father having removed with his family to Boston in 1790, at an early age he engaged himself in the occupation of a watchmaker. In 1806 his attention was especially attracted to astronomy by witnessing the sublime spectacle of the total eclipse of the sun, which took place in June of that year. From that time the contemplation of celestial phenomena became his highest pleasure.

He was the earliest discoverer, in America, of the celebrated comet of 1811, while it was yet barely visible to the unassisted eye, and several months before it was generally recognised. At this time, unprovided with a telescope, he was accustomed to make use of the expedient of shading the eyes from stray light, and gazing for several minutes into a dark well. By this means the vision was stimulated, so as to discern the fainter celestial objects which, without such precaution, would have been altogether invisible; the young astronomer little

dreaming, in his utter destitution of the means of observation, that he would one day be in possession of unsurpassed advantages for prosecuting his favourite pursuits.

To supply immediate deficiencies, and especially to provide for an accurate determination of local time for the regulation of the ships' chronometers under his care, he constructed a quadrant, and an instrument for obtaining meridian and altitude passages of stars, and subsequently a chronometer—the latter the *first* of American manufacture.

In 1815, Mr. Bond visited England, with instructions from the authorities of Harvard College to examine and report upon the principal English observatories, with the view to the contemplated erection of one at Cambridge. The project was not, however, carried into execution until several years later.

By the addition of astronomical, magnetic, and meteorological instruments, and by the erection of a private observatory at Dorchester, his appliances for observation in these departments became the best in the country.

In the year 1838, he was appointed by the United States' Navy Department to prosecute a series of observations in connexion with the Exploring Expedition under the command of Captain Charles Wilkes. While thus engaged, he was invited by the corporation of Harvard College to remove with his apparatus to Cambridge. In compliance with this request, he continued his observations at Cambridge, under a provisional arrangement, until 1840, when he was appointed Astronomical Observer to the University.

The present Observatory of Harvard College, erected in 1843–44, liberally furnished and endowed by private munificence, has, under his charge, contributed in no inconsiderable degree to the scientific reputation of the country.

Mr. Bond was the first to point out a practicable and exact method by which the instant of time of an astronomical observation could be recorded, by an immediate connexion of the sidereal clock with the galvanic circuit. The idea thus conceived, and discussed between him and the late lamented Professor Walker, has since been perfected in its details at the observatory. Celestial photography was here also first recognised as a possible art, and conducted through its earliest stages. Each of these aids to observation, at the time of its introduction, was a new conception among astronomical methods. The superiority of the former to all other methods for the registration of time-intervals is now universally admitted. The latter, though in its infancy, is full of promise of future application to the most important purposes.

The observatory, under his conduct, has further advanced astronomy by the discovery of a new ring in the system of *Saturn*, and by adding the satellite *Hyperion* to its list of secondary planets. Ten or fifteen comets have also been independently discovered there, and much labour has been

expended in the examination and delineation of nebulæ, and in the formation of star-catalogues, the greater part of which remain unpublished.

The accurate longitude of all points of the Continent relatively to the European meridians is wholly dependent upon that of Cambridge, which rests mainly upon the laborious accumulation of observations made for that purpose by Mr. Bond at Dorchester, and subsequently at Cambridge. In considering these, he has been in a position, by constant co-operation with the great national surveys of the coast and of the interior, to render important public services in establishing the geography of the country.

Mr. Bond was a member of various scientific bodies of the United States; a Corresponding Member of the Institute of France, of the Société Philomatique of Paris, of the Society of Natural Sciences of Cherbourg, and of the Academia de Nuovi Lincei at Rome. He died January 29, 1859.

The very recent date of the death of our most eminent Fellow, General Sir THOMAS BRISBANE, renders it desirable that an account of his astronomical career should be deferred until the next report. The Council therefore stop at the announcement of his death on the 28th of January last, at the age of eighty-five.

EDWARD HUGHES was born in Ireland in 1819, and received the rudiments of education in the Kildare Place Model School, Dublin, where his assiduity procured him the friendship of the master, Mr. Irvine, through whose recommendation he was accepted as the junior teacher of writing and arithmetic in the Clergy Sons' School at Edgeworth's Town in 1836. Through the interest of the same friend, Mr. Hughes removed, in 1839, to Aubyn's Pauper Model School at Norwood, which was then under the active patronage of the Committee of Council on Education.

Shortly after this, Mr. Irvine was appointed Head Master of the Lower School of Greenwich Hospital, and in 1841 procured the appointment of Mr. Hughes as his first assistant; and in 1844, retiring in ill health, Mr. Hughes succeeded to his situation.

While at Greenwich, Mr. Hughes edited and compiled several elementary educational books, principally on geography and arithmetic, evincing considerable research and an appreciation of the requirements of modern elementary instruction.

In his last work—a reading-book, in four volumes—he secured the co-operation of many persons distinguished in their special departments of knowledge; but it may be conjectured that the expense which this entailed upon him, while it exhausted his means, harassed and distressed him, burthened as he was with a very numerous family. His health failed him

for a considerable time before his death, which took place at Greenwich Hospital on July 30th, 1859, in his fortieth year.

JAMES INMAN, a distinguished member of our Society, who ably directed the application of astronomy to one of its most useful objects—the navigation of the seas—was the younger son of a Yorkshire *statesman*, whose ancestors had been landowners for centuries in the valleys which lie under the height of Penegant. He was educated at Sedbergh Grammar School, and proceeded from thence to St. John's College, Cambridge, where he graduated as Senior Wrangler in 1800. Some little time after his degree, he was sent by the Board of Longitude to assist Flinders in his astronomical observations in the South Seas. This duty was performed under some difficulties. The astronomer was diverted from his object, and was sent to take off the crew of the Porpoise from the reef on which she was wrecked. On that reef Dr. Inman first met Sir John Franklin, who was then a midshipman in the Porpoise, and commenced an intimacy which continued until the last fatal enterprise of that celebrated navigator. The observations made by Dr. Inman in Australia are to be found in the *Transactions* of the Royal Society, but were not brought home without peril. He found it necessary to return by way of China, and was in that well-known East India convoy which beat off the squadron of Admiral Linois. Among the college friends of the subject of this memoir was the celebrated Henry Martyn. On his lamented death in the East, Dr. Inman set out to take up the thread of his missionary labours, but learnt, while pursuing the study of Arabic at Malta for the purpose of his enterprise, that all hope of being useful in that quarter of the world was precluded by a declaration of war with Turkey. In his absence he had been nominated by the University of Cambridge to the post to which he was afterwards appointed by the Admiralty—the Professorship of the Royal Naval College at Portsmouth. It is very well known how assiduously he applied himself to the duties of this situation, how entirely he remodelled the education of our naval officers, how carefully he arranged and composed for their use many valuable works on all the subjects connected with their profession, and so entirely superintended their instruction from first to last that he has been considered by some of his more partial pupils to have fairly earned the title of the "Father of the Navy." Another consideration will give him a still greater claim to this designation.

About the year 1810, he was desired by the late Lord Melville to place the construction of ships of war upon a footing more in accordance with the principles of mechanical science. We have long lost all remembrance in how rude a state our naval construction was in those days. In a few

words, our builders scarcely ventured to build a ship except upon a French model taken from the enemy. In many cases the ribs of the prizes were preserved and overlaid with new planking. Even the weights carried by ships of war were not established with any precision; and it was not supposed that the amount and nature of these weights should have any influence upon the form of the ship's body. So far did this ignorance prevail, that nearly to the last period of Dr. Inman's career no effort of his could procure the abolition of a fatal Admiralty rule—that a ship of a certain burden must carry a certain number of guns. Many of his favourite models were rejected, and afterwards reduced, to their great detriment, to support the requirement of this rule.

None of his nautical problems, we believe, were solved free from the operation of this unfavourable condition, so great were the difficulties and discouragements under which this patient son of science laboured.

Dr. Inman's first effort in naval architecture was the translation of Chapman's *Architectura Navalis*, with notes. This work was printed at the press of the University of Cambridge. It was followed by the formation of a superior class of shipwrights; and the object of the institution, which was afterwards termed the School of Naval Architecture, was not, as many have thought, to build ships on purely scientific principles, but to educate a class of men in so liberal and sound a way as to make them the fittest instruments for determining those great problems which might afterwards be brought before them. Few facts had been ascertained—fewer experiments had been made. The exertions of Dr. Inman and his school may, perhaps, be best described as a series of experiments, conducted under the guidance of scientific principle, towards the right establishment of truth. These exertions, when they began to throw a little light upon a difficult and unknown question, raised, as might be expected, a host of competitors, who contributed, both by their errors and success, to the elucidation of the common object. They multiplied experiments; they modified generally-admitted facts. How little way had been made toward the full construction of the science of naval architecture may be learnt from the *brochure* which its founder (for he deserves the title) issued toward the close of his life; how much had been practically done is best proved by the grand specimens of "ships of war" which now carry our flag.

The curtailment of the public expenditure which followed the Reform Bill closed both the Royal Naval College and the School of Naval Architecture, and Dr. Inman returned into private life, from which he was but occasionally summoned: once, to give evidence before a Committee of the House of Commons on the tonnage question as to how the method then in use of taking the tonnage of merchant-ships affected their build, and

the best method of amending it; and a second time, when he presided over a Committee of Revision appointed to report on the best method of applying steam to men-of-war. It is said that his influence and determination mainly contributed to relieve our merchantmen from the old method of taking tonnage, and so contributed to the introduction of our modern clippers.

Dr. Inman did not receive, at the termination of his official career, any mark of approbation from the country which he had served so long and so usefully, and for which he had laboured at improvement in a matter affecting her very safety as a nation. His life was prolonged to the advanced age of eighty-three years.

Manuel John Johnson was born May, 1805. He was educated at Addiscombe for the service of the East India Company. In 1821, he was appointed to the St. Helena Artillery, and while in that island was aide-de-camp to General Walker, who encouraged his taste for astronomy. He twice visited Mr. Fallows at the Cape of Good Hope for instruction in matters connected with observing; and constructed the Catalogue of Southern Stars, which afterwards received the Society's medal, and was printed at the expense of the Court of Directors in our *Memoirs*. This labour was performed while he was in charge of a small observatory which the Court had ordered to be established, and in which operations were commenced in 1830. In 1832, the island was made over to the King's Government, and Lieut. Johnson retired on a pension. He entered Oxford as an undergraduate in 1833, and was appointed Radcliffe Observer in 1839. In this post he died, March 1, 1859. He was President of the Society in the years 1857 and 1858.

Mr. Johnson, as has been stated, was appointed by the Trustees of the Radcliffe Observatory at Oxford to the Directorship in May, 1839; but, on account of various circumstances, including the necessity of repairs to the dwelling-house, he did not come into residence till October of that year, and observations were commenced at the beginning of 1840. The instruments at this time consisted mainly of a transit instrument of 8 feet focal length and 4 inches aperture, and a 6-foot meridian-circle by Jones. There were, in addition, two achromatic telescopes, one of 10 feet focal length and 4½ inches aperture, and the other (equatoreally mounted) of 42 inches focal length and 3¼ inches aperture. The transit-instrument was for more than three years a source of great trouble to Mr. Johnson, on account of the bending of the axis; and, after bestowing upon its peculiarities a great amount of labour, and exhibiting considerable skill in the treatment of the observations made with it, he was induced, in the year 1843, to propose to the Trustees the

establishment of a new one, such parts of the old one as were available being used in its construction. This transit-instrument and the original transit-circle are in use at the present time, the latter instrument being similar in its general construction to that used by Dr. Robinson at Armagh.

The personal establishment of the observatory for ten years consisted of the Director and one assistant; and with this small staff—and, as far as the writer of this memoir is aware, without any extraneous help—the observations with both instruments were made, and reduced, and passed through the press, and published, with a regularity which is believed to be without a parallel.

On taking the appointment of Director in 1839, and carefully considering to what class of observations the resources of the Radcliffe Observatory could be most usefully devoted in the existing state of astronomy at that time, he determined to apply himself to the observation of a catalogue of circumpolar stars.

In general, there are two great branches of the science to which, either wholly or in part, an observatory may be devoted, namely, to observations of the sun, moon, and planets, or to observations of the fixed stars; and the choice must depend, if it be made judiciously, both on the requirements of the science, and the kind of instruments at the disposal of the director. At that time the planetary observations had been amply provided for by the Astronomer Royal, and the plan laid down by him for the conduct of the Greenwich Observatory left little doubt that for a long series of years this department of the science might be absolutely and solely left to it.

It is true that at that time only four small planets were known to exist between the orbits of *Mars* and *Jupiter*, and that there was not the slightest tendency to that laborious and complete sweeping of the heavens which has resulted in the addition of 53 more of those minute bodies to our list of known planets, and has rendered it necessary to divide the labour of observing them amongst various European and American observatories. But this labour cannot be undertaken at all successfully without large instruments; and even at this present time the transit-instrument and meridian-circle of the Radcliffe Observatory could not with any great effect be devoted to them. Star-observations admit of being classed in two great divisions, namely, of meridional observations for determination of their absolute places and the formation of accurate catalogues, and of double-star observations. The latter, equally with the small planets, require a large equatoreal, and were therefore, in Johnson's case, quite out of the question. The only question left then was the principle by which he should be guided in the choice of his stars to be observed with his two meridian-instruments; and, without much hesitation, he decided on observing the stars within 50°

of the North Pole, taking as his basis the Circumpolar Catalogue of Groombridge. The catalogue itself was of moderate size, consisting of rather more than 4000 stars, and was therefore fitted for an observatory with a small observing force, as giving hope of the completion of it within a reasonable time. All the stars were, at their upper transit, at a considerable elevation; and, finally, the interval of time from Groombridge's epoch (1810) gave facility for a moderately good determination of their proper motions. This catalogue, of which the observing portion was completed some years before Mr. Johnson's death, must be considered as his greatest and most important work; though we shall see in the sequel that, excellent and accurate as it is, it is far from forming the only claim to the high estimation in which his labours have been held by astronomers and scientific men generally.

The idea of the erection of a large heliometer was entertained long before it was introduced into the observatory; and, in fact, it had been ordered of Messrs. Repsold, of Hamburgh, almost at the commencement of Johnson's directorship, being mentioned in the Preface to his first volume, published in 1842, though, through delay of the makers, it was not received at the observatory till the year 1848. In connexion with this instrument, it is gratifying to learn from Mr. Johnson's own statement that the proposal of it originated with Sir Robert Peel, as did also that of the new transit-instrument, and of the purchase of the library of the former director, Mr. Rigaud.

The plan and subjects of observation having been settled, the records of the observatory for several years offer little subject for remark. The director and his single assistant, Mr. Lucas, worked with unremitting diligence by night and by day; and the results of the observations of each year were given to the world in a perfectly reduced shape with a punctuality which, under the circumstances, must be considered marvellous. In the adjustment of the instruments, and in the reduction and discussion of the observations, the most fastidious accuracy was used, and the best elements employed. The errors of division of the circle were determined and allowed for, the zenith-point for some years was determined by reflexion-observations of stars and afterwards by means of the reflected image of the wire, and the discordance between the direct and reflexion-observations of the same stars was determined from the combined results of all the star-observations with great accuracy.

But that which most deserves remark as characteristic of Mr. Johnson, was the constant aim at improvement in subjects and processes which did not seem to ordinary minds to admit of anything beyond routine treatment, and the originality of his ideas evinced in his discussion of such subjects. In proof of this it is sufficient to give as an instance his treatment of the

magnitude of the stars of his great catalogue. To appreciate this fully it is necessary to read carefully and continuously the introductions to his volumes of observations. One can see how by successive steps the subject grew under his hands; how he soon recognised by comparison of the magnitudes of the same stars assigned by himself and his assistant, that these estimates were not of the vague character which might be supposed beforehand, but that the difference of estimation — the personal equation so to speak — was as well determined as that of any other class of phenomena subject to observation; how he found it necessary, as the eye became more sensitive to small differences in the light of different stars, to adopt a decimal notation to discriminate the gradations existing between one magnitude and the preceding and following; and how, finally, the result of his labours on this subject was a complete discussion of the subject in a perfectly original manner, which connects his name indelibly with that important branch of stellar astronomy.

Before we leave the subject of his meridional observations, it will be necessary to make a few remarks on his treatment of the proper motions of the stars, though his labours on this branch of astronomy were, alas! cut short by his untimely death, and his ideas on the subject may be considered to have been only in their infancy. It is not many years since an eminent astronomer stated that he considered our ignorance of the proper motion of the stars to be the opprobrium of modern astronomy; since that time a good deal has been done, but still it is but little compared with what is necessary to be done to give the history of the heavens for this particular epoch. This is not the fault of the astronomers of this age, that is, it does not arise from any want of care and labour on their part, but from the scantiness of the materials.

The so-called proper motion of a star is that residual error which remains when its place, determined at one epoch, is compared with that determined at another, by means of the application of its precessional motion in the interval between the two epochs; and, to determine this quantity with adequate accuracy, it is necessary that the interval of time separating the two epochs be considerable.

Now there are very few catalogues, or collections of reduced places of the stars, which are separated from our time by a sufficient interval, and which, at the same time, are sufficiently accurate to enable us to deduce the proper motions with any confidence. The chief of the catalogues which can be so employed for the northern heavens are, first, that of Bradley as given in Bessel's *Fundamenta;* secondly, that of Piazzi; and thirdly, that of Groombridge. The epochs of those catalogues are respectively the years 1755, 1800, and 1810; and to obtain the proper motions of all the stars in any one of them it is necessary that they all be re-observed at a comparatively

recent epoch. For Bradley's catalogue this most useful labour has been performed by Dr. Robinson, and the results have been recently given to the world in a volume of which honourable mention will be made in another part of this Annual Report; the comparisons with Bradley have been also made by him, and the proper motions of all the stars have been deduced.

The re-observation of Piazzi's Catalogue, as a whole, is still a desideratum; but that of Groombridge has formed, as we have seen, one of the important portions of the labours of the Radcliffe Observatory under the direction of Mr. Johnson. As he was coming to the conclusion of this admirable work, and collecting results for the formation of his catalogue, his attention was naturally drawn to the subject of proper motion, and in his volume for 1856 he has given some excellent discussions of his mode of treating the subject for particular stars. He does not depend upon *one* catalogue for comparison with his own observations, but carefully collects all the results obtained by different astronomers at epochs antecedent to his own, and thus obtains ultimately a definitive proper motion by assigning weights to the partial results depending on the intervals of the epochs and the number of observations on which each result depends. This process is of course laborious, but to obtain the *best* results, or even to avoid great occasional errors arising from accidental mistakes in the results of two catalogues (if such only be used) it *must* ultimately be undertaken; and Mr. Johnson's labours, cut short as they have been by his premature death, are an excellent specimen of the method which must be employed.

In addition to the catalogue of circumpolar stars, a considerable number of stars were observed in the years 1854, 1855, and 1856, selected as having special claims on the attention of astronomers. The chief of these stars are, first, those above the third magnitude; secondly, stars within 6° of the Pole; and thirdly, those having physical peculiarities or orbital proper motion. In the volume for 1856, all these stars, to the number of 1481, are collected in a provisional catalogue, which forms an excellent basis for the researches of other astronomers.

We now come to a new epoch in the annals of the observatory, namely, the establishment of the heliometer, which had been ordered of Messrs. Repsold at the commencement of Mr. Johnson's directorship, but which, from unexplained circumstances, was not given into his charge till the end of the year 1848. A new building was erected for it in 1849, and the instrument was mounted in the October of that year. The addition of this instrument made another assistant indispensable, and the situation of second assistant was in the summer of 1851 offered to Mr. Pogson and accepted by him, though he did not come into residence till the end of the year. Mr. Pogson was at that time engaged at the South Villa Observatory of Mr.

Bishop, and had already distinguished himself by his zeal and activity in the making of double-star observations, and the choice of him was singularly fortunate for the Radcliffe Observatory. By the discovery of two of the small planets during his residence at Oxford, and by his researches on variable stars, he has established for himself a reputation amongst the most successful and zealous cultivators of the science, while his assistance to Mr. Johnson during the time of his engagement at the Radcliffe Observatory was of the greatest service at a very critical period of its existence.

Mr. Johnson proceeded with his usual vigour and skill to master the peculiarities of the magnificent instrument now put at his disposal. An elaborate description of the heliometer, with an account of the methods employed in making and reducing the observations, is contained in the introduction to the eleventh volume (for 1850). The difficulties attending the use of the instrument are increased by a defect in the relative positions of the two segments of the object-glass, which are too close to each other; and which has this bad consequence, that the moving image overlaps the fixed one, when there ought to be a coincidence of images, by a quantity at present amounting to at least 10″, and that therefore measures of very close double stars are impossible.

Mr. Johnson, who was particularly well read in German astronomy, had studied accurately the disquisition on the heliometer in Bessel's *Untersuchungen*, and for the most part uses his methods, though with due regard to the peculiarities of his own instrument.

Every refinement and correction thought necessary by that astronomer are introduced; the methods of determining the value of the scale, of flexures of various kinds, of refraction and aberration, all these matters are discussed with a rigour and an accuracy which can be judged of only by the actual perusal of the introduction to the heliometer observations in the fourteenth volume, and can only be hinted at in a brief memoir like the present.

In the year 1850, besides some measures of the diameters of *Mars* and *Jupiter*, a catalogue of twenty-six of the most important double stars was observed, but in the years 1852 and 1853 the observations were much more important and valuable, consisting, in addition to a re-observation of the chief stars in the *Pleiades*, of continuous and very numerous observations of stars which were known to have, or were suspected of having measurable parallaxes, the chief of which are 61 *Cygni*, Groombridge 1830, and α *Lyræ*. On each of these stars there is an exceedingly complete and instructive disquisition, which will repay the labour of perusal.

Mr. Johnson continued during the years 1854 and 1855 to observe with the heliometer, confining himself to the stars *Castor*, *Arcturus*, and α *Lyræ*, and the discussion of the results

for these stars is given in the volume for 1855. After this time, whether from the failing health of the director or from other causes, the instrument appears not to have been used.

Thus far we have spoken only of the astronomical labours of Mr. Johnson, but there is another department of science, namely meteorology, which is in its degree equally indebted to him. Observations of the usual elements of the weather were taken during the whole of his directorship; but, during the latter years, an extension was given both to the observations and to the discussion of them, which has gained for the Radcliffe Observatory a reputation in this department of science equal to that which it has acquired in the purely astronomical department. It is interesting, also, to observe in this subordinate subject the same tendency to progression, extension, and improvement, which has been before remarked as characteristic of Mr. Johnson in the discussion of his astronomical labours. In the volume for 1847 we find the first discussion of the meteorological observations, and, for the next ten years, these observations and the exhibition of them in the best possible shape, appear to have occupied a steadily increasing portion of his time and attention. His predecessor, Mr. Rigaud, had commenced in 1828, and had continued without intermission, a series of observations. These, in the year 1853, Mr. Johnson connected with his own, and was thus enabled to exhibit the comparison of the results for the elements of each year, with the average results for the same elements deduced from the observations of a quarter of a century. From this time each element of the weather is subjected to a very severe discussion, according to the methods practised in the best meteorological observatories.

But, commencing with 1854, an organic change was made in this department by the introduction of photographic registration. The apparatus employed was designed by Mr. Ronalds, and is similar to that at the Kew Observatory, and the management of the photographic processes was for some time after their introduction conducted by Mr. W. Crookes, a professional photographer.

There may be a doubt about the propriety of introducing into the permanent work of any establishment subjects or methods of observation, which require the executive control of professional persons not on the ordinary staff of assistants, and over which the director can exercise very little more than administrative superintendence, and the inconveniences which may arise are sufficiently obvious, but every one must admire the energy of purpose which enabled Mr. Johnson in spite of difficulties to continue this class of observations, and to exhibit the results without any interruption, up to the very time when death snatched him from his labours.

Every important part of the labours of Mr. Johnson has

now been discussed with sufficient detail,—a detail in fact which may prove wearisome to those who are not personally interested in him—to enable any one to understand the nature of the qualities which he brought to bear upon the administration of the Radcliffe Observatory. To a vigour of body which enabled him to endure for many years almost uninterrupted fatigue, and a powerful and well-trained intellect which enabled him to grapple successfully with difficulties, and to keep pace with the astronomical activity of his epoch,—he united an inflexible integrity and honesty of purpose, which forced him to give all his time and energy to the duties of his office. In looking at the observations of many consecutive years in which he took upon himself the duty of observing with one of the meridian instruments, it is difficult to find a period of even a fortnight in which he could have been absent from the observatory. It is now too late to deprecate this incessant labour in dull routine processes which could have been performed by an assistant, if his staff had been larger; but we cannot but admire the resolute perseverance and the patient confidence in the value of his work, which, without much present encouragement, animated him with hopes of the future, and induced him year after year to devote his days to the reduction of the observations which he had himself made by his nightly labour.

There is no doubt that to him labour was a necessity of his being, and that his application of it in the duties of his office was limited only by his physical capacity for endurance. But, for his friends at Oxford, who cannot so well appreciate all the excellencies of his professional character, there are other qualities to dwell upon; and the University of that city will long venerate his memory for those endowments of mind and heart that made him so popular while living, and, now that he is gone, regretted with so deep a sorrow. There was a largeness of heart about him which so well accorded with his largeness of view and depth of purpose, that the whole character was as well balanced as it was soundly composed. His social qualities were of the first order. Kindness of feeling towards others, combined with frank and agreeable manners and much love of jocularity, made him a pleasant companion in society and opponent in conversation. Though of very decided opinions and uncompromising maintenance of them, his mode of conducting himself did not leave behind it the sense of past difference. By what is a most excellent gift, not always possessed by men as worthy as himself, his thorough honesty and singleness of mind were made apparent by his conversation and demeanour. All these things put together added weight to his opinions and power to his representations. His memory will be cherished by all who knew him, as well in our Society as in the University of Oxford.

The Council cannot announce the name of Mr. Johnson's successor at the Radcliffe Observatory; no appointment having yet been made.

JOHN PRINGLE NICHOL was born in the year 1804 at Brechin in Forfarshire. He completed his education at King's College, Aberdeen, where he acquired the reputation of a first-class student. At an unusually early age he was appointed schoolmaster of the parish of Dun. From Dun he removed to the town of Hawick; from Hawick to Cupar; and from Cupar to Montrose. While engaged in teaching he studied with a view to the Church, but the tendency of his mind was rather towards scientific and literary pursuits. In 1836 he was appointed Professor of Practical Astronomy in the University of Glasgow. He wrote several works on popular astronomy of considerable merit, and was eminently successful as a lecturer on Astronomy to a popular audience. He died at Rothesay on the 19th of September, 1859.

Professor Nichol possessed a kind and genial disposition, which made him a favourite with all classes of society. He was twice married, and has left a son and daughter by his first wife.

JOHN ASHTON NICHOLLS, who died at the premature age of 36, was a native of Manchester; at the New College in which city the higher branches of his education were received. He was well versed in both the theory and practice of chemistry; and, besides establishing a small laboratory for private investigation, he frequently gave chemical lectures to Mechanics' and other similar institutions, illustrating the subject by popular experiments.

To the science of astronomy Mr. Nicholls devoted a large share of his time and study. He possessed an observatory furnished with a good refracting telescope, equatoreally mounted, a transit, and other instruments, mostly of the manufacture of Mr. Dancer of Manchester. Unfortunately, however, his health did not prove sufficiently robust to withstand repeated exposure to the nocturnal atmosphere; and he was reluctantly compelled to abandon the attempt at a systematic course of practical observation. He was well known to many astronomical amateurs in the district, and became a Fellow of our Society in June 1849.

But undoubtedly, the object which he had most dearly at heart, was the moral and social improvement of his fellow-men, and especially of the so-called working classes. Here he began *ab initio;* and, while educational efforts of every kind found in him an active friend and supporter, he was principally interested in the schools established under the Factory Act for the instruction of children employed, during half the day, in cotton-mills. And so convinced was he of the advantages of a

compulsory system of education, that he contended for the extension to *all* sections of the operative community, of the enactment which provides, in factory labour, that no child shall obtain remunerative employment without having first secured a schoolmaster's certificate.

Among the numerous lectures which he delivered in various parts of the country, two well deserve preservation. One was a discourse upon Public Health, printed at the request of the Manchester and Salford Sanitary Association; the other was a lecture on Strikes (also published) delivered to a large audience of working men in Manchester, and the simple, though not superficial, exposition of facts bearing on *both* sides of the question, which he then laid before his hearers might have been circulated in London with infinite advantage during the recent unhappy dissensions between the master and operative builders.

For about two years before his death Mr. Nicholls held the position of Honorary Secretary to the Manchester Athenæum; a post which, though honorary, entailed very arduous duties upon its occupant. That excellent institution had not previously been prosperous; and, it is universally admitted that the beneficial change which it has undergone is largely owing to the indefatigable attention of the late honorary secretary.

He visited the principal countries of Europe, as well as Canada and the United States: but his object in travelling was rather to examine the condition and institutions of the people than the nature of their country. His observations, communicated from America in the form of letters to friends and to a local newspaper, upon the rapid development of border settlements in the States, with critiques on their systems of finance, were highly interesting.

On domestic policy his views were decidedly liberal, though by no means *ultra;* and, while consistent in his own religious belief, he would always extend to all others of different faith, that perfect freedom of action and expression of opinion which he claimed on his own behalf. He took a prominent and successful share in the elections for his native city, and was himself invited by an important section of the constituency of Nottingham to become a candidate for the representation of that place. This invitation, though offering the opportunity of embarking in a career for which he had both taste and qualification, he felt bound to decline; being unwilling to throw additional labour upon his father, with whom he was engaged in business.

The recollection of his private worth, of the kindness of his disposition, and the charm of his conversation, to which a fund of natural humour, aided by the resources of varied and extensive readings, gave a brilliancy not often equalled, will not soon be effaced from the memories of those who enjoyed the privilege of his personal friendship. His funeral was most [redacted]ly attended by mourners of every grade; and the

monument, which is about to be erected to his memory *by working men*, will testify alike to the excellence of his brief career, and to the appreciation of his labours by those for whose benefit his earnest efforts were so disinterestedly bestowed.

James Adey Ogle was born in 1793, and was entered at Trinity College, Oxford, in 1810, where he devoted himself chiefly to mathematical studies, and took the degree of B.A. with a first class in mathematics in 1813. He graduated M.A. in 1816, and became soon after tutor of his college, taking chiefly the mathematical department; meanwhile he graduated in medicine (B.M. 1816, M.D. 1820). He married, but retained his tutorship; in 1822 he was elected F.R.A.S., and in 1826 F.R.S., he was appointed public examiner in mathematics 1825. In 1827 he was an unsuccessful candidate for the Savilian Professorship of Geometry; on which an eminent medical friend congratulated him, as success would have debarred him from all professional advancement, and he must have been contented to sit down for life on 300*l.* a-year.

He now entered upon medical practice; and, having been previously nominated Aldrichian Professor of Medicine in 1824, he succeeded to the Clinical Professorship in 1830, and lastly was appointed Regius Professor of Medicine in 1851. He had before this time been left a widower with a large family. One of his daughters became the wife of the late lamented Radcliffe Observer, Mr. Johnson. Dr. Ogle died in 1857.

Robert Stevenson, F.R.S.E., an eminent civil engineer, was born at Glasgow in 1772, and died at Edinburgh in 1850. By the death of his father, who was a West India merchant, he was left, in infancy, in very straitened circumstances; but, by the self-denying energy of his widowed mother, he received a good education, and even acquired some tincture of classical learning. He had, however, such a decided liking for the physical sciences, and early conceived so great an admiration of Smeaton, that he determined to adopt the profession of a civil engineer. Notwithstanding many disappointments, which he bore with fortitude, he was enabled to prosecute his studies first at the Andersonian Institution, then just opened at Glasgow, and afterwards at Edinburgh under Robison, Dugald Stewart, and Playfair. In 1797 he became engineer to the Board of Northern Lighthouses, and held that office for nearly fifty years. During that period he erected no fewer than 23 lighthouses, of which one was a Tower on the Bell Rock, off the east coast of Scotland, where the foundation is covered at spring-tides to the depth of twelve feet. This was a most arduous work, and made large calls, which were most manfully met, on the courage and perseverance of Mr. Stevenson, who also showed, in its design and execution, great mechanical skill

and practical wisdom. Following the model of Eddystone he yet introduced some important improvements into his design; and, in particular, he turned the floors of the apartments, whose *outward thrust* Smeaton had counteracted by an ingenious device, into direct ties for the outer walls of the tower. He, moreover, greatly improved the optical and mechanical arrangements of lighthouses in general; and added some useful distinctions for enabling the mariner to recognise the lights on a line of coast. As a civil engineer he executed many important works in various parts of the United Kingdom, and he surveyed and traced the lines of many canals and railways which have since been made in accordance with his views. He also gave his advice in many cases of harbour and river navigations both in Britain and Ireland. We owe to his ingenious suggestion the form of suspension-bridges for small spans, by which tall piers are dispensed with. For timber bridges he also prepared a simple construction of arch, in which the *ring-course* consists of layers of thin planks bent to the circular form, and stiffened by means of the *king-posts* which support the roadway of the bridge. This form is now much used for railway bridges. He was, besides, the first person who pointed out the advantages of using malleable iron for the rails of our *iron ways*. His observations on the changes observed in the bed of the German Ocean and on the wasting effects of the sea on the shores of Great Britain show how much his mind was given to physical inquiries. He also, while engaged on the survey of the river Dee, in Aberdeenshire, designed an ingenious instrument for drawing water from the bed of the estuary and bringing it to the surface, unmixed with the upper waters, thus showing that the salt water flows under the fresh.

Mr. Stevenson was a man of great sagacity, fortitude, and perseverance, and was early remarkable for his scrupulous punctuality in all his engagements. When at work he allowed no interruption to take off his thoughts: and this habit he preserved throughout that portion of life when most men plead age as an excuse for indulgence and love of ease. His mind was ever guided by an unobtrusive and sincere piety. His love of independence and his gratitude for kindness were remarkably exhibited in the fact that he devoted the first humble rewards of his professional labours to the payment of what he considered a debt to a charitable institution, of which his mother was compelled, by her difficulties, to take advantage for his education in his childhood. The memory of his early struggles deeply impressed him, and produced unwearied efforts to forward the progress of young men through life, a duty from which no thought of trouble or expense ever made him shrink.

The Council do not attempt any detail of the lives and

labours of the two celebrated engineers, Isambard Brunel, and Robert Stephenson, who died, the first on the 15th of September, aged 54, the second on the 12th of October, aged 56. When men of such note, of whom there is not much to say in direct connexion with astronomy, pass away from among us, their lives are given to the public through many channels, in a manner which would render detail superfluous. These two men lived at a fortunate time for the character of an engineer, the time in which the railroad system was brought into operation. How much both, but especially Stephenson, had to do with its first successes, is well known to the world. It more concerns us to note that their time was also that in which the peculiar knowledge of the engineer began to be of use to the astronomer; and it may be presumed that both Brunel and Stephenson saw the connexion when they joined our Society. The approximation of the physical sciences to one another, and to the arts which should always have depended upon them, is one of the characteristics of our day. The Society will always have reason to feel proud that it has enrolled such names as those of which we now speak. And though the Council do not feel competent to lay before the Society an account of their labours, worthy of their reputation, they may at least join the rest of the community in showing that they know how to appreciate their high intellectual and moral worth, and their unspeakable utility to mankind.

In the ordinary operations of the Royal Observatory, Greenwich, very little change has taken place since the preceding year. The observations are precisely of the same character as before, and the reductions have been made to keep pace with the observations. The volume of Observations for 1858 has been very nearly passed through the press, and will be ready for publication in a short time. The printing of the volume for 1859 has commenced.

The galvanic operations of the Observatory have been carried on without material alteration, but considerable difficulty has been experienced in keeping up the time-communication along the lines of railway and for the drop of the Deal ball, on account of the bad state of the wires leading from the Observatory to the Lewisham station of the North Kent Railway. These wires pass underground, and the difficulty of examining and repairing them is found to be so great, that preparations are being made for carrying another set above ground across the Park and through the town to the station of the Greenwich Railway.

It has been the custom at the Royal Observatory, since the year 1835, at the end of every sixth year to collect the results of the observations of stars and of the sun during the preceding period of six years, and thus to form a new and independent star-catalogue, with independent place of equinox. The fourth

of these six-yearly periods expires with the termination of 1859; but, as there is reason to think that in the course of another year the observations necessary for forming a complete catalogue of stars of the 5th magnitude visible at Greenwich may be finished, the Astronomer Royal has decided to defer the preparation of the new or seven-year catalogue to the close of the year 1860.

The printing of the Reduction of the Greenwich Lunar Observations from 1831 to 1851 (in continuation of the Reductions from 1750 to 1830, and on the same general system of calculations and the same elements of tables, as far as circumstances permitted) is now advancing. The principal object in these Reductions was, to deliver the annual results of the Greenwich observations from the petty inconsistencies which had crept in by reason of the uncertainty on the elements and the theory of the moon's parallax (an uncertainty from which we are at last delivered by the researches of Professor Adams), and from the small uncertainty on the equinox; and to compare these with a lunar theory more legitimate in its form than that of Burckhardt's Tables. Every observation for a period of *one hundred and two years* is now computed by the same elements of reduction and compared with the same tabular elements. It will not be necessary to continue these reductions beyond 1851, as the parallax may be considered as established, and as Hansen's Tables are now applicable (and partly applied) to the years commencing with 1852. In the course of printing, some small errors of computation have been discovered; and these will effect, in an inappreciable degree, the numerical corrections of lunar elements given in the supplemental *Monthly Notice* for 1859.

During the last year several computers have been employed on the principal reductions of the magnetical observations made at the Royal Observatory from 1848 to 1857. They are based entirely upon the photographic registration of the positions of the three magnetometers; and will present, it is believed, the first example of reductions so made. The measures taken for every hour are not the ordinates of the photographic curves, but the ordinates of pencil curves traced by hand as representing the general sweep of the curves, suppressing inequalities of very short period. It is hoped that, in this way, results of considerable value may be obtained. The ordinates have been taken both for solar hours and for lunar hours; and are so classified as to present, on both systems, the monthly means for days and for hours. The calculations are now far advanced.

The Great Equatoreal at the Royal Observatory will, it is anticipated, be shortly ready for use. Much delay has arisen from the tardiness of experiments on the instrument and its driving clock, from the personal occupation of the Astronomer Royal on public business not connected with the Observatory, from the pressure of other business on the opticians, and finally

from little errors of measure derived from the German opticians. It is expected, however, that the whole will soon be in a state fitted for active employment.

From the Royal Observatory, Edinburgh, we learn, that since the commencement of the present year, the restriction imposed in August 1858 on the printing and free distribution of a certain number of copies of the Observations, has been withdrawn; and the typographical work has accordingly been recommenced.

In December 1859, Sir W. Gibson Craig, President of the Edinburgh Board of Visitors, made a personal application at the Home Office, and effectually proved to the Secretary of State—that the additional expense of paper and press-work for printing off 250 free copies, in place of the authorised 50 only, was exceedingly small: so small, indeed, as actually to be covered by the value of the publications annually sent to the Observatory from its numerous correspondents, observatories, societies, and scientific individuals, at home and abroad; and sent in most cases, under honourable expectation of receiving in return a similar free gift of the Edinburgh Observations.

In consequence of this explanation, the full significance of which was appreciated by Sir G. Cornewall Lewis, the Comptroller of the Stationery Office has been instructed to allow the printing of the twelfth Edinburgh volume to proceed on the same plan as its eleven predecessors; and from the second week of last month it has been proceeding accordingly.

In the course of the last summer, while the settlement of the above question was still in abeyance, the Edinburgh Astronomer took the opportunity of a friendly offer from the owners of the Leith and Cronstadt screw-steamer Edinburgh, to make a voyage to Russia for the double purpose of testing his improved free-revolver apparatus in the observation of altitudes at sea, and of perfecting his acquaintance with practical astronomy by a visit to the already classic establishment of Pulkowa.

There, he was most hospitably entertained for many weeks; and had the peculiar pleasure of witnessing, one day on the banks of the Neva, and in company with the astronomers of Pulkowa, St. Petersburg, Siberia, and many other places, of assisting at the reception of the venerable father of astronomy in Russia, W. Struve, on the occasion of his return to his own country, after his long severe illness and convalescent tour. He had been absent nearly a year; or, in the terms of local illustration, had left on the 30th of August *old style* 1858, and had come back on the 30th of August, *new style* 1859. His health now happily re-established, he received affectionately and nobly the earnest protestations of regard from his many

devoted pupils, who had come up from distant parts of the great empire; and his countenance—said to be more genuinely Anglo-Saxon than anything that England can show—was lighted up with a benignant smile, as he addressed a few words of excellent English to the British stranger in the crowd.

A residence for so long a time at Pulkowa, first under the direction of M. Otto Struve, and then of his father, could not but give an inquiring visitor many new ideas, and confirm some old ones if good. Among the latter the Edinburgh astronomer is happy to class the eminent qualifications of the official head of astronomy in Great Britain; for these appear to be as extensively and emphatically praised, and as continually heard of in Russia as in England; what Mr. Airy has said, or what he has done, being always looked to with the greatest interest and respect. Among the former, besides various methods of eliminating from the instruments and observations, minute temperature effects and personal equations, there was a method of procuring equability of clock-rate under rapidly varying atmospheric circumstances, which recommended itself so powerfully from Mr. W. Struve's statement of the principle, as well as from its practical performance in the intensely changing climate of Russia, that a Pulkowa pendulum was asked for, and brought away, for application to the Brisbane transit clock of the Edinburgh Observatory.

A letter from Pulkowa was found to be the best sort of introduction in travelling through Russia: for the astronomers, wherever they were stationed, were always at home; while Prof. Sawitch at St. Petersburgh and Prof. Schweizer at Moscow vied in their kindly attentions with the Struves themselves. Very interesting it was to find the former gentleman with Sir John Herschel's collected reviews before him, and fully appreciating the ring of their true eloquence and lofty views; and equally interesting, though in a perfectly different manner, was it to hear Prof. Schweizer describe his extensive operations for determining the local attraction about Moscow,—a comparatively flat country, yet with twelve seconds of deviation of the plumb-line existing over several square miles, and with the observatory accidentally pitched over just the focus of the disturbance.

Of the mountains in the South of Russia, important particulars were given, and are soon to be published by General Chodzko, who was encamped with 300 men on Mount Ararat between the two peaks the summer before last; and made geodesical observations on the summit, 16,000 feet above the sea. These experiences, combined with the result of the Teneriffe experiment, led M. Otto Struve, after reading both the popular book and the Admiralty paper on Teneriffe, to think so well of mountain astronomy, as to report to the Russian government on the advisability of establishing a new obser-

vatory at Tiflis, with a special view to the astronomer taking his instruments each summer up into the elevated and transparent atmosphere of the summit of the sacred mountain.

The minister having been favourably impressed with the project, transmitted it to the Emperor; who sent for the general, and after a favourable interview, empowered M. Otto Struve to carry out the whole arrangements. The consequence was, that within a few weeks after the first idea was submitted to the Russian Government, the realization of it was actually begun before the visitor's eyes; and on so efficient a scale, that 5000*l.* was given for the erection of the permanent observatory, and 2500*l.* for the purchase of the instruments.

To this new post in Russian astronomy, M. Oblomicosky has been appointed; and it may be confidently expected that he will reap, in his annual ascents from Tiflis, most of the results that a similar following up of the Teneriffe experiment would have earlier given.

The sympathetic appreciation of the Pulkowa astronomers was shown even more strikingly still with the Edinburgh astronomer's attempts to apply the principle of free revolution, to enable altitude observations of stars to be procured at sea. Some parts of his apparatus, which had given way on the voyage, were repaired in a first-rate manner by the intelligent mechanical artist of the central Observatory, M. Brauer; and it was then, with consent, shown to two Russian Admirals (Admiral Lütke and Admiral Zailyónee) of scientific eminence. These gentlemen were so much pleased with what they saw, as to call the attention of the Grand Duke Constantine to the subject; and he having had the instruments sent to his Palace of Strelna, and given them a full examination during two hours' trial, decided on endeavouring, with the assistance of the astronomers and mechanicians of Pulkowa, to adapt them to practice at sea. His Imperial Highness had already perceived with regret the contracted limits for star observation on shipboard; and having on sundry occasions attempted to improve the existing state of things, but without attaining all his wishes, he adopted with enthusiasm the free-revolver appliances, when brought before him on this occasion.

With eminent mechanical appreciation, the Grand Duke separated in the machine before him the effects of imperfect workmanship and deficient proportions from the scientific principle involved in it; and considering that he had in the latter what would enable him to realise his long-sought hope, of a naval instrument with sufficient optical power to show, even through thin haze, a bright image of a 2d–3d mag. star, and with mechanical arrangements that would enable the angular altitude to be ascertained with the solar precision of a sextant, he at once gave M. Otto Struve *carte blanche* for undertaking the manufacture of the new instrument, even to the extent of

any number of preliminary experiments on materials and proportions.

At the Cambridge Observatory, during the year 1859, the observations of the small planets, both meridional and extra-meridional, have been discontinued for the purpose of gaining time for the reductions of the observations of the preceding year. The meridian instruments have been employed only in obtaining the places of stars employed in past years for comparison with planets and comets. Comets are still observed with the Northumberland equatoreal, and occultations of stars by the moon are not neglected.

A portion of the dividends of the Sheepshanks' Donation, which is mentioned in another part of this Report, having been appropriated for the payment of two computers to assist in the reduction of the observations of past years, this work is proceeding continuously with great vigour. The printing of the meridian observations for 1852 is in a great measure completed, except the comparison of observed places of planets with tabular places. Professor Challis is devoting considerable time and labour to the comparison of the observed places of the small planets with tabular places, calculating places from the elements where necessary.

The Council have to announce the retirement of Captain Jacob from the superintendence of the Madras Observatory. The place of this zealous astronomer has been supplied by the appointment of Major Tennant, of the Bengal Engineers.

Mr. Hartnup states, in his Annual Report to the Mersey Docks and Harbour Board, that the astronomical and meteorological observations taken at the Liverpool Observatory during the past year differ in no material respect from those mentioned in his Report for 1858. The observations taken with the self-registering anemometer and rain-guage have been tabulated for each hour of the day throughout the year. The Greenwich mean time has been regularly communicated to the port by the dropping of the time-ball and by the regulation of the electric clocks. The large clock in the Victoria Tower, which has six dials, each of which is eight feet in diameter, is now under the control of the normal clock at the observatory; and the first blow of the hammer at each hour of the day indicates Greenwich mean time with all the accuracy that can possibly be required for nautical purposes. On a calm day the striking of this clock can be heard at the observatory, and it is frequently compared with the normal clock. The time which sound takes to pass from the clock to the observatory is 2·6 seconds; and so perfectly does the small clock at the observatory control the large one at the tower, that this

difference scarcely varies the tenth part of a second. The striking, Mr. Hartnup says, is not, however, the most convenient test of the accuracy of its performance. The large clock is made to send a galvanic current once every minute, and by so doing the needle of a delicate galvanometer is deflected at the observatory, and the deflection of this needle is found to be quite simultaneous with the beat of the normal clock. Mr. Hartnup is not aware that any clock so large as this was ever before made to perform with such marvellous accuracy. The new time-ball at the Victoria Tower, which is just completed, is 5 feet 6 inches in diameter, and when raised to the top of the mast it is 134 feet from the ground. The accurate dropping of this ball will not, as is the case with most time-balls, be affected by any interruption in the passage of the electric current at the instant of one o'clock each day. The clock at the tower, being controlled by electricity, is made to drop the ball by a mechanical arrangement; and in the event of an accident to the wire of such a nature that no current would pass, even for half-an-hour, the clock, being carefully regulated, would, Mr. Hartnup says, not vary sensibly from correct time. The number of new chronometers sent to the observatory to be tested previous to their being sold has become so great, that the charge for testing was increased, on the 1st of March last, from seven shillings and sixpence to twenty-one shillings. Notwithstanding this, Mr. Hartnup states that the number of chronometers at the observatory on the 1st of January, 1860, was considerably greater than on the 1st of January, 1859; and the subject of making extensive alterations, to facilitate and render more perfect the working of this department of the observatory, is now under the consideration of the Mersey Docks and Harbour Board.

Mr. Lassell has proceeded successfully with the equatoreal mounting of his 4-foot reflector, of which a drawing is now on the table. With the single exception of the figure of the great speculum not being as yet quite brought up to the standard of the 2-foot reflector, the instrument is entirely finished, and in all its mechanical arrangements and contrivances fully equals or surpasses Mr. Lassell's expectations. It requires, indeed, two assistants, one of whom is almost entirely occupied in driving the telescope in R.A.; his duty being simply to turn a handle once round with every beat of a neighbouring pendulum, beating approximately sidereal seconds. This arrangement, originally intended to be but temporary, answers so well that Mr. Lassell has not as yet attempted the contrivance of any machinery which shall do the thing better. A labourer of ordinary intelligence almost immediately acquires the art of carrying on the telescope so that a star remains, even with a high power, apparently immoveable in the field. The other assistant is chiefly occupied in moving on the turn-

table which carries the tower, with occasionally a motion of the tower itself, which he has the means of doing so gradually and steadily as not to distract the observer. The four stories of the tower and the supplemental stages adapted (book-shelf fashion) to each, render it easy to adjust the observer's position to any required altitude of the telescope; and the amount of shelter from wind and dew, even at a high elevation, is such that Mr. Lassell could often fancy himself observing within the dome of the 2-foot equatoreal.

A moderate breeze has no sensible effect upon the stability of the telescope, nor does the moulding and whirling of the image seem to be proportionally so great in this skeleton tube as in the entire tube of the 2-foot telescope under its dome. On this latter point, however, more observations are required to speak positively.

The position of the axis of the tube has been so happily chosen that after all the furniture has been attached no counterpoise, properly so called, has been found necessary. The tube, when unlocked in polar distance, has just that slight tendency to the zenith which will insure the speculum-end always being the lowest.

Notwithstanding the size and weight of the telescope, the observer has the means, while his eye is at the eye-piece, of moving it himself small quantities of polar distance—a most essential condition, to enable him to place the object in any required part of the field of view.

The unusually violent gales of this winter have supplied a pretty severe test of the stability of the whole erection, especially of that of the tower, which has not suffered any injury whatever.

The speculum has a system of levers to prevent flexure at low altitudes, similar to those applied to the 2-foot, excepting that in these the short ends are introduced into cavities cast in the back of the speculum, combining the advantages of firmer attachment and a nearer approach to its centre of gravity. Their efficiency is shown by the penumbræ of stars near to focus appearing truly round.

The speculum was lately withdrawn from the tube, and in the laboratory, where Mr. Lassell was endeavouring to give to its surface the requisite amount of perfection; but, in a more recent communication, Mr. Lassell says that the speculum has recently come out of the laboratory with apparently a very fine figure; and, so far as the results of a somewhat interrupted starlight evening enable him to judge, he thinks that he may pronounce the figure perfectly satisfactory. At any rate, it has shown him some objects so as he never saw them before.

The observatory erected by the Rev. W. R. Dawes at Haddenham, Bucks, has recently been enriched by the acquisition of a larger and new Equatoreal of great excellence, the

mounting of which abounds with ingenious mechanical contrivances to insure perfect steadiness and uniformity of clock-motion. The instrument has been amply described by Mr. Dawes, page 60 of the current volume of our *Monthly Notices*; and the whole reflects the highest credit upon the makers, Messrs. Alvan Clarke and Sons, of Boston, U.S.

Mr. Carrington's attention has been mainly directed to the subject of the solar spots throughout the year 1859. Though absent from home on five days out of each week, he succeeded in registering the state of the sun on seventy days; and his present assistant has nearly concluded the reduction of the recorded positions, which, at a suitable time, will be laid before the Society. Several weeks were spent on the records of Dr. von Sœmmerring, an account of which was given in the last number of the *Monthly Notices*. Mr. Carrington purposes, in the next place, to employ his assistant in laying down graphically the variations in declination of the magnet, as recorded in the Greenwich volumes, from the commencement of the year 1854, a piece of work in which a beginning has been already made, with the object of (if possible) tracing in some closer detail the relation which has hitherto been only generally established.

The very fine appearance presented by the sun's disk at the end of July last led to the insertion in the public newspapers of several letters from amateurs residing in different parts of the country, and, among others, one from Mr. R. S. Newall, of Gateshead, whose name is better known in connexion with submarine telegraphy. This gentleman, while finding some fault with what he deemed the insufficient success of more professed astronomers in the yet obscure though advancing subject of solar physics, mentioned the possession of a series of solar observations made by himself, and extending over a period of eight years. Mr. Carrington wrote to Mr. Newall a letter of inquiry respecting these alleged observations, but has not yet received a reply.

It is to be hoped that this Society will receive information of all records of solar phenomena which may possess value in themselves, and tend to elucidate the observations of others.

The Hartwell Observatory, under the management of Mr. Pogson, has been chiefly devoted to observations and researches upon the variable stars. An atlas comprising all the known or justly-suspected variables, together with the chief recorded temporary stars, is in a forward state of preparation. The want of such a work of reference has hitherto impeded the progress of this interesting branch of sidereal astronomy, and Mr. Pogson has long felt desirous of supplying the deficiency. For each telescope variable a map of 1° $20'$ square will be provided, giving *all* stars down to the 12th magnitude; suitable

comparison-stars being selected, and their magnitudes carefully determined and tabulated.

Nineteen of these maps are ready for engraving at the present time. For the naked-eye variables convenient maps will also be provided, containing all stars visible with an ordinary opera-glass. The atlas will be followed by a complete catalogue, and detailed history of its contents.

In addition to this as a chief work, twenty-two telescopic variables have been under continuous observation for the determination of their elements of change. Of these, although in some cases the light curves extend over a period of seven years, Mr. Pogson prefers to await the accumulation of a satisfactory number of maxima and minima to making premature announcements of approximate epochs and periods, soon to be superseded by later results.

The annual ephemeris of the variable stars, now in the third year of its existence, though useful to all persons interested in the subject, is especially intended to aid the meridian observer who desires to fix the positions of these objects with the precision requisite for investigating their proper motions, and yet to be spared the loss of time and labour occasioned by picking up wrong stars, the inevitable result of searching for them at any other time than near their maximum brilliancy.

The ephemeris of the magnitudes of the minor planets—also a work of great utility to the meridian observer, by forewarning him what sized object to expect—has now been adopted by the superintendent of the *Nautical Almanac* in the annual supplement of that work. The revision by observation of this ephemeris, and the determination of the constant M (mean opposition magnitude) for the later-found planets, is a further object continually borne in mind at this Observatory.

A careful examination of the space intervening between the Berlin academical charts and those published by Mr. Bishop is kept up by the aid of Mr. Pogson's manuscript charts, constructed in his leisure time while attached to the Radcliffe Observatory; and, numerous as the family of asteroids has already become, additions will, it is hoped, ere long result from the constant scrutiny maintained for this purpose.

In compliance with the Astronomer Royal's recent circular, the ephemeris-errors of such of the minor planets as are re-observed at Hartwell are communicated to Greenwich immediately on their recognition. Except for this purpose and the comparison of the magnitude-ephemeris, however, observations of the known asteroids form no part of the routine of the Hartwell Observatory. *Lætitia*, *Lutetia*, *Mnemosyne*, *Parthenope*, *Polyhymnia*, and *Proserpine*, have thus been observed, the ring-micrometer being usually employed in consequence of the minute size of the objects sought.

Measures of the interesting binary star γ *Virginis* so long and ably followed up by Admiral Smyth, were obtained with

the ocular crystal micrometer on seven nights during its apparition in the spring of 1859.

The Equatoreal, erected upwards of twenty years back by the late worthy member of our Society, Mr. G. Dollond, has during the last summer been overhauled by his able successor the present Mr. Dollond, and may now be considered in fair adjustment. The excellent Clock, made expressly for this observatory by the late eminent Mr. Vulliamy, has also undergone a thorough examination by the distinguished member of our Council, Mr. Charles Frodsham, and has since performed admirably. Until last October the observatory had the benefit of a marine chronometer, most courteously lent by the Royal Geographical Society, when from some unknown cause the mainspring snapped, and the chronometer has been since returned with thanks to the Society. The 5-foot transit instrument, constructed by the late Mr. T. Jones, F.R.A.S., has been employed exclusively for the requisite determination of local time.

The meteorological observations have been carried on as in former years by Mr. Samuel Horton, with his usual diligence and accuracy, and are regularly forwarded to the Registrar General for publication in his quarterly reports.

Mr. Pogson has now been a regular contributor to the proceedings of this Society for more than twelve years. For four years previous to his holding any official capacity as a professional astronomer, he laboured diligently at the calculations of the orbits and ephemerides of several comets and new planets under the guidance and kind encouragement of his friend and fellow-townsman Mr. Hind. In 1851 he was engaged as assistant at the South Villa Observatory, to aid in the zone observations for Mr. Bishop's ecliptical charts. At the close of that year Mr. Pogson entered the Radcliffe Observatory at Oxford as transit-observer, from which date all the determinations of right ascension up to the end of 1858 were made by him. Of his official duties in that capacity the seven successive volumes, xiii. to xix., are worthy and sufficient records, but it is of his voluntary and ex-official labours with the 10-foot equatoreal, that we have here to take especial notice. The limited leisure available after the hour for closing the meridian instruments was employed with such method and success as to achieve results of the highest importance. Numerous observations of the minor planets, published in the *Astronomische Nachrichten;* the first discovery of the planets *Isis*, *Ariadne*, and *Hestia*, the first of which was rewarded by the Lalande medal of the Académie des Sciences; the independent discovery of *Amphitrite*, and the records of five other undoubted planets, lost only in consequence of the disadvantages occasioned by the equatoreal work being subordinate to his meridian duties; the construction of seven charts subsidiary to Mr. Bishop's, but extending to the 12th magnitude, all unpublished, though long since ready

for the engraver's hands; the discovery of ten new telescopic variable stars; an extensive series of observations of his own, and other variables, also unpublished; photometric investigations and other observations of occasional phenomena; all these were facts actually accomplished with a very roughly mounted instrument in the open air exposed to wind and weather, without the highly efficient and comfortable equipments now placed at his disposal in Dr. Lee's Observatory at Hartwell. Mr. Pogson entered upon his new duties as Director of this last-named Observatory on January 1st, 1859, and already a valuable work is announced as in fair progress for the promotion of a branch of our science too long neglected in this country.

Mr. De La Rue has continued to devote his own Observatory to the photography of celestial objects, and he has with the aid of Mr. Beckley, assistant at the Kew Observatory, brought the photoheliometer and the process employed to that degree of perfection that a photograph of the sun can always be obtained under favourable conditions. Mr. De La Rue read at the British Association a report on celestial photography, describing the methods employed, which will appear in full in the forthcoming volume of Reports.

Mr. De La Rue has proposed to the Royal Society to undertake experiments, having for their object to determine if it be possible to considerably increase the aperture of reflecting telescopes in proportion to their focal length with advantage in celestial photography.

Monsieur Goldschmidt of Paris, the indefatigable planet-hunter and discoverer of twelve of these bodies, was, we regret to state, compelled to retire from his labours temporarily for the benefit of his health, and to seek change of air by a residence in the country.

He has, however, still pursued his astronomical recreations; and the most singular feature yet known in the history of the wonderful variable star U *Geminorum*, which has so unceremoniously cheated everybody by its premature return one-fourth of a period before its expected time, is entirely due to his vigilance and zeal. His interesting variable star ι *Virginis*, too, proves to be one of short period, being now visible and approaching another maximum.

Mr. Baxendell, of Manchester, in addition to a steady adherence to his selected objects of research amongst the variable stars, has recently added one more to the list, of his own discovery, in the constellation *Sagitta:* changing from the 8·4 to the 9·8 magnitude in about 74 days. His observations are chiefly made with the instruments of Mr. Worthington's Observatory; sometimes with the achromatic equatoreal, at others with the large Newtonian reflector, 13 inches in aper-

ture, and judging from Mr. Baxendell's observations evidently of very superior quality and performance.

In regard to astronomical instruments, the principal novelty of the last two or three years is the introduction, by MM. Steinheil and Leon Foucault, of a method of constructing the mirrors of reflecting telescopes by grinding to the proper form a disk of glass, and depositing upon it a coating of metallic silver, by a chemical process which has been explained to the Society by our Secretary, Mr. De La Rue. The Astronomer Royal had the good fortune in the past summer to make a careful scrutiny of a large part of the moon's surface with one of M. Foucault's telescopes at the Imperial Observatory of Paris, and speaks in very high terms of its performance. The image was very brilliant, and many minute and delicate shadings, both on the volcanic and on the more level parts of the moon's surface, were brought out with a distinctness which he had never before witnessed. The reflector in question is made of a rather thin disk of glass, which bends by its weight, and a singular contrivance is used for keeping it in form. An air-cushion of caoutchouc is placed behind it, from which a flexible pipe with a stopcock is led to the observer's mouth, and the observer blows air in till the image is perfect. If he blows in too much the distinctness is lost, and a little air must then be allowed to escape. The success of this contrivance is complete.

There is strong reason to suspect that a new planet, interior to *Mercury*, was on the 26th March, 1859, observed to transit over the sun's disk by M. Lescarbault at Orgères in the department Eure et Loire. The circumstances attending this remarkable observation are referred to, and the conclusions deduced from the observations, as to the elements of the orbit, are mentioned in the last number of the *Monthly Notices;* it only remains to add, that by the collection of prior cases of suspicion which this observation has led inquirers to make, there appear to be not less than four cases recorded in which dark bodies have been seen on the disk of the sun, possessing abnormal motion, on dates which fall within the limits of visibility of Lescarbault's planet during transit at its ascending node. In addition to these four cases, which may be taken as of some value in confirmation of a unique observation, it cannot be overlooked that other accounts exist of considerable circumstantiality which indicate the transit of planetary bodies the orbits of which cannot be identical with that of the planet of Lescarbault. In an imperfect list of such cases, printed in the last *Monthly Notice*, one case is quoted of an observation by Canon Stark of Augsburg on Feb. 12, 1820. To the surprise no doubt of many will be found in the *Olbers-Bessel Correspondence* the following passage (vol. ii. p. 162) in a letter from Olbers to Bessel, "What do you

say to *Steinhübel's* observation on the 12th of February this year (1820) of a dark round well-defined spot, which in five hours completed its transit across the sun's disk?" There is also the observation on Jan. 10, 1818, by Mr. Capel Lofft, the account of which has recently been republished by the Rev. T. W. Webb, in a little handbook, to which the Council refer with pleasure as useful and carefully prepared. Then there is the observation of Mr. Scott in the first fortnight of July 1847. Time will show, perhaps even the next two years, in spite of the known difficulty of proving a negative, whether any of the above are to be received as the first imperfect observations of actually existing interior planets, provided that disengaged observers will apply the telescope and acquaint themselves with ready means of recording the position of bodies they may see, so that such bodies may not again be confounded with ordinary solar phenomena. In this sense amateurs may be told that a new mine of observation has been opened.

On the 2d of April, 1859, a comet was discovered by M. Tempel at Venice. It continued visible till about the middle of May. The elements have been found to be sensibly parabolic.

On the 22d of September a new planet was discovered at the Observatory of Bilk by Dr. Luther. This forms the 57th of the group of minor planets revolving between *Mars* and *Jupiter*. The new planet has been called *Mnemosyne*.

Among the works which have appeared within the year the Council have particularly to notice the *Places of* 5345 *Stars*, observed at Armagh, by Dr. Robinson, a general account of which has already been given in the *Monthly Notices*. This work is a considerable contribution of materials of high class workmanship, available as data in the subject of proper motion in the solar system, and in stellar astronomy generally. The scheme followed has been the re-observation with modern precision of Bradley's Catalogue, and, as intervals became available by the exhaustion of the first working list, of Piazzi's Catalogue as a secondary one. Where exceptions occur to this plan they have been incidental. The arrangement of the published results is singularly lucid and economical of space, and on inspection will at once attract attention to a leading feature of the work, namely, that the instrument with which the results have been obtained has passed not only the maker's but the astronomer's hands. The division error applied is in the case of each star scrupulously stated, and the reports given in the Preface of the various precautions taken and considerations taken into account by the author, attest his high position as a practical astronomer.

A few weeks since, our indefatigable member of Council,

Admiral Smyth, published, in a splendid volume,* printed at the expense of Dr. Lee, a continuation of his observations on double stars, with other observations, and much historical detail on many subjects. This volume, an essential supplement of the *Cycle* by the same author, is a remarkable union of utility and beauty of execution. As the details, so far as they consist in new observations, are in continuation of a well-known series, no further description is here necessary.

The approaching total solar eclipse of July 18, of the present year, has received from English and Continental astronomers that attention which its importance deserves. With regard to the facilities of travelling in Spain and the choice of stations for observing, M. Lamont has afforded excellent information in a letter to the Astronomer Royal, which is printed in the last *Monthly Notice*. The Astronomer Royal at our evening meeting of December last entered very minutely into the subject, and detailed the various methods which had been proposed for observing the various physical phenomena which usually present themselves. Mr. Hind has published a circular with a map of the revised path of the shadow, drawn by the use of the best existing elements, Hansen's Lunar Tables and Le Verrier's Solar Tables being the chief authorities employed. In the Paris *Cosmos* of October 28, 1859, is a paper by M. Faye, on the methods of observing and the ends to be answered in the observations of the eclipse; and it has been proposed by M. Le Verrier at the Institute that he should have the direction of a commission to conduct the observations in Spain and Algeria.

At the suggestion of Mr. De La Rue the Royal Society has voted the sum of 150*l.* for the expenses of removing the Kew photoheliometer to Spain, for the purpose of photographing the eclipse, and Mr. De La Rue and Mr. Carrington intend to accompany the expedition, unless prevented by unforeseen circumstances.

Papers read before the Society from February 1859 *to February* 1860.

1859.
Mar. 11. On Determining the Latitude by Transits. Major Tennant.

R.A. and N.P.D. of Planets, from Observations at Madras. Capt. Jacob.

* *The Cycle of Celestial Objects continued at the Hartwell Observatory to* 1859, with a notice of recent discoveries, including details from the Ædes Hartwellianæ. By Vice-Admiral W. H. Smyth. London, 1860, 4to.

On Donati's Comet. Mr. Goddard.
On the Planet *Saturn*. Rev. W. R. Dawes.
On the Silvering of Glass Specula. Mr. De La Rue.
On the Proper Motion of the Solar System in Space. Mr. Airy.
On a new Process for Constructing large Reflecting Telescopes. M. Foucault.
Second Paper on same subject. The same.
On some Indications of Rotation in a Solar Spot. Mr. Birt.

April 8. On the Disturbing Function in the Lunar Theory. Sir J. Lubbock.
Extract of Letter on the Solar Spots. Dr. Peters.
Observations of Small Planets at Greenwich. Mr. Airy.
On a Cause of Irregularity in a Chronometer. Mr. Wackerbarth.
Elements of *Europa*. M. Lepissier.
On the Corrections of Transits on the Prime Vertical. Maj. Tennant.
On a Phenomenon seen during the Solar Eclipse, March 1858. Lieut. Hardy.
On a new Method of Clearing the Lunar Distance. Col. Shortrede.
On the Secular Variation of the Eccentricity and Inclination of the Moon's Orbit. Mr. Adams.
On the Apparent Projection of Stars on the Disk of the Moon. Mr. Airy.

May 13. Observations of Small Planets at Greenwich. Mr. Airy.
Comparison of Burckhardt's and Hansen's Lunar Tables with recent Greenwich Observations. Mr. Airy.
Ephemeris of Variable Stars for 1859. Mr. Pogson.
Letter to Admiral Smyth on Donati's Comet. Mr. Maclear.
Occultation of *Saturn*. May 8. Capt. Noble.
Ditto ditto Mr. Jeans.
Ditto ditto Rev. W. R. Dawes.
Ditto ditto Mr. W. Simms, Jun.
Ditto ditto Mr. Airy.
Ditto ditto Capt. Shea.
Ditto ditto Mr. Burr.
Ditto ditto Mr. Grove.
Ditto ditto Mr. Whitbread.
Ditto ditto Sir W. K. Murray.

June 10. On Traces of Eruptive Action in the Moon. Rev. T. W. Webb.
Suggestions as to the Structure of Tails of Comets. Rev. T. W. Webb.

Observations of Small Planets. Mr. Airy.
Occultation of *Saturn*. Mr. Lassell.
Sur la Variation du Moyen Mouvement de la Lune. M. de Pontécoulant.
Extract of a Letter to the Astronomer Royal. M. Hansen.
On the Polarisation of the Moon's Light. Prof. Secchi.
Occultation of *Saturn*. Mr. Pogson.
Observations of Transits of the Sun by means of his Projected Image. M. Ragonaatha Charey.
On the Successive Illumination of the Lunar Crater *Geminus*. Mr. Birt.
On the Star ν *Scorpii*. Capt. Noble.
Occultation of *Saturn*, May 8. Prof. Challis.

July 8. Elements of Comet I. 1859. Messrs. Safford and Hall.
On Abnormal Refractions during Eclipses of the Sun, and on the Determination of Longitude by Eclipses. M. E. Liais.
Note on the Occultation of *Saturn*. Mr. G. F. Pollock.
Corrections of the Elements of the Moon's Orbit deduced from the Lunar Observations made at the Royal Observatory, Greenwich, from 1750 to 1851. Mr. Airy.
On the Polar Distances of the Greenwich Transit Circle. Mr. Marth.

Nov. 11. Correction of an Expression occurring in a passage in a former Communication. Capt. Jacob.
List of Star-Magnitudes not included in Baily's Edition of Lalande's *Histoire Céleste*. M. Hencke.
Note on Mr. Pollock's Description of the Occultation of *Saturn* by the Moon. The Astronomer Royal.
On the Triple Star ν *Scorpii*. Rev. T. W. Webb.
On the Telescopic Appearances of the Lunar Craters *Geminus* and *Bernouilli*. Mr. Birt.
Description of an Equatoreal recently erected at Hopefield Observatory, Haddenham. Rev. W. R. Dawes.
Extract of Letter to Mr. Carrington. M. Schwabe.
On an Appearance seen in the Sun, Sept. 1, 1859. Mr. Carrington.
Observations of Comet VIII. 1858, and Small Planets. Mr. Pogson.
On Determining the Longitude by the Phase of the Moon. Mr. Brown.
On some lately discovered Double Stars. M. O. Von Struve.
On the Shape of the Moon. M. Gussew.
New Double Stars discovered by Mr. Alvan Clarke. Rev. W. R. Dawes.

On the Periodical Variations and Level and Azimuth of the Transit. Mr. Ellis.

On a curious Appearance seen in the Sun. Mr. Hodgson.

Dec. 9. Physical Observations of *Jupiter*. Sir W. K. Murray.

Observations of Small Planets and of *Jupiter's* Satellites. Mr. Airy.

On the Physical Constitution of Comets. Mr. O. G. Downes.

On a Remarkable Solar Spot. Rev. W. R. Dawes.

Observations of *Jupiter's* Satellites. Mr. Lassell.

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Royal Geographical Society.
Royal Institution.
Geological Society.
Linnean Society.
Zoological Society.
Photographic Society.
Society of Arts.
British Association.
Art-Union of London.
Institute of Actuaries.
British Horological Institute.
University College, London.
The Radcliffe Trustees.
The Royal Irish Academy.
Royal Dublin Society.
Royal Society of Tasmania.
L'Académie Impériale des Sciences de l'Institut de France.
Le Dépôt Général de la Marine.
Le Bureau des Longitudes.
The Society at Cherbourg.
Imperial Observatory, Paris.
Imperial Academy of Vienna.
Royal Academy at Berlin.
The Royal Academy of Sciences, Munich.
Royal Observatory at Munich.
Royal Academy of Sciences, Brussels.
Royal Observatory, Brussels.
Royal Observatory, Königsberg.
The University at Göttingen.
The Academy of Sciences, Stockholm.
Society of Sciences, Batavia.
Philosophical Institute of Victoria.
The Department of State, Washington.
American Philosophical Society.
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Address delivered by the President, the Rev. Robert Main, on presenting the Gold Medal of the Society to Professor Hansen.

Gentlemen,—I wish it had fallen to the lot of one of the great physical astronomers of whom this country can boast, and whom this Society is proud to enrol amongst its members,—I wish, I repeat, that it became the duty of an Airy or an Adams, a Herschel or a Lubbock, to detail to you the grounds on which your Council have decided to award the Gold Medal of the Society to Professor Hansen for his Lunar Tables. The subject which it is my duty to expound to you in connexion with this award is so vast and so difficult, the analysis which must be treated of is so refined and complicated, that were I to confine myself solely to the task of setting before you in a popular shape only such a sketch of the subject as is necessary to give you an adequate idea of the merits of the great work under consideration, without reference to the successive steps by which the lunar theory has been brought to its present perfection, the task would be sufficiently formidable to men much more intimately acquainted with the subject than I can pretend to be. But when you consider that, in justice both to your Council and to the illustrious astronomer whose work is crowned in this award, it is necessary to give you some idea of the ana-

lytical processes of preceding astronomers, and of the merits of the Tables which have embodied their results,—and that these, too, must be taken in connexion with the successive advances of practical astronomy, which have gone hand in hand with the theoretical developments in producing the work before us,—I may well ask for your indulgence both as regards the manner in which my duty is performed, and for the rather wearying details which will occasionally task your patience and attention. Yet, Gentlemen, permit me, before commencing the serious portion of my subject, to congratulate you as members of this Society on the pleasing duty which we are met together to perform,—a duty pleasing at all times, and under all circumstances, as exhibiting at stated periods that no improvement either in the theory or the practice of astronomy escapes our notice, and that we are eager to recognise it by the bestowal of the only distinction which it is in our power to award, but particularly pleasing on an occasion like the present, which marks the accomplishment of an astronomical era rather than the bestowal of a reward, and reminds us that one of the greatest of the tasks which were imposed on the astronomers of this age has been successfully achieved.

To justify these remarks, Gentlemen, it is necessary that I take you back for nearly two centuries, to the infancy, in fact, of modern astronomy, and remind you of the single and expressed object which the cultivators of the science in that age had in view. The growing greatness of this nation and the rapid development of its commercial relations made a study of astronomy absolutely necessary, not as a matter of pure science or of abstract speculation, but with a view to the observations of the fixed stars and planets for the purposes of navigation.

And, if I should be addressing any who have made the astronomical methods in use at that day a subject of study,—any, in fact, who are acquainted with them in Jonas Moore's Mathematics, and who understand the use of one of the principal nautical instruments at that epoch, namely, the nocturnal,—he will appreciate the difficulties which mariners had to encounter in a voyage to the East or West Indies, and the urgent necessity which existed for the introduction of better instruments and better methods. And it fortunately happened that this age was an age of great men, of giants in intellect, who could lead the advancing intelligence of the time, and could alternately urge and assist the Government of the country by advancing schemes suitable for the scientific and social as well as the commercial interests of this country. It was the age of Newton, Boyle, Hooke, and Wren, not to mention many others who were quite prepared to take every advantage of the Continental scientific discoveries bequeathed to them by that immediately preceding, and above all, and as a natural consequence, it was the age which gave birth to the Royal Society. Well, Gentlemen, one of the first-fruits of this intellectual activity was

the establishment of the Greenwich Observatory by the Government, with the expressed object of advancing the art of navigation, or of enabling the mariner to navigate the ocean more safely, and to find by observation and calculation his position upon it at any given instant.

It required no great sagacity to point out the objects to be aimed at in the prosecution of this idea, and, accordingly, in the warrant of the first Astronomer Royal, his duty is defined to be,—"to rectify the tables of the motions of the heavens and the places of the fixed stars, in order to find out the so-much-desired longitude at sea, for perfecting the art of navigation." You all know this, Gentlemen, but I call your attention to it now in a special manner, because it is the groundwork of our discussion on the subject of Lunar Tables, and because, if the Government and its advisers of that period had not been wise enough to lay down these stipulations, and, if the series of Astronomers Royal, from that time to the present, had not, under occasional misrepresentation and under evil report and good report, acted upon it with an honesty and a fidelity that have no parallel, the Tables now before you would in all probability never have existed.

Then, Gentlemen, look what was to be done to carry out these intentions, and be careful not to measure the amount of work, the necessary inventions, and the necessary discoveries by the notions derived from the present state of science; but go back in thought to that epoch absolutely destitute of everything but the genius and the determination of the men entrusted with this precious charge.

The notion of the grand method of discovering longitudes at sea by what are called "Lunar Distances" was not new to the seventeenth century. Apian* and Gemma Frisius, in the early part of the sixteenth century, and Kepler, in his Rudolphine Tables, had separately proposed the method, and a French Commission was appointed in 1634 to examine into the method as proposed by Morin.† But there was not one of the necessary requisites for carrying it out, and in particular the Lunar Tables of that epoch were quite inappropriate. Even Halley's Tables, constructed on the Newtonian theory of gravitation, had frequent errors amounting to 7′ or 8′, and this on the equator would produce at times an error of 180 nautical miles in longitude.

The art of chronometry was also, equally with pure science, in its infancy. It was not till the impulse was given in the early part of the eighteenth century, by the large reward offered by the British Government for the discovery of the longitude at sea, that the series of chronometrical improvements was begun, which, commencing with Harrison, was

* In his *Cosmographia*, published first in 1529.

† *Longitudinum Terrestrium necnon Cœlestium nova et hactenus optata Scientia.* Paris, 1634.

taken up successively by Arnold, Earnshaw, and a multitude of artists, extending to the present day, who have made the chronometer to become an instrument of wonderful precision, and a worthy attendant on the noble science of Nautical Astronomy, which has been by similar well-directed efforts brought to its present state of perfection. To make the method of Lunar Distances applicable, there are required, in the first place, instruments capable of observing the distance and the time with great precision; and, in the second, tabulated places of the moon and stars, which shall represent their true places with all attainable accuracy. With regard to the first requirement, the chronometer and the sextant, with their successive improvements, have done all that is needed; but to satisfy the second, there have been required all the resources of our own National Observatory, continued for above a century — not to speak of others — and the intellectual energies of all the great European mathematicians and astronomers continued for the same period. To avoid unnecessary prolixity, I shall assume that you are quite familiar with the successive steps by which the places of all the bright fixed stars have been determined with such accuracy, that doubt or error to the amount of a second of space in the position of any one of them now becomes a matter of serious discussion. I need only remind you that Maskelyne's thirty-six stars formed the basis of all following star catalogues, and that the Greenwich Mural Circle by Troughton was the origin of the precision of modern observations. But, with regard to the moon, it is necessary that I beg your attention again to the fact that, from the time of Bradley, observations of that luminary were made at Greenwich unremittingly with scrupulous care, and with all the accuracy attainable by the instruments and methods in use at each particular epoch. Thus, when mathematicians began in earnest to study the movements of the moon, and required observations for the more accurate determination of the elliptic elements on which their calculations were to be founded, it was to Greenwich that they looked, and not in vain, for what they needed. But to estimate the labour which was to be undergone by any one who at that period engaged in lunar researches, it is to be remembered that all these observations were left just as they were made, that is totally unreduced. There *was* then the raw material, the rich ore which would, with due labour, give forth the precious metal, but which was absolutely useless till much severe labour had been bestowed upon it. Still the observations existed. Greenwich was the treasure-house where the Astronomer, provided with funds and skilful in the processes of the practical department of his science, could find the materials which he needed. But here lay the difficulty: — first, astronomers had not themselves imbibed that idea which seems intuitive to us, of the comparative worthlessness of unreduced

observations, and funds were much less readily produced for such work than at the present time; and, secondly, it was extremely rare then, as it is at present, that the same individual should be a first-rate physical as well as a first-rate practical astronomer, and, till an individual possessing these rare qualities should be found, there was, in fact, no chance of obtaining Lunar Tables adequate for the finding of the longitude at sea.

With these preliminary remarks, I am now in a condition to point out to you the successive steps by which the theory of the moon has been brought to its present state of perfection. At the outset there is a curious fact to be noticed, namely, that half a century after the publication of Newton's *Principia*, there was no advance worth mentioning made in the analytical lunar theory. In England it is true the doctrine of universal gravitation was eagerly embraced; and the Cartesian philosophy was very soon expelled from Cambridge, and Newton's theory was expounded first by Dr. Samuel Clarke, and afterwards by Whiston, while at nearly the same epoch Keill taught it at Oxford. But, on the Continent, the hostility to it was for a considerable time universal. — Leibnitz and John Bernouilli, and, at first, Huygens, were unanimous in rejecting it, as well as the French astronomers Cassini, Maraldi, and Maupertuis; and it was not till the celebrated Voltaire gave to the world a popular exposition of it, that it began to gain ground and to conquer the delusions which the fascinations of the Cartesian philosophy had produced.

I need not dwell on the causes which led to this want of recognition of the truth; they are well known and obvious, and belong to human nature.

It is not impossible that there are men even now who would, through love of the study of Aristotle's ethical works, endeavour to maintain the exploded assumptions of his physical theories. The prejudices of education are with exceeding difficulty eradicated, and the Cartesian philosophy had been universally taught and universally admired from its apparent beauty and simplicity: the scientific men of the day gained their reputation by expounding its principles, and students flocked to every university to imbibe them. On the contrary, the Newtonian philosophy was severely mathematical beyond the acquirements of that age. Many of the analytical processes were invented by the illustrious discoverer, and even those processes were concealed by an after interpretation according to the old synthetic methods of geometry. Still this blindness could last only for a time, and the original tardiness of the Continental geometers was more than atoned for afterwards by a succession of brilliant analytical discoveries commencing with the middle of the eighteenth century, and continued without intermission to the present time; while, on the contrary, the English mathematicians, content with the fame

of Newton and with the methods which he had barely sketched for them, continued even within our own remembrance to expound his doctrines without improvement, and almost to ignore in our seats of learning the analytical developments which had become rudimentary science to the astronomers of the Continent.

It will now be necessary, for the sake of exhibiting to you more clearly the progress of the Lunar Theory, to divide the time from the middle of the last century, when the analytical methods were first promulgated, into epochs. These will be four; namely, first, that of Clairaut, Euler, and D'Alembert; secondly, that of Mayer and Mason; thirdly, that of Burg, Burckhardt, Damoiseau, and Plana; and fourthly, the present, which I will designate as that of Hansen.

It is remarkable that three of the greatest geometers of the Continent employed themselves almost simultaneously on the analytical theory of the movements of the moon, namely, Clairaut, Euler, and D'Alembert. Of these, the most remarkable was Clairaut. In 1747 he read before the French Academy a paper on the subject, in which he announced his celebrated difficulty about the motion of the apogee, deducing from the Newtonian theory, as Newton himself as well as D'Alembert and Euler had done, only half the amount. But, his process being one which proceeded by successive approximations, he soon found the cause of his error, namely, that the motion of the apse required a higher order of terms to be taken into account than was necessary for the other large inequalities; and, in 1749, he read another paper before the Academy with the important announcement that he had succeeded in reconciling theory with observation. About this time the Imperial Academy of St. Petersburg had offered a prize for the best Essay on the Lunar Theory, the subject being defined thus:—"To determine whether all the inequalities which are observed in the motions of the moon are in accordance with the Newtonian theory; and what is the true theory of all these inequalities, by means of which the place of the moon can be most exactly defined at any given time."

Clairaut, who sent in his MS. in 1750, was the successful competitor, and his memoir was printed in 1752, a most important epoch in the history of the Lunar Theory. His method is essentially similar to that afterwards adopted by La Place, that is, he obtains an expression for the mean longitude and radius vector in terms of the true anomalies of the sun and moon, and then, by reversion of series, he obtains the true in terms of mean quantities. He commences by resolving his forces in the direction of the radius vector and at right angles to it, his plane of reference being the ecliptic, and finds the differential polar equation of the orbit by the elimination of the time between them, according to methods which have since been familiar.

A second edition of his work* appeared in 1765, in which interval of time he had taken the opportunity to revise it, and to add Tables for the computation of the place of the moon, and to subjoin a comparison of two hundred places derived from French observations with the Tables. The maximum error of the tables is about $1\frac{1}{2}'$.

D'Alembert's "Théorie de la Lune", appeared in the first part of the *Recherches sur Différens Points Importans du Système du Monde*, published at Paris in 1754. It is accompanied by Tables, and offers little occasion for remark. Essentially the same method is followed as in the researches of Clairaut, including the reversion of series and the resolution of the forces in the direction of and at right angles to the radius vector. He obtains the ordinary differential equation for the orbit of the moon in polar co-ordinates; and it may be remarked that neither he nor Clairaut makes any use of rectangular co-ordinates. I may remark that the Preliminary Discourse of D'Alembert is well worthy of perusal even at the present time.

Euler's Memoir on the Lunar Motions, entitled "*Theoria Motus Lunæ exhibens omnes ejus Inequalitates*," was published by the Imperial Academy of St. Petersburg in 1753, but it is not accompanied by tables. The Memoir appears to have been sent in to the Academy as an accompaniment to the judgment which he thought it his duty to deliver on the merits of Clairaut's paper, and as giving his own views of the method by which the Lunar Theory might be advantageously treated. He refers the place of the moon to rectangular co-ordinates, the ecliptic being the principal plane of reference. His method which he had applied originally to the theory of *Jupiter* and *Saturn* is totally different from that of his rivals, and is remarkable as containing the germ of the method of deducing the true or disturbed place of the moon by the method of the variation of the elements of the orbit. He arrives, however, at expressions involving the true anomalies of the sun and moon, and leaves them in that state without reverting the series. In his second treatise, however, published in 1772, he recognises the inconvenience of this form, and by a process totally different arrives at expressions in which the mean longitudes and anomalies of the sun and moon are involved. The tables which accompanied this work were inferior in accuracy to those of Mayer. Important as these papers were, we need not devote any more space to them, because the tables resulting from their theories were not of sufficient accuracy for any of the purposes of navigation. Yet, even at the present time, they would amply repay the perusal of any one who could afford time to do so, and are, considering the state of mathematical as well as physical science of that period, admirable productions.

* *Théorie de la Lune, deduite du Principe de l'Attraction*, par M. Clairaut. Second Edition, à laquelle on a joint des Tables de la Lune: à Paris, 1765.

We now come to the second epoch of the history of Lunar Tables, namely, that of Mayer, an epoch of extreme importance, because his tables were the first that could be applied effectively to the solution of the great nautical problem of finding the longitude at sea. Mayer was an excellent practical astronomer as well as mathematician, and, as such, was not only capable of taking up the analytical theory in the state to which Euler (whom he took for his model) had reduced it, but of correcting by means of observations the coefficients of the inequalities which were exhibited by theory. In 1751 he was appointed to the superintendence of the Observatory of Gottingen, and exhibited remarkable skill in the examination and mode of use of the six-foot quadrant which was committed to him, and in the treatment of the observations made with it. The observations, however, which he used for the correction of the lunar elements and inequalities, consisted chiefly of about two hundred eclipses of the sun and occultations of the fixed stars. His analytical method, though setting out with Euler's equations, as given in his first treatise, is sufficiently original to show his perfect mastery of the subject, and is, on the whole, very simple, so that it can be understood with considerable facility. After giving the general expression of his forces, and the formation of his differential equations, he proceeds to change the independent variable, using first the mean motion of the moon, and then another quantity depending upon it by a very simple equation, so that it can easily be reinstated at the end of the work; and, for the true longitude, &c. of the sun, he proceeds at once to substitute the mean values. He then proceeds to determine the values of the unknown quantities in his expansions by the method of indeterminate coefficients. The resulting true longitude of the moon contains fifteen inequalities, and of these he compares the values of the coefficients derived from theory with those deduced from observation and used in his Tables. Mayer died in the year 1762, being then only 39 years of age, and his widow transmitted to the Board of Longitude a revised copy of his Tables. By the recommendation of the Board the sum of 3000*l.* was paid to her by the British Government, while, at the same time, Euler was rewarded by a grant of 300*l.* Mayer's Theory of the Moon was published in 1767 by order of the Board of Longitude; and in 1770 his Solar and Lunar Tables were published, under the auspices of the Board. Mason's Tables, published in 1782, may be considered as a new and improved edition of Mayer's. A few new equations are added, and the coefficients of some of them are slightly altered from the results of comparisons with 1200 Greenwich observations. By these means the tabular errors of longitude were on the average diminished in the ratio of 25 : 17, as is shown in the comparison of both sets with Greenwich observations, exhibited at the end of the Tables.

We now come to the third epoch, namely, that of Burg and Burckhardt. It must be remembered that I am not giving a history of Physical Lunar Astronomy, but only indicating a few of the principal features of the successive discoveries from the time of Clairaut to the present, in connexion with the Tables based on the theoretical developments. Time forbids me to do more than this. It must, however, be borne in mind that between the epochs of Mayer and Burg, La Place and Lagrange, not to mention Poisson and other mathematicians, had in generous rivalry excited the admiration of Europe by the continuity and profusion of their brilliant discoveries in physical astronomy, and that the commencement of the present century was characterised by the publication of the greater portion of La Place's immortal work, the *Mécanique Céleste*, in which all the researches of himself and his illustrious rivals were embodied. In this interval the analytical theory of the moon had been, by the labours of these illustrious men, rendered almost perfect. The cause of the secular acceleration of the mean motion had been discovered by La Place, and he had shown that the node and the apogee are subject to secular inequalities from the same cause, namely, the secular diminution of the excentricity of the earth's orbit. He had also discovered an inequality in longitude and latitude, arising from the spheroidal form of the earth, and had by his analysis found the value of that due to the solar parallax, or the parallactic equation. It became now necessary to make a practical use of all these brilliant discoveries, by the construction of tables based on theory, in which the coefficients of all the inequalities should be obtained without the aid of observation, by carrying the successive approximations so far that no sensible numerical error should remain in any of them.

An inducement to undertake the enormous labour requisite for carrying out these researches was offered by a series of prizes proposed by the French Academy, the funds for which were furnished through the bequest of a large sum of money by a French citizen, M. Rouillé de Meslay, in 1714.

The first prize, offered by the Academy in 1798, had a more limited object in view, and did not discard the employment of observations. In fact, the subject proposed was, "to determine, by at least 500 observations, the epochs of mean longitude of the moon, and of the apogee and nodes of her orbit." This prize was divided between Bouvard, since so well known for his Tables of *Jupiter* and *Saturn*, and Tobias Burg, an Assistant Astronomer at the Observatory of Vienna. Burg did far more than was required of him. He employed more than 3000 Greenwich observations, and obtained the required quantities for the beginning of the eighteenth as well as for the beginning of the nineteenth century, and thus gave occasion for the discovery of some inequalities in these epochs. A new prize of 150*l.* was now offered by the Consular Government at

the instance of the Academy for the construction of Lunar Tables, based on these materials, and this, as might be expected, was won by Burg.

His Tables were sent in in 1802, and, on a favourable report being given of them by a commission appointed to examine them, of which La Place and Lagrange were members, the First Consul doubled the prize.

These Tables by Burg form an important epoch in the subject before us. They were far more elaborate and correct than any which had preceded them, and included in their construction all the physical discoveries which had been made up to that time. We may observe that, in the course of his calculations, Burg discovered a varying error of the epoch of mean longitude which La Place failed to account for, and which both he and his follower, Burckhardt, supplied empirically.

In December 1811, Burckhardt presented to the Institute his own Lunar Tables, in which he had employed more than 4000 observations to correct the elements and the coefficients of the inequalities employed by Burg. A commission was appointed to examine them, and, in an evil hour, they reported so favourably of their accuracy, as compared with those of Burg, as to bring them into general use, and in particular to cause their employment in the English *Nautical Almanac*. The commission executed their task with fidelity and care, and were certainly justified in their decision by the results of the comparison of 167 Greenwich observations, by which it appeared that the sum of the squares of the errors of longitude were less than those of Burg, in the proportion of two to three. Nevertheless, nothing could have proved more unfortunate to the lunar theory than their adoption. This has only become notorious within the last few years, but was well known to some great physical astronomers many years ago. Any one who will take the trouble of looking at the Greenwich observations since 1836, will easily find errors which in the present state of the lunar theory are intolerable, amounting, in fact, frequently to half a minute in longitude. It is well known that Mr. Adams,* in his lunar researches, a few years since, detected a very serious error in the parallax, which Burckhardt had calculated most elaborately from La Place's theory; but no one but himself can tell the labour which the tracing of this error and its correction must have cost him, on account of the artificial form of the arguments used in these tables. Burckhardt adopted this form after experimenting a great deal on the subject; but the result has been that it is very difficult to compare his tables with theory for the purpose of correcting the errors of the elements and coefficients of inequalities indicated by the observations, and that, in consequence, a vast amount of labour, which would have been other-

* See "Appendix to Nautical Almanac" for 1856.

wise unnecessary, has been thrown on the Astronomer Royal and the other astronomers who have contributed to bring the theory to that perfection, of which the result has been the grand work of Hansen, which we are now discussing.

Thus far observation had lent its aid, both for the correction of the elliptic elements of the orbit and for that of the coefficients of the inequalities. But the lunar theory had now been brought so near to perfection by the labours of La Place, Lagrange, and Poisson, that the French Academy in 1820 gave as the subject for the great mathematical prize the construction of Lunar Tables as exact as those hitherto furnished, but in which the coefficients, as well as the arguments of all the inequalities, should be determined solely by the theory of gravitation.

The successful candidates for the prize were, first, Damoiseau, a retired French artillery officer, and, secondly, the Italian astronomers Plana and Carlini, who shared between them the labour as well as the honour of the work which they sent in. Damoiseau's method was, without any variation, that of La Place, which we have had occasion to mention before as consisting in the expression of the mean in terms of the true longitude, and in proceeding by reversion of series to obtain the true longitude in terms of the mean, increased by a series of inequalities depending only on the time. La Place was not blind to the advantage which would be gained by proceeding directly to form the equations of motion so as to have the time or the mean longitude as the independent variable; but he despaired of its success, on account of the comparatively slow convergence of the resulting series.*

There is no doubt that the method of La Place has the advantage of simplicity, and the differential equations of motion naturally present themselves under the form in which Clairaut, D'Alembert, and almost all geometers, had put them, that is, with the true longitude of the moon as the independent variable. But on account of the enormous labour, when the approximations are pushed to a high order, of the necessary operations which must be performed before the results are put into a shape fit for tabulation—part of which consists in expressing the true motion of the sun as function of the true motion of the moon, and part in afterwards expressing true longitude, true latitude, parallax, and all other required functions in terms of mean quantities—some geometers (including M. Pontécoulant and Sir John Lubbock) have tried to solve the problem by using either the time or the mean longitude as the independent variable. The former of these astronomers has been perfectly successful, and he has carried his approximations as far as Plana in his great work on the lunar theory, so as to afford a most valuable

* See *Méc. Céleste*, lib. 7, Introduction.

test of the accuracy of his coefficients.* Sir John Lubbock appeared to be equally successful up to the point to which he carried his researches; but, unfortunately, want of leisure prevented him from carrying his approximations far enough to afford an ultimate test of his theory. If time did not fail us, it would be necessary to speak with some detail of the great work of Plana, afterwards published in three large quarto volumes, which is an imperishable monument of his genius, as well as of his industry; but we must bear in mind that our immediate subject is the history and consideration of lunar tables, and not of the lunar theory generally, and this passing mention must suffice. It is, however, necessary to state that, independently of the extent to which the approximations are carried, Plana's investigations have tended in a remarkable degree to the improvement of the Lunar Theory by his method of expansion of the coefficients of the inequalities in converging series, as, by this means, security is given for the accuracy of successive approximations, and the operations are performed with much greater facility. We may also be allowed to express our regret and surprise that the work sent in to the French Academy by him and Carlini in competition for the prize, and which must have been accompanied by tables, was not printed; and we could scarcely believe the fact without eminent French authority for it that the memoir has not even been preserved.†

The Tables of Damoiseau, you are well aware, Gentlemen, were printed by the Academy of Sciences, as well as the theory on which it was based — the latter in vol. i. of the *Mémoires des Savans Etrangers* (3d series). In the theory there is nothing to show but the enormous labour in pushing the approximations to a higher order than La Place, all the terms of the fourth order being taken in, and such of the fifth as are necessary for obtaining those of the fourth correctly, and the consequent value of his results, which, however, must be taken, when they are not verified by comparison with others, on his own authority, since there is scarcely any man living who would undertake to follow him through the complicated mazes of his analysis till the numerical results are arrived at. Still the importance of this work of Damoiseau was exceedingly great; and it is only to be lamented that, in the *Nautical Almanac* and other similar works, Damoiseau's Tables had not been chosen instead of those of Burckhardt.

To make this portion of our subject complete, so as not to omit the mention of any great name that tended to throw light by original research on the doctrines of the lunar theory, I will barely mention a remarkable paper by Poisson on the "Motion of the Moon round the Earth," printed in vol. xiii. of the

* Pontécoulant, *Système du Monde*, vol. iv.

† See *Journal des Savans* for October and December 1857, and January 1858, containing "Review of Hansen's Tables," by M. Biot.

Mémoires de l'Institut; in which the author deduces the lunar inequalities by the method of the variation of the elements, though the expansions are not completely effected. The memoir, therefore, becomes only an interesting subject for study, but does not present a complete solution of the problem, and thus only remotely concerns us at present.

There is still one more work of considerable importance to be mentioned previously to that of Hansen, namely, the American *Tables of the Moon*, published in 1853, under the superintendence of Lieutenant Davis, U.S.N., and by the authority of the Secretary of the Navy, according to the arrangement of the distinguished mathematician Professor Peirce. These Tables are based on Plana's theory, but the coefficients of the inequalities are corrected by the results given by the Astronomer Royal as arising from the discussion which closed his great work of the Reduction of the ancient *Greenwich Observations*, as well as by some corrections due to investigations of Professor Longstreeth. There are also included: Hansen's two long inequalities due to the action of *Venus*, as well as his value of the secular equation of the moon's mean motion, and of the motion of the perigee. If these Tables be considered merely with reference to their current value—that is, as giving tabular places of the moon worthy of considerable confidence—there can be no dispute with regard to the high estimation in which they should be held; but the want of uniformity in the chosen values of the coefficients of the inequalities must detract in some measure from their value, when considered in relation only to the advancement of the abstract theory of the lunar motions.

Having thus, Gentlemen, cleared the way for a consideration of the labours of Professor Hansen, by giving you an idea, however imperfect, of those of his predecessors, I am enabled to devote the remainder of this Address exclusively to the subject which more immediately concerns us; and it will perhaps induce to clearness if I first give you a sketch of what may be called the external history of the work, confining myself chiefly to those features which make it peculiarly interesting to us as Englishmen and English astronomers. I was not aware myself of the amount of anxiety which the bad representation of the lunar motions by the Tables of Burckhardt had caused, more than twenty years ago, in the minds of some of the greatest of our astronomers, till, when I was preparing for my present task, the Astronomer Royal, with his usual kindness, put into my hands his correspondence on the lunar theory, continued from the time when he was first called upon to occupy his present position, namely, the year 1835, to the present time. Even if I had not known how many great works he has brought to completion in the same period—how he has revolutionised the theory and the practice of the construction of astronomical instruments, as well as of

the making and reducing of observations—how he has borne the chief labour in almost every Government commission for scientific purposes, of which we need only mention the Standard Commission—how he has been occasionally engaged in optical researches, or in the writing of profound memoirs in some branch of abstract or mixed science, some of which adorn our own *Transactions*—and all apart from and in addition to the direct duties of his office—I should have thought the amount of care and labour which this subject of the lunar theory brought upon him, even before he took upon himself the responsibility of the reduction of the *Ancient Greenwich Observations*, a very serious addition to the heavy duties which are necessarily imposed upon him by the ordinary administration of the observatory. And I assure you, Gentlemen, that till now I have been myself quite ignorant of the amount of his indirect labours in this department, though I have been for nearly five-and-twenty years so near his person, and generally cognizant of all his efforts for the advancement of lunar astronomy. I am not writing an *éloge* on the Astronomer Royal, which would ill become the place or the occasion on which we are met together, but simply stating a fact which will make what follows more intelligible. I wish, in particular, to point out to you that the imperfect state of the lunar theory (or, rather, of its embodiment in tables, for Plana's great work even then existed) had attracted attention. Sir John Lubbock was then engaged in the attempt, to which I have before alluded, of applying a totally new method to the lunar theory, by making the mean longitude the independent variable in the equations of motion; and in the course of his researches had frequent correspondence, involving many deep questions, with Mr. Airy; and the chief difficulty which they had to encounter, as I before hinted, was the disentangling of the artificial arguments devised by Burckhardt, and the finding out what values of the coefficients and what arguments as deduced from theory he really had used, so as to enable Sir John to compare his own results with them. A great deal of labour was fruitlessly bestowed on this part of the subject. Then, again, there was a dearth of reduced observations for comparison with theory: a few existed, but even of these the elements of reduction were questionable, and they were generally unfit for the purpose of putting the lunar theory upon a better basis; and it was interesting to me to observe how, after various expedients were devised and rejected, the idea gradually became clear that the *Greenwich Observations*, which were confessedly the only ones continuous and accurate enough for the purpose, must, some time or other, be reduced on a uniform system. The project of this great work, when viewed prospectively and barely hinted at by Mr. Airy in one of his letters, he calls, and justly so, "stupendous;" and indeed any one who should be called upon even now to reduce nearly 9000

observations of the moon, made with old instruments of which the theory had to be studied and by men who have passed away and of whom no question can be asked, would, I presume, think it a very formidable task, even without the amplification and partial reconstruction of tables as the first and indispensable step towards the laborious tabular computations necessary for the comparison with observations. But, Gentlemen, it was in this gradual way that the great work of the *Greenwich Lunar Reductions* became an accomplished fact; and in the history of science there are few, if any, that can rank as greater or more important. Yet, even while this great work was going on, the *Nautical Almanac* kept on the even tenor of its way, giving places of the moon and lunar distances so far at variance with the truth as to be an opprobrium to the astronomical science of the nineteenth century. This, even, was not without remonstrance; and it was urged to the late superintendent of that work that it would be comparatively easy to substitute better tables; but timid counsels prevailed for a time, and then the hope of the tables promised by Hansen made it prudent to defer any radical change till they had appeared. It was understood, for many years previously to the interference of the British Government with offers of help to complete Professor Hansen's Lunar Tables, that he was engaged on them and on researches connected with them. At intervals he gave some of the results of his researches to the world in the *Astronomische Nachrichten*, commencing with the year 1829; and it was generally understood that the Danish Government had undertaken to supply the funds necessary for the completion of the work. Unfortunately that Government became involved in the Schleswig-Holstein wars, and, in 1848, was obliged to withdraw the very moderate assistance which it had hitherto furnished.

A year previously M. Hansen had shown his complete command of the analytical theory which he had employed for the calculation of the lunar inequalities by the discovery of two curious inequalities of long period in the epoch of mean longitude arising from the action of *Venus*. The *Ancient Greenwich Observations*, extending over eighty years, had brought this inequality to light in a very conspicuous way, and no existing theory gave any probable solution of it. The investigations of La Place and Damoiseau had tended to show that the action of the planets produced scarcely any appreciable effect on the lunar orbit, at least none which could by possibility prove sensible without taking into account higher powers of small quantities than had been till that time employed. This, however, Hansen proceeded to do, suspecting rightly that as the almost accurate commensurability of the mean motions of *Venus* and the Earth, produced a sensible perturbation in the motion of the latter dependent on terms of a high order, the same might take place with respect to the lunar orbit, even

though it should be necessary to carry the approximation still farther. The attempt was quite successful, and the result was, that two large inequalities were brought to light, one depending on the direct and the other on the indirect action of *Venus* on the moon, having for their periods respectively 273 and 239 years. The coefficients assigned to these inequalities in the tables are 15″·34 and 21″·47. The determination of these coefficients, it ought to be explained, has been accomplished with very great difficulty, and M. Hansen, in attempting by a different method to verify the results obtained in his first calculations did not perfectly succeed, so that probably the complete development of these two inequalities by theory is still one of the desiderata of the lunar theory. This digression is necessary, and historically valuable, because it was this brilliant discovery that increased the eagerness of the English astronomers, and especially of the Astronomer Royal, for the completion of Hansen's great work, and induced the latter gentleman to inquire of him directly, and also of Professor Schumacher, in the year 1850, concerning his progress. The answer was a melancholy one—as has been stated, the funds furnished by the Danish Government were suspended without much prospect of speedy renewal; the computers were discharged, and the work was comparatively at a stand. It was well, Gentlemen, that at this crisis the correspondence was in the hands of one who deservedly had the ear of the Government of this country—who could apply with confidence for funds for the completion or the performance of any great scientific work, without chance of refusal, because he had been never known to fail either in his estimation of the national importance, or in the successful prosecution of other great works which had been previously proposed by him. On learning the true state of affairs he lost no time in learning from M. Hansen that assistance from the British Government would be acceptable to him, and in making a direct application to the First Lord of the Admiralty for the estimated sum which was modestly demanded. And what was the sum, think you, Gentlemen, which was wanted and which stopped the progress of this great work? The whole cost of the remainder of the work, up to the time when the MS. of the Tables was fairly written out and ready for the printer, was 300*l.* The want of a sum which a private gentleman devotes to a yachting or a travelling expedition was sufficient to stop for a time the progress of perhaps the greatest astronomical work which has been produced in the present century—greatest, if not absolutely or intellectually, yet at least in its effects as giving the finishing stroke to that great problem of finding the longitude at sea in which the whole civilised world is vitally interested. Yet, Gentlemen, though the sum be small that was demanded of the Government, this does not detract in the slightest degree from their merit in readily granting it, and still more may we

admire the spirit with which the Board of Admiralty took upon themselves the expense and responsibility of *printing* the Tables and providing for their distribution so liberally that there is scarcely an institution in the world where a copy could be advantageously lodged, which has not had a copy presented and forwarded to it. Time will not allow me to dwell longer on the history of the production of these Tables, because it still remains that I say a few words on their construction and on the theory on which they are based. It may be sufficient to state, that in November 1854, M. Hansen announced that the greater portion of his work was completed, and at the same time sent some comparisons with observations extending from 1824 to 1850, to test the accuracy of his results;* that in the year 1856 he came to Greenwich to superintend the printing of the Tables; and that the copies were distributed in the summer of 1857.

I might perhaps be excused from entering on the subject of the theory on which these Tables are based, because the Council, being well assured of the high character of the analytical investigations of M. Hansen, as exhibited in his elaborate work on the Lunar Theory, published in 1838;† in his work on *Jupiter* and *Saturn*; and in that entitled "*Determination of Absolute Perturbations in Ellipses of any Excentricity and any Inclinations whatever*," and not having before them the full development of the theory (which however the scientific world is anxiously expecting), have been guided in their award of the Medal by, for the most part, practical considerations.

On the publication of the Tables, Mr. Hind, with that ready zeal which characterises all his efforts for the advancement of science, undertook, at the instance of the Astronomer Royal, the comparison with them of all the observations of the moon which have been made at Greenwich since the institution of the altazimuth in 1847. For this work of love, involving as it did so considerable an amount of personal anxiety and labour, Mr. Hind deserves the warm thanks of this Society in particular, and of the scientific world in general, because, in the absence of the complete theoretical investigation, neither would your Council have been able otherwise to make this award, nor would the exceeding great value of the Tables have become so readily apparent. In the *Monthly Notice* for April 1859, is given the result of the comparison for the year 1852, and you may see for yourselves that for this year the sum of the squares of the errors in comparison with Burckhardt are greater than those in the comparison with Hansen in the proportion of 8 : 1. The comparisons have been completed for almost all the remaining years including 1858; and, as they are not published, I beg you to accept my assurance that the errors are equally small during

* See *Monthly Notice* for November 1854.

† *Fundamenta Nova Investigationis Orbitæ Veræ quam Luna perlustrat.* Gotha, 1838.

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the whole space of time from 1847 to 1858, and that M. Hansen's own estimate of the accuracy of his results, as given in the letter before referred to, is not at all exaggerated. He estimates that the mean error in R.A. is rather more than $2\frac{1}{2}''$, and in declination rather more than $2''$, while that of Burckhardt is according to his own (Burckhardt's) estimation $5\frac{1}{2}''$. Thus, Gentlemen, without going into details, you may rest assured that the residual errors of the lunar theory are reduced now to most insignificant limits; and certainly within the errors incidental to meridional observation with first-rate instruments, and very far within the limits of accuracy of observations with the sextant;—that Hansen's Tables are practically perfect for all the purposes of navigation, and that the great nautical problem of finding the longitude at sea is now completely solved. That rebellious satellite, which has hitherto successfully resisted all the most complicated and refined methods of analysis, is now completely reduced to obedience to the laws of gravitation, and her predicted place corresponds to her true place in the heavens as nearly as in the cases of the other planets and the fixed stars; there remains indeed enough undetermined in the theory of her motions to excite the emulation and the genius of the most profound of our physical astronomers (for none but the *most* profound can meddle with the remaining questions), but there is nothing left unsettled which vitiates in the slightest degree the absolute accuracy of the tabulated places.

Still, though your Council have judged that they could not do wrong in resting with perfect confidence on these practical considerations, and that further delay in their award of the Medal would be as injurious to the reputation of this Society as it would be unjust to M. Hansen, yet there is sufficient knowledge of the theory to prevent any sensible doubt of the correctness of the principles applied to the formation of the Tables. You will have observed, in my historical sketch of the analytical processes employed by the predecessors of M. Hansen since the time of Clairaut, that though the developments of the disturbing function are prodigiously increased in the later researches, yet there is essentially the same method employed in all, and that the theory of the moon has always been treated as an exceptional case, and as requiring different treatment from that of any other case occurring in the general problem of the motions of three bodies mutually attracting; that, taking advantage of the fact of the ratio of the mean motions of the sun and moon being small, as well as the excentricity of the orbit and the inclination to the ecliptic, all the methods have been devised with the view of expanding the perturbation-function and all resulting functions for the deduction of the true longitude and latitude and parallax of the moon, in terms of powers of these quantities. Professor Hansen's object has been to do away with this exceptional character in the methods

of dealing with the problem of the three bodies, and to arrange his calculations in such a shape that they shall be equally applicable to orbits of all degrees of excentricity and inclination, and with any ratio of the mean motions whatever; a method which should be equally applicable to the theory of *Jupiter* and *Saturn*, to that of the small planets between *Mars* and *Jupiter*, and to the moon.

I will devote a few words to the explanation of the most remarkable points in his method, which, as you may imagine by what has already been said, is totally different from that of any of his predecessors. The ordinary method, or that employed by La Place, and by almost all other geometers, consisted in obtaining the differential equation of the disturbed orbit, and solving this equation by successive approximations. The first part of the process for longitude consists in expressing the terms of the disturbing forces, or the disturbing function, which must be manifestly functions of the elements of the solar and lunar orbits, and of the position at any given instant of the sun, moon, and earth, in terms of the true longitudes of the sun and moon, and ultimately of the true longitude of the moon; and of correcting by successive approximations the solution of the equation which expresses the relation between the reciprocal of the radius vector and the true longitude of the moon; while at the same time the differential equation expressing the angular velocity of the moon gives means for expressing the time or the mean longitude in terms of the true longitude, and by reversion of series of expressing the true longitude in terms of the mean; and thus of ultimately expressing the parallax in terms of mean quantities. Professor Hansen's method is totally different from this from the very commencement. He does not attempt *immediately* to obtain the actual expressions for the perturbations of the true co-ordinates of the moon's place, but he does this ultimately by deducing these quantities from the disturbed mean anomaly, the greatest portion of the labour being devoted to the expansion of this function in terms of mean quantities. This disturbed mean anomaly he afterwards substitutes in the equation of the centre of the mean lunar orbit according to the ordinary elliptic theory, and thus obtains the true anomaly in the disturbed orbit, which can be reduced in the usual way to true longitude by the application of the tropical longitude of the perigee, and of the reduction to the ecliptic together with its inequalities. He has in fact convinced himself by his investigations that the perturbations of the mean anomaly when converted into series converge much more rapidly and more surely than those of any other element, and that none of those doubts and difficulties about the degree of convergency of the series occur which meet the computer at every step of the approximations by the ordinary theory.

To effect his development M. Hansen of course proceeds as in other methods by successive approximations. Setting out

from the mean or undisturbed elliptic elements, he calculates successively the terms of the development which are due to the first, second, and higher powers of small quantities, using, according to well-known methods, the expressions derived from each approximation as the basis of the succeeding. But there is an important peculiarity in his method which cannot be passed over, because it is one on which he himself very much insists, and because it has been the subject of some discussion between himself and M. Delaunay. Since the whole problem of the lunar theory consists in finding for any given instant the three true co-ordinates of the moon, namely, the longitude, the latitude, and the radius vector or the parallax, there are various ways in which this can be done; and it is perfectly legitimate to assume that three out of the six elliptic elements are absolutely constant (that is, to attribute to them throughout their mean values derived from observations), and to throw all the variation on the other three elements. The three which M. Hansen has chosen for his constants are the excentricity, the inclination, and the mean motion; and he has been able so to arrange his calculations, that these three primitive elements shall enter into the coefficients of all the inequalities which it is necessary to calculate; and he is thus enabled at each step of his process to replace the analytical values of his coefficients by numerical values, and to avoid that intolerable complication of terms which in the other method increases so rapidly, as higher degrees of approximation are aimed at, as in the end to conquer the most industrious and the most skilful of analysts. M. Delaunay, whose theory is not yet published, states that he has discovered a method by which he can retain the advantages (for some such must be allowed to be attached to it) of the preservation of the literal values of the coefficients throughout the whole of the investigation, together with the absence of this extreme complexity of operations, by dividing his whole process into several subordinate ones. As neither his complete theory nor that of M. Hansen is yet published, it is useless to speculate on the preference due to their respective methods, nor to attempt to judge them. We have hope that both will be speedily published, and the appearance of them will be hailed by the whole scientific world as the most precious additions to physical astronomy that have been made in the present century.

I have not time to enter more into detail concerning the theory of the construction of the Tables, and it only remains that I say a few words about their form. In the year 1854, when M. Hansen had made a provisional construction sufficient for his own purposes, so as to enable him to calculate a sufficient number of places, that astronomers might judge of their accuracy by their comparison with Greenwich observations of the moon, he used as the unit of his arguments the mean lunar day, that is, the mean of the intervals of time taken by the

moon in departing from a given meridian and returning to it again; and he seemed inclined to use this unit in the final composition of his tables. Though this unit has some advantages, particularly for the construction of ephemerides, yet there are so many more cases in which it is absolutely necessary to compute for equal intervals of mean solar time, that he was induced to change his mind, and the unit of his argument in the tables is a mean solar day. It is known to many amongst you, Gentlemen, that this unit of argument in the construction of planetary tables was proposed first by Carlini, and employed in his Solar Tables; and the advantages are so obvious — especially for solar tables, from the facility with which the arguments are formed when their periods are known — that I need not dwell upon them. The chief part of the Tables is occupied with the formation of what M. Hansen calls the *Fundamental Argument* — that quantity, in fact, which, when substituted in the equation of the centre, produces the true anomaly. The tables necessary to form this *Fundamental Argument* are fifty-three in number, and this after the utmost possible skill has been shown in combining the terms expressing the perturbations of the mean anomaly, so as to produce as much compression as possible. Many of these are tables of double-entry, that is, having a vertical and a horizontal argument; and every one knows how troublesome and wearying these are in practice, even when they are expanded so as to reduce the intervals of the successive values of the arguments as much as possible. But M. Hansen has found an ingenious device by which one of the arguments depending on the mean anomaly of the moon may be considered absolutely constant for a whole period of the mean anomaly by adding to each of the various values of the other argument the movement of the first from its initial value to that which corresponds to the actual value of the other. These are the two chief peculiarities of the actual construction of the Tables which I find it necessary to point out to you, and the only ones which time will allow me to dwell upon. It may, however, be mentioned, in addition, that the Tables contain the means not only for computing the co-ordinates of the moon referred to the ecliptic — that is, the longitude and latitude — but also for deducing from them the right ascensions and declinations referred to the moveable equinox, which, of course, is the point to be arrived at in every computation of a place of the moon; and this appears to be effected with the greatest possible simplicity. The only remaining question which we have to ask ourselves is, whether these Tables, so faultless in theory and so skilfully elaborated, are, after all, convenient in practice, and, above all, whether ordinary computers would be able to deduce from them places of the moon with the requisite facility. The book is undoubtedly a large one, and, at first sight, the task of computing a place from it looks formidable; but this very size of the book

is for the advantage of the computer, the tables being so expanded as to enable him to take out all his numbers at sight. This is not so with other Lunar Tables; and especially, I remember, that, in the use of Damoiseau's Tables, the first necessary step was to expand a great many of them very considerably to avoid second differences. But without putting undue confidence in these reasonings, I was not content till I had satisfied myself of their convenience and facility of use by going through a computation of longitude, and had assured myself that for a great book it is a very easy book, and that ordinary computers might easily be trained to compute readily places of the moon from it. But I have a still better guide for my judgment in this matter in the expressed opinion of Mr. Hind, who, at the present time, has had perhaps more experience of the use of the Tables than any other astronomer living, having, as I before stated, undertaken and very nearly completed the comparison of all the Greenwich observations from 1847 to 1858. At my request he, with his usual ready compliance, gave me the following opinion of them:—

"Hansen's Tables considered simply as machinery for the practical determination of the moon's places may, although the process is longer than that by Burckhardt's Tables, be considered a production of the first order, and their excellence is particularly evident in the construction of an ephemeris.

"The operation at first sight appears formidable even to the initiated in such matters; this arises principally from the rather serious array of double-entry tables; the use of these tables does not however involve the irksome process of cross proportion, as every thing required from them is adapted to tabular increments of the horizontal argument.

"It is no bad test of their excellence to say that they are popular in this office; and further, that the parties employed in the twelve-years calculations by them for the Astronomer Royal, although at all times eager to devise artifices for shortening labour, failed to discover much scope for the exercise of their ingenuity."

Thus, Gentlemen, have I completed, however imperfectly, the somewhat formidable task which I proposed to myself at the commencement of this address, by giving you as far as my limited time and my facilities for collecting information would allow me, an exposition of the various theories which have been used in the construction of tables of the moon since the time of Newton, as introductory to such an account of the Tables of M. Hansen, which are this day crowned by the award of your Council, as would enable you to understand the *grounds* of the award. I felt indeed, that if I performed my duty properly, I must take a somewhat different course from that usually pursued on similar occasions, because it appeared absolutely certain to me that the Council would require no justification whatever, either in your eyes or in those of any other scientific

community. The general expression of eagerness and satisfaction with which the appearance of the Tables was greeted, on their publication,—the unanimous favourable verdicts of all competent authorities,—proved the universal conviction that a new epoch in physical astronomy had arrived, and that the difficulties in representing the moon's true place accurately by theory, which have, ever since the time of Newton, so occupied the minds of astronomers, and been such a hindrance to the perfection of nautical astronomy and navigation, belong solely to the history of the past. And your Council might have accepted this favourable judgment at the very first; and there is no doubt that the *immediate* award of the Medal to M. Hansen would not only have met with *your* approbation, but that their decision would have been amply ratified by all who are capable of judging accurately on this deep subject. But, with the caution which becomes a Society like this, that looks rather to the future than the present, and would rather be blamed for being tardy in its awards than liable by precipitation to have its judgment reversed, your Council chose rather to wait for such evidence as must be shortly forthcoming and which would prevent the possibility of error in their judgment. It was expected that the publication of the complete development of the theory on which the Tables are based would speedily follow them, and we were waiting impatiently for this formal justification, that we might crown the *entire work* with our award.

In defect of this publication your Council have felt that they have still the best possible guarantee for the justice of their decision in that most severe test of comparison with the Greenwich observations, to which the Tables have been subjected, and they feel sure that no additional knowledge of the fundamental theory can tend to invalidate this testimony to their unrivalled excellence. The work is indeed perfect in theory and practice, emanating from the profoundest conceptions of the analytical developments required, and elaborated by a union of skill and industry rarely found in the same individual.

I have previously said that it is exceedingly difficult to find in the same man all the attainments necessary to form a great theoretical and a great practical astronomer, but it is rarer still to find one who in his own person unites not only these qualities, but also the patient industry of the experienced computer, with a modesty which caused him unhesitatingly, when compelled by necessity, to accept the drudgery of performing the numerical computations of the simplest character. Indeed, I should have told you, Gentlemen, that when the funds furnished by the Danish Government were withdrawn, he was prepared to continue the necessary computations unaided, and only lamented the length of time which must on this account elapse before his great work could be completed.

Thus, whether we consider the moral or the intellectual character of the man before us; whether our attention be fixed

on him who, *tenax propositi*, could look undaunted on difficulties apparently unsurmountable, and undertake for the present the meanest offices, that he might realise the brilliant future which he saw already within his grasp,—or whether we look to him who unites in his own person the qualifications of the profound analyst and the skilful observer,—the originator of new theories in physical, and of new methods for the treatment of instruments in practical astronomy,—he is equally the object of our respect and admiration, and one who confers honour on us by his acceptance of the award of our Medal.

(*The President, then delivering the Medal to Admiral Manners, addressed him in the following terms*):—

Admiral Manners,—I request that you will be pleased to transmit to M. Hansen this Medal, as a means of expressing in a formal manner the opinion of this Society of the excellence and the great importance of the work for which it is awarded. Request him to accept it as the tribute of our unqualified admiration of this great work, and of the brilliant talents, the rare sagacity, and the patient industry which have contributed to produce it; and convey to him at the same time our best wishes for his future health and happiness, and for the successful issue of any other works which he may be prompted to undertake.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Rev. Robert Main, M.A.

Vice-Presidents:

Augustus De Morgan, Esq.
Rev. Baden Powell, M.A. F.R.S.
Rev. C. Pritchard, M.A. F.R.S.
Admiral W. H. Smyth, K.S.F. D.C.L. F.R.S.

Treasurer:

Samuel Charles Whitbread, Esq., F.R.S.

Secretaries:

Richard C. Carrington, Esq.
Warren De La Rue, Esq. F.R.S.

Foreign Secretary:

Admiral R. H. Manners.

Council:

J. C. Adams, Esq. M.A. F.R.S.
G. B. Airy, Esq. M.A. F.R.S. Astronomer Royal.
Arthur Cayley, Esq. F.R.S.
Captain Alexander Ross Clarke, R.E.
Richard Farley, Esq.
Rev. George Fisher, M.A. F.R.S.
Charles Frodsham, Esq.
Richard Hodgson, Esq.
William Lassell, Esq. F.R.S.
John Lee, Esq. LL.D. F.R.S.
Charles V. Walker, Esq. F.R.S.
Rev. Thos. Wm. Webb, A.M.

ERRATA.

Page 69, *dele* the announcement of the election of the Rev. P. A. Fothergill as a Fellow of the Society.

Page 69, line 12, *for* termination *read* terminator.

London:—Printed by G. Barclay, Castle St. Leicester Sq. and Published at the Apartments of the Society, March 12, 1860.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *March* 9, 1860. No. 5.

The Rev. R. MAIN, President, in the Chair.

Capt. H. Ingold Strutt, Southampton;
Rev. Thomas Wiltshire, Rectory, Bread Street Hill;
W. Corbett Burder, Esq., Clifton, near Bristol; and
Richard Ford Heath, Esq., Totteridge Park, Herts,

were balloted for and duly elected Fellows of the Society.

The twenty-eighth volume of the *Memoirs* is now ready for delivery. It contains the longer papers read at the Meetings of the Society, and also the nineteenth volume of the *Monthly Notices* reprinted in a quarto form, thus giving a complete account of the proceedings of the Society during the year. The price is *2s. 6d.* to Fellows, and *7s. 6d.* to the public.

The Astronomer Royal addressed the Society in reference to the observation of the approaching Total Eclipse of the Sun, on 1860, July 18; which will be visible in no part of Europe except the north-easterly portion of Spain, its shadow entering on the northern coast (the centre being not far west of Santander) and crossing the peninsula nearly in a south-easterly direction.

The first point adverted to was the locality favourable for observation, due regard being had to the facility of approach. The remarks were, however, confined to the north-west portion of the shadow-path, as being that which is most likely to be

visited by English astronomers. It was pointed out that the Pyrenees, (which, commencing from their Mediterranean extremity, range in a westerly direction to the longitude of Bayonne, and nearly so far serve as the boundary between France and Spain,) do not cease to exist as a mountain-chain at the longitude of Bayonne, but are continued in the same direction under the popular names of "the Cantabrian Pyrenees," "the mountains of Asturias," &c., ranging parallel to the north coast of Spain, at a distance of 40 or 50 miles from it. Their elevation can be judged of from the circumstance that the lowest gaps which could be found for the railways going southward from Bilbao and Santander are 2500 feet high. The tract between these mountains and the coast is reputed to be liable to fogs, but it appeared to be the opinion of persons locally acquainted with them, that excellent observing stations, offering every probability of clear sky, might be found on points of much greater elevation, perhaps not less than 5000 feet, in the neighbourhood of Orduña (on the road south of Bilbao), or in the neighbourhood of Reynosa (on the road south of Santander). From these mountains there proceed two chains in the south-east direction, bounding the valley of the Ebro. That on the north-east side of the valley is rather general high land with occasional valleys than a distinct mountain-chain; and, as it is far from the centre of the shadow, being generally near to its north-east boundary, it is less likely to be visited (although observations in this part of the track, south-west of Pamplona for instance, in conjunction with other observations in the centre and south-west part, will be exceedingly valuable). But the chain on the south-west side of the valley is nearly in the centre of the shadow, and is much more likely to be visited. This chain proceeds in the south-east direction for a considerable distance, till it is suddenly cut off by the valley of the Xalon (a large affluent of the Ebro); it then turns suddenly in the south-west direction, with a point of flexure almost as sharp as the apex of the letter V, and that point of flexure is the lofty mountain Moncayo. (In the hollow of this V rise the streams of the Douro.) This position must possess remarkable advantages for the view of the landscape under the successive phases of the eclipse; it is also precisely under the centre of the shadow; and for these reasons and for its elevation it has been selected by M. Faye with the French official expedition, as also (it is understood) by Don Antonio Aguilar with a party of Spanish astronomers, for their place of observation. The chain between Reynosa and the Moncayo presents undoubtedly many favourable positions; the mountains of Pancorbo, near the pass bearing the same name through which is led the royal road between Vitoria and Burgos, have been specially mentioned.

Many persons, however, may be unwilling to risk the possible inconveniences of mountain excursion in a country which has always had the reputation of presenting great difficulties to the

traveller who leaves the highroads. To those who wish to confine themselves to towns, the following may be suggested: Vitoria, said to have good accommodations, on the royal road, north-east of the central shadow; Miranda, a smaller place, on the same road, nearer the centre; Burgos, on the same road, south-west of the centre, its accommodations are said to be not equal to those of Vitoria. It is probable that all these places will be very hot. Bilbao and Santander will offer good accommodations, but Santander, being a sort of watering-place for the inland inhabitants, will probably be as full of visitors in this hot season as our own Scarborough would be.

In regard to the access to these places:—a traveller by land reaches Bayonne without difficulty by the excellent French railways, and then if going in the direction of Vitoria and Burgos he will find numerous diligences. But he may have some difficulty in returning by the same route, as, during the watering-place season, every seat will probably be taken in the diligences going northward from Madrid. It is believed, however, that no difficulty will be found in procuring a voiturier at Bayonne, and the Astronomer Royal is authorised by Mr. Vignoles to say that he will be happy to aid in making arrangements for this purpose. Similar assistance has been offered by M. D'Abbadie, who dwells in the immediate neighbourhood of the French and Spanish boundary.

To those, however, who are willing to brave the inconvenience of a sea-voyage, it is evident that great facilities for the conveyance of instruments would be given by a direct voyage from England to Bilbao or to Santander. The Astronomer Royal distinctly expressed that he had no authority to make any promise or engagement in reference to this matter, but stated that he had no doubt that, if several persons joined in desiring a sea-conveyance to that coast, the Admiralty would appoint one of their steamers for this service. At the same ime, he wished to point out two circumstances. First, For communication with the valley of the Ebro, and also for certain assistance to which he would shortly advert, the port of Bilbao would be the more favourable. But he had lately heard that its bar is in a bad state, and a large Government steamer might be unwilling to attempt the entrance. At Santander there is no difficulty. Secondly, It is obvious that a large steamer cannot be kept waiting long, but must be dismissed for her return as early as possible. Every astronomer, therefore, arriving by the steamer, must be held engaged to re-embark his instruments and himself if he wished to return with the same ship, with all possible despatch after completing the observations.

Supposing, however, that a footing is gained on Spanish ground, the arrangements for proceeding (in a country where above all others it is necessary to make such arrangements in good time) are next to be considered. And he had the gratification to announce that, by a most favourable combination of

circumstances, facilities of no usual kind might be anticipated. A railway is now in progress from Bilbao, crossing the mountains in a southerly direction to Orduña, and turning down the valley of the Ebro to and beyond Miranda and Logroño. The whole of this district is occupied by English engineers, railway officers, and workmen, under the direction of Charles Vignoles, Esq., engineer-in-chief, Mr. Brassey, contractor, and Mr. Bartlett, sub-contractor; and the Astronomer Royal is authorised by Mr. Vignoles to state that every possible assistance, in the nature of lodgings, food, assistance of men, horses, and carriages, will be given by his party; and he believes that Mr. Brassey and Mr. Bartlett concur in this offer. Another railway is in progress from Santander, crossing the mountains in a southerly direction by Reynosa; and the Astronomer Royal is authorised by Philip Sewell, Esq., engineer of that railway, to offer the services of himself and his parties to astronomers.

It is proper to mention that these gentlemen, and others personally acquainted with the country, insist on the importance of arriving early in Spain. Movements of every kind are so slow that unless an unnaturally long period be allowed for every inquiry, reconnaissance, or journey, it will not be properly effected. Apparently all parties ought to have passed the frontier not later than July 8; and even an earlier time was recommended as preferable.

Pamplona, near the north-east boundary, will be easily reached by diligence from Bayonne. For the south-west boundary, it is said that there are coasting steamers to Gijon, whence Oviedo is reached; Torquemada and Aranda are on high-roads from Burgos to Madrid.

A very serious difficulty had been anticipated by many persons, from the amount of the Spanish custom-duties on instruments imported (30 per cent *ad valorem*, without drawback on return), and from the severity of the custom-house examination. The Astronomer Royal had written officially on these points to Lord John Russell, Secretary of State for the Foreign Department, and had been assured by his lordship that the matter should be duly represented to the Spanish Government. But before such representation could have reached Madrid, a letter had been addressed to him by Don Antonio Aguilar, Director of the Observatory of Madrid, for a translation of which he is indebted to Mr. Main, President of the Society. The letter is to the following effect:

"*To the Director of the Observatory of Greenwich.*

"At the request of the Observatory of Madrid, the Spanish Government has made the following arrangements with the [illegible] of facilitating as much as possible the performance of its responsibilities towards foreign astronomers who may

come to the Peninsula in the approaching month of July to observe the eclipse of the sun which has then to be verified.

"1st. In the Spanish custom-houses will be admitted without payment of any duties the instruments which the astronomers may bring for observation in the study of the phases and peculiarities of the eclipse; without prevention, notwithstanding, that the aforementioned places take the necessary precautions for the purpose of hindering persons, strangers to science, from fraudulently introducing other effects resembling these, by taking advantage of a similar privilege.

"2d. In case of necessity, and at the request of the expeditionary *savans*, the local authorities shall afford effectual assistance, so that they may freely and securely be able to acquit themselves perfectly of the task intrusted to them.

"3d. The rectors of the universities nearest to the zone of the eclipse will take care also, with the utmost precision, to give information to the astronomers, from their practical acquaintance with the country and with the most habitual atmospheric phenomena; to accompany them in their scientific excursion; and to take, on the invitation of the same, a part more or less active in the operations which they will engage in.

"To the end that the good intentions of the Spanish Government may not be frustrated, it will be proper that the astronomers who intend to observe this eclipse should communicate with the Observatory of Madrid, indicating approximately the time of their coming, the point from which they intend to penetrate into Spain, the region in which they wish especially to station themselves, and the number and quality of the instruments which they will bring with them. In regard to such communications — which, on account of the nearness of the epoch, ought to be made without loss of time — the observatory will take care to give to the astronomers who will favour it with them, subjects of information which may be of interest, acquainting every person of the travelling arrangements made by the others; so that they may all, if they please, proceed systematically in avoiding excessive augmentation of observers at one point, and a complete neglect of other stations no less advantageous or interesting. Thus, admitting the convenience for the complete observation of the accidents and phases of the eclipse, of the adoption of four principal stations in the mountains of Oca, Moncayo, the neighbourhood of Calatayud, and Peña-Golosa — for example, the points of preliminary reunion might be at the cities of Burgos, Agreda, Calatayud, and Castellan de la Plana, of easy access, where the astronomers could have circumstantial information of the neighbouring territory, expert guides, and zealous authorities, to whom they might apply in case of need, and where every one could arrange his plan of operations with full knowledge of that adopted by the rest of his colleagues, and afterwards

all might compare results with each other. On this point I may be excused the statement that, with decided preference, M. Le Verrier has already declared his fixed determination that the French expedition, of which M. Faye has the direction, shall be stationed upon the heights of Moncayo.

"The Observatory of Madrid will feel itself honoured if any will accept its services offered to the expeditionary astronomers, with this design, that an account of this short letter may be given in the academical and scientific publications; so that its aspirations and desires may speedily come to the knowledge of all those who have an interest in it.

"The Director of the Observatory,

"ANTONIO AGUILAR.

"*Observatory of Madrid, February* 25, 1860."

The Society would acknowledge gratefully this spontaneous act of the Spanish Government and the Spanish astronomers, expressing so fully and so carefully their readiness to remove every difficulty, and to render every assistance which under any possibility they might be able to give. At the same time, the Astronomer Royal invited particular attention to that part of the letter which requests the most exact preliminary information (at least as far as circumstances permit) of the names of the observers, their place of entrance, the number and nature of their instruments, and the place at which they propose to make observations. Careful compliance with this request will evidently enable the Spanish Government to make far better arrangements for the advantage of observers than could possibly be made without it. The Astronomer Royal, in his correspondence with Lord John Russell, had anticipated the necessity and advantage of such a course.

Remarking the number of points to be arranged with the British Government, the Spanish Government, the English friends who had offered their assistance, and others, it is evidently desirable that all correspondence should pass as far as possible through one centre of correspondence. The Astronomer Royal undertook that, if English observers should think that he could render the services to which he alluded, he would with pleasure place himself at their disposal; and he would beg them, as early as could conveniently be done, to address themselves to him in reference to all matters connected with the observation.

The Astronomer Royal stated that he hoped to have the society of M. Otto Struve, who, with M. Winnecke, proposed to accompany him wherever he should decide on going. He added, that in regard to the selection of a place, his thoughts turned towards Reynosa, or some position among the mountains not far south of Reynosa.

The Astronomer Royal then adverted to the observations

and the mode of making them. And in the first place, he took the opportunity of correcting the impression which might be left by some remarks which he had made on a preceding evening. He had pointed out, as what he had certainly seen, that red prominences disappeared on one side of the moon, and new ones appeared on the other side, just as would happen if these red prominences were parts of the sun; but that he could not undertake to say that their elevation increased on one side and diminished on the other side by the proper quantity. He had now to say that this point had been entirely established by the measures made by M. Otto Struve at Lomja in observing the eclipse of 1851, which are detailed in his account printed in the *Mélanges Mathématiques et Astronomiques de l'Académie de St. Petersbourg*, tom. i. 3e livraison. In two instances the change of elevation of the prominences was measured with considerable accuracy, and the results agreed well with the relative motion of the sun and moon in the same interval of time. This accordance greatly strengthened the presumption that the prominences distinctly belonged to the sun.

Still it might be conceived that the prominences were, in fact, produced in some unexplained way by the passage of the sun's light grazing some mountains or hollows of the moon, and that their apparent elevation was altered when the course of the sun's light became less deflected by the moon before it reached the observer's eye; and that nothing therefore was proved with certainty as to their origin, unless we could show that in some cases they shifted their apparent position upon the moon's limb. Very great value, therefore, would attach to observations which should define with accuracy the angular position of the red prominences, as referred either to the astronomical meridian, or to any other line whose angle with the astronomical meridian could be calculated. An agreement was already tacitly made between M. Otto Struve and himself, that while M. O. Struve should take care for the elevations of prominences on the front and rear of the moon, the Astronomer Royal should use his best endeavours to settle the angular positions of any which might be seen on those parts of the limb which are nearly parallel to the direction of the moon's apparent motion over the sun. His thoughts, therefore, had been directed mainly to the contrivances for measuring the angular position. Adopting the fundamental principle that no attempt is to be made to use a micrometer, but that measures must be confined to eye-estimations, he proposed the following wire apparatus. From the measured focal length of his telescope, and the known angular diameter of the moon, he would compute the linear diameter of the moon's image. Four wires (or lines etched on glass) would be placed in the field of view, in such a position as exactly to include this image; and by these, with ordinary rackwork motion, he conceived that he could at any time very quickly bring the moon's image to an almost perfectly

definite position in the field. Each of the four wires should have an external parallel wire at the distance of 1′ for estimation of elevations. Twelve radial wires, at equal angles of 15°, should intersect in the centre, the wires which are parallel to the sides of the square being thicker than the others, for facility of reference. The great elevation of the sun above the horizon (about 59° at the beginning of the eclipse, 54° at the middle, for the station Reynosa) made it very desirable that the observations should be made with a diagonal reflector, turning the rays of light, however, not necessarily 90°; and, the reflector being supposed to be of glass, he thought it would be most convenient that the lines above mentioned should be etched on the surface nearest to the eye. In regard to the mounting of the telescope, he proposed to abandon all idea of equatoreal motion, knowing the difficulty of making the proper adjustments in a field-telescope, and intended to mount it with an ordinary altitude-and-azimuth motion, the verticality of the axis being ascertained by a small spirit-level. With this mounting it is easy to verify, by a terrestrial object, the horizontality of one wire; and this circumstance would determine the position of his system of wires. It appears, from the numbers given in Mr. Hind's ephemeris, that about the middle of the eclipse the moon will appear to be moving across the sun in a direction inclined about 16½° to the horizon. He would therefore use, as his principle of verification, that the line next to one of the strong lines of which he had spoken should be exactly parallel to the horizon; and then that strong line would show the direction, within 1½°, of the moon's apparent motion, and the observer would be fully prepared to decide on the portions of the limb to which his observations ought to be directed.

It is an unfavourable feature of this mode of observation that the power of the telescope must be rather low; but he thought it likely that the advantages accompanying the method would more than balance that disadvantage.

The peculiarities of the method which he had described attach only to the determinations of angular position. For the measure of elevations, even by estimation, each observer would devise his own method, of a different class from that above mentioned, and a high power might undoubtedly be used. Those observers who wished carefully to scrutinise the prominences would of course use high powers. But on those observations, and on those of colour, polarisation, &c., or on those of the corona, or of external appearances, he did not intend to speak.

The method of registering time had been a subject of repeated consideration, and he thought that it might be best to make no time-record whatever, except perhaps of disappearance and reappearance. He proposed to hold in his hand a book in which he could write without looking at the book, inscribing only a

few symbols on each opening according to a system which he himself should understand. He proposed to divide the whole time into observations, estimated as of equal duration; any turning away from the telescope to look at the sky or the landscape being also an observation; and to appropriate to each of these one opening in his book (even though no entry should be made). The durations might be so nearly equal that no sensible error would be committed by assuming them to be equal; and then the precise epoch of any observation would be found with sufficient accuracy by simple interpolation between the times of disappearance and reappearance. G. B. A.

Extract of a Letter from M. D'Abbadie to one of the Secretaries.

"The *Monthly Notices* are not sufficiently explicit about the coaches which ply daily between Bayonne and Madrid. In July many rich Spaniards go to the north for coolness and sea-baths, and it is then necessary to bespeak places three weeks in advance. Not so in proceeding from Bayonne to Burgos; for the coaches, being generally empty in July, travellers get places easily, and at half price: those going to the north then pay double. I beg you to communicate this part of my letter to Professor Airy, and add that Pancorbo is an excellent station (separated from Miranda by a narrow mountain-pass) on the north end of an elevated plain, where the village Cubo is on the line of central darkness. There is no good map of those parts, and none shows Altotero, somewhere in the neighbourhood, and a geodetical station in the new Spanish survey."

Dated Paris, 1860, *Feb.* 24.

An Eye-piece for the Solar Eclipse.

Mr. Carrington exhibited an eye-piece which he believed to be well adapted for reading off angles on the limb of the sun or moon during the ensuing eclipse, the main principle involved being that of Bessel (see *Ast. Nach.*, vol. xvi. p. 161), modified by Dr. Littrow (*Ast. Nach.*, vol. xxxiii. p. 129). The further modification proposed is the common diagonal reflexion, partly on account of the altitude of the sun in the coming eclipse, and partly because the light of the sun can in that construction be very conveniently reduced before totality by Mr. Hodgson's

plane glass reflector, to be replaced by a silvered reflector or prism shortly before the totality. In the field is a glass plate, divided by fluoric acid lines into right-angled parallelograms, with sides in the proportion of 3 : 2 for ready distinction, and capable of rotation through 90° only. It has commonly been represented that the method of observing a position-angle on a limb by Bessel's wire consisted in placing the wire strictly as a tangent to the limb at the required point; but it is obvious that a chordal line parallel to the tangent equally gives the same angle, while the estimation of the equality of distance between the object on the limb and the two chordal points of intersection is more readily made than that of a true tangency. The observation proposed by Mr. Carrington would consist of noting the quadrant and the angle shown by the index which will be in all cases less than 90°; and he pointed out that in the construction exhibited, in which rotation through an angle of 90° was alone possible, confusion of adjoining quadrants was impossible, and the particular quadrant easily recorded. The quadrant would be recognised by any notation the observer might adopt to indicate that the preceding or following limb was placed on a long or a short side of one of the parallelograms. The size of any object seen would be readily estimated by comparison with a side of a parallelogram. Mr. Carrington further recommended the use of an altazimuth mounting, admitting of free reversion of the telescope, and that a small vertical circle be attached to the telescope, with a reading level for the purpose of adjustment of the vertical axis at the commencement of operations, and of its verification afterwards. Without further explanation the observer would see that by these means he would have his angles under perfect and ready command, and would have a convenient position for himself while observing. While making this proposal for the consideration of observers who prefer direct vision, Mr. Carrington still reserved a previously-expressed opinion about the desirability of others projecting the image on a properly shaded screen, and that photographic registration would be superior to either.

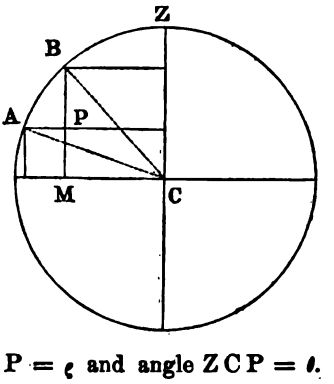
Proposed New Design for Vertically placed Divided Circles.
By R. C. Carrington, Esq.

Mr. Carrington exhibited a small brass model of a circular plate for carrying the divisions in instruments of the larger dimensions, intended for the observation of zenith distances in or out of the meridian. It is well known that the theory of the flexure of vertical circles of large dimensions, provided with numerous radii, is complex, and that the best provision for equal temperature is not thereby attained; so that some

have proposed to reduce the dimensions to little more than a foot, in order to replace a spoked wheel by a plane plate of not greater dead weight, and to compensate the smallness of the circle by increased power of the reading microscopes. Cast circles with spokes are also well known to be in a state of excessive molecular strain by the occasional occurrence of the rim parting company with the spokes while on the lathe. Mr. Carrington thinks the form exhibited worth consideration, namely, that of a thin corrugated circular plate, obtained by turning down in a lathe by opposed cutting tools an annealed cast plate till the skin of the original plate and unnecessary metal be entirely removed. By taking advantage of the strength afforded by the corrugated section, and by turning down from the rim inwards to the axis, the thickness and dead weight might be rendered very trifling, while the resistance to gravity flexure would be considerable and the same at all angles, and the greatest facility afforded for rapid equalisation of temperature. The cost of a pattern, which is a considerable item in the shop account, is avoided, and the engineer's finishing work reduced to lathe-work only. The finished plate might with advantage and at moderate cost be electro-gilt.

Formulæ for the Reduction of Pastorf's Observations of the Solar Spots. By R. C. Carrington, Esq.

Mr. Carrington communicated the following formulæ, in the hope that some gentleman, possessing more leisure than himself, might, by their use, extract some results from the three volumes lately presented to the Society by Sir J. Herschel. There is no probability of his doing so himself.



Z being the zenith point of the sun, Pastorf fixed the position of a spot P by measuring the lines $PA = a$ and $PB = b$. To render his measures available, it is required from these highly inconvenient data to find $CM = x$ and $PM = y$, or the distance $CP = \varrho$ and angle $ZCP = \theta$. Manifestly,

$$r^2 = (a + x)^2 + y^2$$

and $$r^2 = (b + y)^2 + x^2,$$

By the solution of which we find

$$x = \tfrac{1}{2}(bm - a),$$

$$y = \tfrac{1}{2}(am - b), \quad \text{where } m^2 = \frac{4r^2}{a^2 + b^2} - 1,$$

whence also result

$$\varrho = \sqrt{r^2 - abm} \quad \text{and} \quad \tan\theta = \frac{bm - a}{am - b}.$$

Example. 1832, May 5, 2^h P.M., *Mercury* on the sun (see *Ast. Nach.* No. 228, p. 199), $a = +427''$, $b = -707''$. By ephemeris, $r = 953''$. Whence result

$$x = +521''{\cdot}5, \quad y = -90''{\cdot}5, \quad \varrho = 529''{\cdot}5, \quad \theta = 99^\circ\,51'.$$

On some previous Observations of supposed Planetary Bodies in Transit over the Sun. By R. C. Carrington, Esq.

In the *Notice* for January I gave a short list of cases extracted from a recent work of my friend Professor Wolf, after rejecting two or three, which a reference to the original will justify. The case of June 6, 1761, may also be rejected, as a reference to the *Ast. Jahrbuch* for 1778 showed that Scheuten was alleging the existence of a satellite to *Venus* seen by him to accompany that planet in its transit on that day. Whatever his observation may be worth, it does not bear on the present question of inferior planets. I find that, in hastily referring to Schmidt's observation of Oct. 11, 1847, I confused the German words *weder* and *entweder* in a way which materially affects his meaning; and the Rev. T. W. Webb has pointed out to me that Professor Wolf and I had overlooked a second similar case under date of Oct. 14, 1849. I do not myself attribute much importance to either of these cases; but as others may differ in opinion, and the dates are singularly concurrent with those of Stark in 1819 and Fritsch in 1802, I give the actual passages from Schmidt's *Resultate*, p. 26: —"1847 (Bonn), October 11. . . . There also (at 9 hours in the morning) rapidly passed across the sun a small black point. This was neither an insect nor a distant bird." Page 31. "1849 (Bonn), October 14. At 11 hours I saw a dark body, about 15″ in size, pass [*vorüberfliegen*] very rapidly from east to west before the sun. It was neither a bird nor an insect."

Schmidt has also a third case, at page 32 of his *Resultate*, under date Feb. 28, 1850, which again singularly concurs with

the date of Staudacher's observation in 1762, at *the end* of February. Schmidt's note is:—"$10^h\ 12^m$ A.M. A dark body, fully 30″, passed over the sun from west to east. I doubted at the time its being a distant bird:" [that is in an unfavourable sense. The original is: "*Ich zweifelte gleich, dass es ein ferner Vogel gewesen sei.*" As we should say, "My impression at the time was that it was not a bird."] But the direction of movement probably condemns the observation as one of a planetary body.

The account of Dangos's observation may be found in the *Jahrbuch* for 1804, pp. 185 to 187, as a "More accurate Notice of the Moving Spot seen on the Sun on Jan. 18, 1798, by M. Dangos, at Tarbes, in South France. Communicated by M. Mechain." The whole account is too long for translation, but the circumstantial part is as follows:—"About 2^h p.m. on the 15th and 17th of January, no spots visible; on the 18th, at about $1\frac{1}{4}^h$, Dangos found a small spot in the west part of the sun, about half way between the centre and the limb, very dark, round, and sharp. At $1^h\ 58^m$ it was only one-fourth (of the radius?) distant from the limb, and the first and last contacts of the exit took place at $2^h\ 7^m\ 12^s{\cdot}5$ and $2^h\ 8^m\ 48^s{\cdot}0$ respectively." The reader must judge of the value of all this for himself.

In the case of the observation by Canon Stark, of Augsburg, on Feb. 12, 1820, so singular a confirmation exists that I first give his entire account, rendered into English from his great *Meteorologisches Jahrbuch*, a work for many years (1813 to 1837) printed at his own expense. His words are:—"On the 12th, a singular spot of well-defined circular form, with circular atmosphere and orange-gold tint, was seen, the apparent size of which was nearly twice the apparent diameter of *Mercury*. At noon, 12^h, this spot was 11′ 20″ distant from the east limb, and 14′ 17″ from the south limb of the sun; and at $4^h\ 23^m$ in the evening it was no longer to be seen. The appearance was rather indicative of the transit of a planetary heavenly body, having its path included within that of *Mercury*, than of a solar spot." A confirmation of this observation has been pointed out to me by Mr. Marth, in a passage of a letter of Olbers to Bessel, dated 20th June, 1820 (see *Corr.*, vol. ii. p. 162). Olbers writes:—"What do you say to Steinhübel's observation of a dark, round, well-defined spot, which, on the 12th of February of this year, completed its transit across the sun's disk in five hours? If the thing is a fact, it indicates a planet interior to the orbit of *Mercury*, and whose distance from the sun must be about 0·19, and period over 30 days. If Steinhübel, of whom I know no more than that he has published some sun-spot observations, is actually a good and true man, it may perhaps be worth the trouble for Littrow to endeavour to procure from him the more exact circumstances of this observation, and in particular . . . ." Thinking that the

present Dr. Littrow might be able to add something to this information I wrote to him; but have received for answer that he only knew that Steinhübel was a private observer, who died about thirty years ago, and that in his opinion it was very improbable that he had any connexion with Stark, a canon of Augsburg. It seems impossible here to withhold the opinion that either a planet or comet in perihelion was observed by two independent private astronomers of Germany.

Mr. Capel Lofft's observation, on Jan. 6, 1818, is described very circumstantially in a letter to the *Monthly Magazine* of Jan 10, 1818, and the account republished by Mr. Webb in his *Celestial Objects for Common Telescopes*, p. 32. For the convenience of readers not in England the insertion of the account in this paper may be permitted: "I saw it about 11 A.M. (on Jan. 6) with my own reflector, with a power of about 80, with an excellent Cassigrain reflector made by Crickmore of this town (Ipswich) with about 260, and with a reflector of Mr. Acton's with about 170. It appeared when I first saw it somewhat about one-third from the eastern limb, sub-elliptic, small, uniformly opaque. About $2\frac{1}{2}$ hours P.M. it appeared to Mr. Acton considerably advanced, and a little west of the sun's centre, and I think it appeared then 6 or 8 seconds in diameter. I had been able to see no spot on the 4th, nor again on the 8th, and even on the 6th Mr. Crickmore could not see it a little before sunset, though the telescope already mentioned gave him every advantage. . . . Its state of motion seemed inconsistent with that of the solar rotation, and both in figure, density, and regularity of path, it seemed utterly unlike floating scoria. In short, its progress over the sun's disk seems to have exceeded that of *Venus* in transit. . . ."

The observation by Mr. Benjamin Scott, in July 1847, of a body of the appearance of *Mercury* in transit, appears to be partly confirmed by an alleged observation at (as nearly as can now be ascertained) the same time and under similar circumstances by a Mr. W. Wray, optician, of 25 Torriano Terrace, Kentish Town, London, N.W. Mr. Scott has favoured me with copies of Mr. Wray's letters to himself in the present year and to Mr. James Breen, then of Cambridge, in 1848. This case may be referred to on a future occasion should either of the observers succeed in recovering the date with more certainty.

The above cases are merely given for as much as they may be worth in the reader's opinion, but are, at least, of unusual interest at the present moment.

Future Observations of the supposed New Planet.

By M. R. Radan.

(*Translated and communicated to one of the Secretaries by M. D'Abbadie*)

The investigations of Professor Le Verrier on the theory of *Mercury*, and the procrastinated publication of Dr. Lescarbault's remarkable observation, have more than ever called the attention of the learned to the inframercurial planets. Since 1761, mention has been frequently made of one or several dark spots passing before the sun. Professor Wolf, of Zurich, has published, in 1859, a list of upwards of twenty such occurrences. The two observations of Lichtenberg (1762, Nov. 19), and of Hoffmann (beginning of May, 1764), seem both to point out one and the same planet; while those of Fritsch (1802, Oct. 10), and of Stark (1819, Oct. 9), have a very probable relation with the planet of 1859, March 26, for which the obvious name of *Vulcan* has been proposed.

I shall briefly state the results obtained by combining the incomplete results of Stark and Fritsch, with the much more available observation of Dr. Lescarbault. Neglecting small corrections, one may put the equation of the passage in the following form:—

$$c\,D-(c-a).\,D^{\frac{3}{2}}=a\sqrt{\frac{2\,D\,D_1}{D+D_1}},$$

D and D_1 being the distances from the sun to *Vulcan* observed in March and October (*i. e.*, near the descending and ascending nodes), c the chord travelled over the sun's disk, and a the angular displacement of the earth during the passage. This latter having lasted $1^h\,17^m\,8^s$, we have $a = 3'\,11''$, and c was $9'\,17''$; $\therefore \frac{a}{c-a} = 0{\cdot}521$, and

$$1{\cdot}521\,D-D^{\frac{3}{2}}=0{\cdot}521\sqrt{\frac{2\,D\,D_1}{D+D_1}}; \qquad (1)$$

Naming Δ the mean distance, ϵ the excentricity, putting $D = x^2$, and $\Delta(1-\epsilon^2) = x^2(1+\lambda)^2$, we obtain

$$x+x^3=0{\cdot}521,$$

which would give $x = 0{\cdot}381$, the distance $x^2 = 0{\cdot}143$, the time of revolution $t = x^3 = 0{\cdot}054$ year $= 19{\cdot}7$ days. But this would be a particular case: generally speaking, we have only one relation between D and D_1 available to fix certain limits for the elements of the orbit. Taking at once 0·25 for the superior limit of *Vulcan's* distance from the sun, one finds by equation (1) that $D<0{\cdot}176$ as long as $D_1<0{\cdot}25$; and since $\frac{2}{\Delta(1-\epsilon^2)}=\frac{1}{D}+\frac{1}{D_1}$, we get $\Delta(1-\epsilon^2)<0{\cdot}206$, or $1-\epsilon<\frac{0{\cdot}206}{\Delta(1-\epsilon)}$. Now the aphelion

distance $\Delta\,(1+\iota) < 0{\cdot}25$; giving it different values between 0·206 and 0·25, an inferior limit is at the same time obtained for ι between 0 and 0·176, and for Δ a superior limit between 0·206 and 0·221. Therefore Δ will be in all cases $< 0{\cdot}221$, and $t < 38$ days. The inclination must be included between 11°·5 and 12°·5; therefore *Vulcan*, already 15′ 22″ distant from the centre of the sun's disk on the 26th March, would not be visible on the 25th of the same month. Computing the longitude of the ascending node with a supposed distance D, and the limit of visibility in October with the corresponding distance D, it follows that D_1 must be $< 0{\cdot}19$ and $D < 0{\cdot}16$, in order that *Vulcan* may be visible beyond the 10th October. Therefore $\Delta\,(1-\iota^2) < 0{\cdot}17$, the inferior limit of ι will remain between 0 and 0·3, and Δ will be $< 0{\cdot}19$, $t < 30$ days; the synodic revolution be $= \frac{365\,t}{365-t} < 32^d$.

The intervals between the three observations are 6208 and $14413 = 2 \times 6208 + 1997$ days. We may suppose $6208 = m\,s + \mu$, $1997 = n\,s + \nu$, m and n being whole numbers, and μ and ν small uncertainties. The remainder of $\frac{6208}{1997}$ is 217, that of $\frac{1997}{217}$ is 44, &c., and $217 = (m - 3\,n)\,s + \mu - 3\,\nu$. Therefore 217 may still be considered as a multiple of s; but the ensuing remainders, 44, &c., are already too much altered by the uncertainties μ, ν, to show the traces of s. We already know that $s < 32^d$: we must, therefore, choose between the quotients $\frac{217}{7} = 31$, $\frac{217}{8} = 27$, $\frac{217}{9} = 24$, &c., a number which will be a common divisor to 6208 and 1997. Unfortunately these intervals are too large to prevent finding common divisors differing slightly from any one of the preceding quotients. Confining ourselves, therefore, to these two, 31 and 27, the others being smaller than the sun's rotation (which must not, according to Laplace, happen to any planet), we find nearly

$$6208 = 199 \times 31{\cdot}197 = 202 \times 30{\cdot}730 = 227 \times 27{\cdot}350 = 230 \times 26{\cdot}990,$$
$$1997 = 64 \times 31{\cdot}197 = 65 \times 30{\cdot}730 = 73 \times 27{\cdot}350 = 74 \times 26{\cdot}990.$$

The relation $s = t\,(1 + \frac{s}{365})$ shows that $6208 = m\,s = 17 \times 365$ will contain $m + 17$ times t: consequently $t = \frac{6208}{216} = 28{\cdot}741$, and for the other cases likewise 28·305, 25·443, 25·113. The periods of return would be 1 Julian year +9, +4, +8, +13 days. One might conclude, therefore, that *Vulcan* will be seen perhaps, on April 4 next, at 2^h A.M. (in China), or March 29, about noon, or April 2, between noon and 5^h, or finally, April 7, between 10^h and 3^h, Paris civil time. Taking $t = 28{\cdot}74$ or $= 25{\cdot}44$, Δ would be $= 0{\cdot}18$ and $= 0{\cdot}17$. In the first case

$\epsilon \geq 0{\cdot}25$, $\Delta(1+\epsilon) \geq 0{\cdot}23$. On July 18, 1860, the places of *Vulcan*, excluding inequalities of motion, would be

Geocentric latitude $+1^{\circ}{\cdot}1$, $+0^{\circ}{\cdot}6$, $+1^{\circ}{\cdot}2$, $-1^{\circ}{\cdot}3$ } in the 4 suppo-
„ longitude $112{\cdot}5$, 122, 119, $125{\cdot}5$ } sitions.

The sun's longitude will then be 116°, that of *Jupiter* 124°, and its latitude 0·5.

On that day, therefore, *Vulcan* must be looked for in a zone beginning at 2°, or 4 sun's diameters to the south of *Jupiter*, passing a little to the west of the latter on nearing the sun to within 1° or 2 diameters, and finishing 4° to the north-west of the sun.

Paris, February 17th, 1860.

On certain Inductions with respect to the Heat engendered by the possible Fall of a Meteor into the Sun, and on a mode of deducing the absolute Temperature of the Solar Surface from Thermometric Observation. By J. J. Waterston, Esq.

Mr. Carrington's observation of the sun on the 1st of September last having fortunately established the fact of an outburst of light above the solar surface, and thus favouring Newton's conjecture as to the sun receiving a supply of force from bodies descending upon it, it may be worth while, and perhaps assist in the formation of more exact ideas on the subject, if we compute, from Mr. Joules' value of the thermal unit, the quantity of heat and intensity of temperature that would accrue by a body falling into the sun or sun's atmosphere with the velocity due to a parabolic trajectory. The modern interpretation of Newton's conjecture is, that the comet, or by whatever name we call the body, does not supply fuel to a fire as oil to a flame, but that it supplies force to the central radiating energy,—force to be converted into heat and light.

It is true that no body belonging to the solar system can fall into the sun so long as the laws that keep them in their orbits are maintained. A body that has once rounded the sun in a parabola or elongated ellipse, will probably continue to do so, unless in the rare case of the perihelion distance being so close to the sun's surface that a slight perturbation from the other bodies of the system may bring it within the limits of the sun's radius. But when the orbit is a hyperbola it is the first and last appearance of the body, and if, as may chance to happen, the perihelion distance is less than the sun's radius the sun must inevitably absorb it. We are thus certain that the velocity of a body that impinges on the sun must exceed 419 miles per second, for such is the orbital velocity in the parabola that

separates the lesser velocity in the ellipse from the greater velocity in the hyperbola.

With this velocity and Mr. Joules' unit it is easy to compute the quantity of heat due to the conversion of the force ; and if the absolute zero of temperature coincide with —461° F., the zero of gaseous tension, the temperature attainable by a known substance under such conditions may also be exactly computed, independent of any theory or hypothesis whatever.

A pound of water falling through 772 feet acquires a velocity of 222 feet per second, and a force that if converted raises the temperature of the pound of water 1° F. If it falls through 4 times 772 the velocity acquired is twice 222 feet, and the rise of temperature 4°. Suppose the acquired velocity to be 1 mile per second, the corresponding rise of temperature is 564°, increasing as the square of the velocity, and 419 miles per second corresponds to $419 \times 419 \times 564 = 99{,}065{,}000°$.

A pound of iron under the same circumstances would acquire the same force, but that force converted would raise its temperature nine times the above amount, because the same quantity of heat has nine times greater effect in raising the temperature of iron than it has on the same weight of water.

A pound of mercury would have its temperature affected about 30 times the above amount, and so on according to the specific heat of the substance.

Assuming that the specific heat of the body that impinges on the sun is the same as glass, the rise of temperature corresponding to the velocity 419 miles per second is 565 million degrees Fahrenheit.

Thus the intensity of the temperature engendered depends on the molecular constitution of the body; the *quantity* of the heat, however, is independent of everything but the velocity and the mass, and each pound of any body whatever that strikes the sun with the velocity 419 miles per second is endowed with force sufficient to raise a pound of water 100 million degrees of Fahrenheit's scale.

Has the mass of the sun been gradually collocated by matter thus descending? To estimate exactly the probability of this, two other data are required, viz., the temperature of the sun's surface, and the quantity of heat radiating from it in a given time. The first is unknown, but the second we know approximately from the observations of M. Pouillet, also from those of Herschel and Forbes. It is estimated that it suffices to melt a stratum of about two feet thickness of ice at the earth's mean distance, in 24 hours. This is equivalent to 1·04 feet of ice per second at the sun's surface. Now it is known that ice requires 142° F. to melt it, and each of those degrees is equivalent to the work expended in raising the weight of the ice 772 feet, against the force of gravity at the earth's surface. We thus deduce that a weight equal to that of the ice arriving at the sun with a velocity of 2649 feet, or half-a-mile per

second (viz. the velocity acquired in falling through 142 × 772 feet) is equivalent to the force emanating from the sun in one second. If the same amount of force is brought to the sun by matter moving at the minimum velocity of 419 miles per second, the mass of this matter must be less than the mass of the ice, in the ratio of 1 to the square of twice 419. Such a mass is represented by a sphere of 11·068 miles in diameter, of the density of water.

Supposing the sun to have the same density and specific heat as water, and comparing its volume with that of the sphere of 11 miles diameter, and allowing for the time, we find that the sun's annual loss of temperature to be 6°·125. This is a simple arithmetical deduction from the fact, that the sun's heat can melt about 2 feet of ice daily. The observations of Herschel and Forbes fix it at 1·835 feet (*Phil. Trans.*, 1842); those of M. Pouillet at about one-third less (Taylor's *Scient. Memoirs*, vol. iv.). If we assume 1·5 feet as the correct thickness, the yearly reduction of temperature is 4°·59. If the specific heat of the matter of the sun corresponded with iron, this decrement of temperature would have to be multiplied by 9, and so on for other assumed values. But it is convenient to found our ideas of such quantitive relations on water as the standard most familiar.

Assuming the earth to have six times the density of water we may extend these calculations, and comparing volumes and time we deduce 69 as the number of *years* that the sun takes to throw out as much force as would accrue to it by the earth falling down upon its surface. The mechanical force thus supplied would be equivalent to the expenditure of heat-force for 69 years, and the rise of temperature of the whole mass of the sun, supposing the increment of heat uniformly diffused through its mass, would approach so much nearer to the maximum limit 317° (=69 × 4°·59) as the temperature of the planet after impact exceeded the temperature of the sun.

Again, if we suppose the planet after it has struck the surface of the sun to settle into a disk of 60,000 miles diameter, having the temperature of 100 million degrees, the temperature of the sun being 12 millions, we should have $\frac{1}{220}$ of its disk shining with eight-fold lustre, giving out probably eight times as much heat as an equal surface in the normal state. Thus $\frac{8-1}{220} = \frac{1}{31}$ represents the increment of solar heating power, and if we estimate the normal amount as what is required to keep the earth's surface at a mean temperature of 60° F. or 521° absolute, we see that $\frac{521}{31} = 17°$ nearly, is the increment of mean temperature over all the earth that would arise from such a planet-fall.

M. Pouillet infers from his observations on solar radiation,

that the temperature of the sun's surface is at least 2660° F. If it do not exceed this amount, the rise of temperature in the whole mass of the sun would be about 200°; but if, as before, we assume the planet to settle into a disk $2'$ in diameter, we have $\frac{1}{2\,2\,0}$ of the sun's disk shining with 38,000 times the normal force, so that a planet-fall of this magnitude would increase the radiating power of the sun 171 times.

Such extravagant conclusions only show the insufficiency of our data, and demonstrate the uncertainty in which the subject is involved, so long as an approximation to the actual temperature of the solar surface is wanting.

Is it possible to ascertain the temperature of the radiating surface of the sun? Ordinary observations give us the temperature in the sun and in the shade. Suppose the temperature of the sun to be double its present amount, it is probable that the absolute temperature in the shade and in the sun would also be double the present amount; consequently, also, the difference between them. We might thus expect this difference, when due precautions are taken, to be a constant quantity and to be a function of the sun's absolute temperature.

Suppose a thermometer to be enclosed in a vacuum and surrounded on all sides by matter, having a uniform absolute temperature t: we may consider it to be the centre of a sphere, the interior surface of which radiates heat to it, and the balance of temperature to be thus maintained by reciprocal radiation: as much power *issuing from* the thermometer on all sides towards the concave surface of the enclosing sphere as *enters* into it by radiation from the concave surface. There is a dynamic interchange of force in constant operation. If the temperature of the sphere is augmented one degree, the thermometer rises until its radiating power increases to the same amount. If half the concave surface remains at t, while the other half increases from t to $t + 2°$, the rise in the temperature will be the same as before, viz., 1°, because the supply to it is the same as if the whole surface were raised 1°. If $\frac{1}{1000}$ of the surface had the temperature $t + 1000°$, the other parts remaining at t, we have

$$\frac{1 \times (t + 1000°) + 999 \times t}{1000} = t + 1°,$$

the resulting temperature as before. If $\frac{1}{1000}$th of the surface had the temperature of $t + 2000°$, then

$$\frac{1 \times (t + 2000°) + 999 \times t}{1000} = t \times 2°$$

is the temperature of the thermometer, and generally, if $\frac{1}{n}$ of the surface of the sphere had the temperature $t + x°$, we have

$$\frac{(t + x°) + (n - 1)\,t}{n} = t + r°,$$

the temperature of the thermometer. Hence $x^\circ = n\tau^\circ$, a simple relation, by which we can deduce the temperature of the sun's radiating surface, assuming for the present the non-absorption of rays in passing through the atmosphere. It will be remarked that τ° is the difference of reading between thermometers in the sun and in the shade taken with due precautions.

Some years ago, when in India, I tried this by enclosing a thermometer within three concentric boxes well protected from external influences, and capable of being equally heated all round to any temperature below 400°, by means of flues ascending from an Argand lamp. The sun's rays, when near the meridian (having an altitude of about 70° and with the atmosphere perfectly clear and calm), were admitted to fall, when required, on the bulb of the thermometer through a narrow triplet glass partition. I found that 50° was the rise that took place in consequence of exposure. There being glass partitions on both sides, the reading of the thermometer was very distinct by transmitted light. Beginning at 80°, without applying the lamp, the admission of the sun's rays caused the mercury to rise to 130°, where it remained steady. The lamp was then applied at low power, so as to maintain the inner box at this temperature while the sun was excluded. When perfectly steady, the sun's rays were readmitted, and the mercury again mounted with the same alacrity as before, until it reached 180°. This continued step by step up to 250°. No difference, either in the magnitude of the step or the time taken to effect it, could be detected.

Thus, for τ° we have the constant 50°, and n we obtain by comparing the disk of the sun with the surface of the sphere. At the earth's mean distance the sun's diameter is $32'\ 3''{\cdot}6$, hence $n = 183960$ and $x = 9180000^\circ$.

If there is no fault in this mode of proceeding, we may with confidence estimate the solar temperature to be *above* 10 million degrees, probably 12 million, allowing a reduction of one-third from absorption in passing through the atmosphere and the three plates of glass.

A notable fact, in making these observations, is, that the step τ° seems wholly independent of the temperature of the medium in which the thermometer lies. Why this should be is apparent from the equation. Substituting $2t$ for t, and consequently $x - t$ in place of x, is tantamount to heating the box from 80° up to 620°. Let τ' represent (in degrees) the step at this higher temperature $2t$, we have

$$\frac{2t + x - t + (n-1)\,2t}{n} = 2t + \tau' \text{ and } x - t = n\tau' \text{ and } \frac{\tau'}{\tau} = \frac{x-t}{x}.$$

Thus the step diminishes only about the $\frac{1}{18000}$th part or $\frac{1}{360}$th of a degree during a change of upwards of 500° in the box.

From Mr. Carrington's observations it appears that the burst of light was of *much* greater intensity than the sun's normal surface. This is quite consistent with its assumed temperature, which is much less than any probable estimate we can make of the temperature of the conversion of the force of a body striking the sun's atmosphere with a velocity of from 400 to 500 miles per second. The existence of a transparent atmosphere seems also to be positively demonstrated by the blaze occurring above the spots.

Edinburgh, Feb. 15, 1860.

On the Variability of the Proper Motion of Sirius.

By the Rev. R. Main, President.

The object of this communication is not so much to discuss thoroughly the question of the variability of proper motion which has been recently attributed to *Sirius*, but to endeavour to disprove an assertion made repeatedly by Professor Calandrelli, director of the Pontifical Observatory at Rome, of the incorrectness of the N.P.D. of this star given for the epoch 1845 in the Greenwich Twelve-Year Catalogue. The papers in which this charge of incorrectness is made are the following:—

1. Pontificio Nuovo Osservatorio della Romana Universita, Notizie Storiche del Prof. J. Calandrelli.

2. Sopra i Movimenti Propri delle Stelle. Memoria del Prof. J. Calandrelli.

3. Sul Movimento Proprio di *Sirio*, Ricerche Astronomiche, del Prof. Ignazio Calandrelli, &c.

4. Appendice alle Ricerche sopra i Movimenti Proprii delle Stelle Fisse, del Prof. Ignazio Calandrelli, &c.

The first two of these memoirs are printed in the *Atti dell' Accademia Pontificia de' Nuovi Lincei* for 1853, April 3, and 1857, March 1. The last two are separate publications, printed in 1858.

The titles of the memoirs above will show that their principal object is the treatment of observations for the purpose of deducing the proper motions of the stars, and ultimately of resolving in particular the question of the variability of the proper motion of *Sirius;* and there is nothing in them which would require any special remark, if the assertion of the incorrectness of the value of the N.P.D. of *Sirius* given in the Greenwich Catalogue were not repeated in all of them so pertinaciously as to impress the reader with the belief that the fact is undisputed, and that it has arisen from some unexplained errors, either in the observations themselves, or the reduction of them.

The following investigations will, I trust, prove that the place given in the Greenwich Catalogue is correct, and that, in consequence, the greater portions of Calandrelli's reasonings with regard to the proper motion of *Sirius* in N.P.D. are defective.

It would be sufficient for my purpose to discuss only the north polar distances of this star; but I take the opportunity of collecting all the Greenwich observations, both of R.A. and N.P.D., which have been accessible to me in a reduced shape, as a small contribution towards the settlement of the disputed question of the variability of proper motion.

For R.A. the observations extend with tolerable continuity from the years 1765 to 1859; and I use,—

1. Maskelyne's "Ledgers of Stars," edited by myself, and appended to the *Greenwich Observations* of 1851. These extend from 1765 to 1807, with occasional interruptions.

2. Pond's Right Ascensions, given in his various catalogues from 1820 to 1835, with some interruptions.

3. Airy's *Greenwich Observations* from 1836 to 1858, both years inclusive.

A very few explanatory remarks are necessary, before giving the tabulated values of R.A.

As the result in N.P.D. for 1845 has been called in question, I have found it convenient to reduce all the results for individual years to this epoch, and I assume the following values:—

Peters' Annual Precession for 1845, in R.A..	$= +2^{s}\cdot681$
Main's Annual Proper Motion	$= -0\cdot035$
Secular Variation of Precession.....	$= -0\cdot0010$

I have corrected Maskelyne's "Right Ascensions," given in the catalogues contained in the before-mentioned work, by the quantities due to the assumed place of the equinox for each year, as given in the same papers.

As Pond's determination of the position of the equinox for each year was made with great care, I have not applied any correction to his results.

To Airy's "Greenwich Right Ascensions" I have applied the corrections assigned in the Twelve-Year Catalogue, and in the Six-Year Catalogue.

I now give the right ascensions deduced from the observations of each year, reduced to 1845:—

Year.	Mean R.A., 1845, Jan. 1.	Numb. of Obs.	Year.	Mean R.A., 1845, Jan. 1.	Numb. of Obs.
	h m s			h m s	
1765	6 38 19·14	44	1769	6 38 (19·52)	1
1766	19·16	5	1779	(19·06)	1
1767	19·08	9	1780	18·93	9
1768	(18·44)	1	1781	19·18	5

Year.	Mean R.A., 1845, Jan. 1. h m s	Numb. of Obs.	Year.	Mean R.A., 1845, Jan. 1. h m s	Numb. of Obs.
1782	6 38 19·01	15	1836	6 38 19·07	23
1783	18·83	13	1837	19·12	17
1784	18·69	15	1838	19·09	21
1785	19·05	6	1839	19·15	26
1796	(19·51)	7	1840	19·15	30
1797	(19·33)	5	1841	19·12	12
1798	19·33	12	1842	19·24	32
1799	19·21	2	1843	19·29	16
1800	19·34	8	1844	19·22	13
1801	19·22	3	1845	19·22	17
1804	18·85	6	1846	19·16	27
1805	18·97	17	1847	19·12	22
1806	19·12	3	1848	19·16	27
1807	18·95	11	1849	19·11	38
1820	19·08	—	1850	19·11	21
1823	19·06	—	1851	19·10	24
1826	18·96	—	1852	19·09	29
1829	19·10	24	1853	19·05	7
1830	19·04	27	1854	19·00	13
1831	19·18	16	1855	19·04	15
1832	19·15	20	1856	19·03	9
1833	19·07	14	1857	18·96	13
1834	19·07	18	1858	18·89	10
1835	19·09	13			

We next proceed to form the table of values of north polar distances for 1845 in a similar manner. The materials I have used are Bessel's *Fundamenta*, Piazzi's Catalogue, Pond's *Greenwich Observations* from 1813 to 1832, Airy's *Cambridge Observations* for 1833 and 1835, and Airy's *Greenwich Observations*, 1836 to 1859.

The values of constants are the following:—

Peters' Annual Precession in N.P.D. $= +3''\cdot340$
Main's Annual Proper Motion..... $= +1''\cdot24$
Secular Variation of Precession .... $= +0''\cdot385$

The tabulated values of mean N.P.D. for 1845, Jan. 1, are as follows:—

Year.	Mean N.P.D., 1845, Jan. 1. ° ′ ″	Numb. of Obs.	Year.	Mean N.P.D., 1845, Jan. 1. ° ′ ″	Numb. of Obs.
1755	106 30 30·43	—	1815	106 30 26·28	—
1800	28·39	—	1822	30·33	—
[illegible]	26·78	34	1823	30·04	—

Year.	Mean N.P.D., 1845, Jan. 1.	Numb. of Obs.	Year.	Mean N.P.D., 1845, Jan. 1.	Numb. of Obs.
	° ′ ″			° ′ ″	
1826	106 30 29·52	—	1845	106 30 26·34	10
1829	31·12	22	1846	26·47	10
1830	30·69	46	1847	26·47	5
1831	30·59	20	1848	26·36	11
1832	30·78	25	1849	26·49	23
1833	29·82	11	1850	27·28	25
1835	29·00	4	1851	25·86	27
1836	29·98	28	1852	26·39	26
1837	30·46	131	1853	26·58	8
1838	30·16	30	1854	26·67	15
1839	28·96	22	1855	26·62	16
1840	29·07	17	1856	26·85	9
1841	29·40	6	1857	27·76	12
1842	28·49	11	1858	28·25	9
1843	27·30	13	1859	27·68	11
1844	27·07	9			

Pond's north polar distances have been taken, without alteration, from his yearly catalogues; excepting that, as Bradley's refractions were used by him in the reduction of his observations, the correction $+1''\cdot5$ has been applied for reduction to Bessel's refractions, or to make the results comparable with Airy's results. The result opposite the epoch 1755 is derived from the N.P.D. given in the *Fundamenta*, unaltered, and that opposite 1800 from Piazzi's Catalogue.

An inspection of the above table of north polar distances will show, first, that there is, on the whole, an admirable uniformity in the values for successive years, which proves the goodness of the observations on which the results are based; and, secondly, that there is a periodical fluctuation in the values of an interesting and well-marked character. This fluctuation I do not intend to speculate upon in the present paper, except by remarking that we might *à priori* have expected such a result to accompany the acknowledged and well-marked fluctuation of the corresponding values of R.A., which indicates a variability of proper motion.

If, now, we take the means of the six results from 1842 to 1847 inclusive, we shall have for the N.P.D., $106^\circ\ 30'\ 27''\cdot02$; and this is, as it ought to be, precisely the result given in the Greenwich Twelve-Year Catalogue for the year 1845, and which Professor Calandrelli assumes to be in error.

A little consideration of the grounds on which the Professor has arrived at this conclusion will show that he has not used sufficient caution in his assumption, especially as it involves the grave charge of an error amounting to more than 2″ in the

place of a fundamental star in a work of so great authority as the *Greenwich Catalogue*.

In the first of his memoirs, namely, that in the *Nuovi Lincei* for April 3, 1853, p. 316, there is the following passage:—

"Nel recente Catalogo di Greenwich, pubblicato nel 1849, si riportano le distanze polari di molte stelle per le due epoche vicinissime fra loro 1840–1845. Accade spesso che, ridotte le posizioni alla stessa epoca, si trova une differenza di 2″, 3″. Per l' α del Cane Maj. si ha

1840,	Δ	=	106° 30′ 6″·98
1845,	Δ	=	106 30 27·02

Riducendo la prima all' epoca dell' altra si ottiene

1845,	Δ	=	106° 30′ 30″·405
1845,	Δ	=	106 30 27·020

e quindi pel 1° del 1855,

	Δ	=	106° 31′ 17″·254
	Δ	=	106 31 13·870

la prima è prossimamente quella che ho fissato dietro il confronto di altri cataloghi; la prima risulta da 234 osservazioni, l'altra da 58, dunque si conferma ciò che ho detto (87°) sugli errori inevitabili delle osservazioni."

In explanation of the above, it may be stated that the results for 1840 and 1845 are those given in the Greenwich Twelve-Year Catalogues for those epochs; the first being deduced by combinations of all the observations from 1836 to 1841 inclusive, and the second by all the observations from 1842 to 1847 inclusive; and Calandrelli's assertion is, that from mere error of observation the first result differs from the second by 3″·4, though the number of determining observations is 234 for the epoch 1840, and 58 for the epoch 1845. He also arbitrarily assumes, on account chiefly of the difference in the number of determining observations, that the result for 1840 is essentially correct, while that for 1845 is totally in error. Without wishing to be severe in our judgment, we may at least say that the charge of incorrectness is, in the instance before us, made without due examination, and on very insufficient grounds.

An inspection of the separate results for 1845, derived from the observations of the separate years from 1836 to 1841, and from 1842 to 1847, will show that the difference in the results of the two epochs noticed by Calandrelli is *real*, but that it has not its origin in any error either of observation or reduction: it depends upon a real fluctuation, most important and interest-

ing, which will render the motions of this star a serious subject for study during the remainder of the present century.

In the other papers of Calandrelli to which reference has been made there are similar statements with respect to the incorrectness of the N.P.D. of *Sirius* given in the *Greenwich Catalogue*, but without any additional discussion. Thus, in the *Atti dell' Accad. Pontif. de' Nuovi Lincei* for March 1, 1857, he says, referring to the N.P.D. for 1845, which he has assumed: "In questa tavola la distanza polare di *Sirio* al 1° del 1845 non è quella che trovasi nel Cat. di Greenwich pubblicato in Londra nel 1849. Notai già in altro luogo, che in quella distanza polare essere incorso un qualche errore."

Again, in his memoir entitled "Sul Movimento Proprio di *Sirio*," published in 1858, p. 8, he makes a precisely similar statement: "Nel tomo secondo degli Annali dell' imperiale Osservatorio di Parigi, pubblicati da Le Verrier, dopo le correzioni date alle osservazioni di Bradley e di Greenwich si danno due posizioni fondamentali di *Sirio*, una pel 1° del 1755, e l' altra pel 1° del 1845. Nella declinazione o distanza polare di questa ultima è incorso un errore di circa 2″, come ho fatto notare in altri luoghi."

Lastly, in the Appendix to the paper quoted above, published also in 1858, pp. 26, 27, he repeats his statement, thus: "A me pare di aver dimonstrato evidentemente che le distanze polari di *Sirio* date pel 1° Gen. 1845 (Greenwich) e pel 1° Gen. 1850 (Madras) sono in errore di 2″ in 3″ colle altre date per le stesse epoche." And again: "Questo errore nella distanza polare di *Sirio* pel 1° Gen. 1845 fu già da me avvertito al num.° 91 della Memoria pubblicata in Roma nel 1855, dove anche notai che la distanza polare di *Sirio* data nel Catalogo di Greenwich (1849) pel 1° Gen. 1840, risultava da 234 osservazioni, e che quella del 1845, dello stesso Catalogo risultava da 58 osservazioni."

It is sufficient at the present time to have drawn the attention of astronomers to this unfounded assertion of Calandrelli, without going into further details than have been found necessary for its refutation, because any astronomer who might find occasion to discuss the subject of the variability of the proper motion of *Sirius* would have found no greater difficulty in arriving at the truth than I have found myself. The tables which I have given in this paper of the values of R.A. and N.P.D. for Jan. 1, 1845, deduced from the observations of single years, will not only prove the general precision of the Greenwich results, but will, I think, establish the fact of periodicity of proper motion in both elements. In general, though this has been esteemed certain for right ascensions, it has been thought that there is not sufficient evidence of variation of proper motion in N.P.D. Yet an inspection of the preceding tables will show that it is pretty nearly as well marked in one element as the other, and will, I trust, give an

additional motive for the study of the peculiarities of this star. If the subject were treated with the utmost rigour, a very nice scrutiny of Pond's observations, in comparison with the more modern Greenwich observations, would be requisite. Still I believe the remaining corrections, in addition to such as I have pointed out, would be very small, and that the results which I now give are entitled to considerable confidence.

Before concluding this paper, it may be interesting to give in a similar shape the results for the north polar distances of *Procyon*, in which variability of proper motion has been suspected. They are contained in the following table, from the year 1836 to the present time:—

Year of Obs.	Mean N.P.D., 1845, Jan. 1.	Numb. of Obs.	Year of Obs.	Mean N.P.D., 1845, Jan. 1.	Numb. of Obs.
	° ′ ″			° ′ ″	
1836	84 22 57·79	12	1848	84 22 54·99	13
1837	56·87	12	1849	54·85	12
1838	57·14	22	1850	55·59	18
1839	56·63	13	1851	55·78	24
1840	55·30	6	1852	56·08	33
1841	57·11	8	1853	55·81	1
1842	55·49	10	1854	56·40	21
1843	55·94	9	1855	56·71	19
1844	55·79	7	1856	56·52	14
1845	55·94	6	1857	56·60	16
1846	55·03	11	1858	57·22	13
1847	56·33	4			

For the reduction to the year 1845, I have used the following values of precession and proper motion:—

Peters' Annual Precession in N.P.D.	+7″·771
Main's Annual Proper Motion	+1·08
Secular Variation of Proper Motion.	+0·429

In the results there is a sensible trace of periodicity, the minimum values being about the epoch 1848–9; but as the number of observations in each year is not large, it would not be prudent to pronounce a definite opinion. By grouping the observations of every three years, a more distinct notion may be gained of the value of the evidence. Thus:—

Years of Obs.	Mean N.P.D., 1845, Jan. 1.	Numb. of Obs.	Years of Obs.	Mean N.P.D., 1845, Jan. 1.	Numb. of Obs.
	° ′ ″			° ′ ″	
1836–8	84 22 57·24	46	1848–50	84 22 55·20	43
1839–41	56·48	27	1851–3	55·95	58
1842–4	55·73	26	1854–6	56·54	54
1845–7	55·54	21	1857–8	56·88	29

Royal Observatory, Greenwich,
March 8th, 1860.

On a Method of getting rid of Personal Equation, by a Test for Micrometer Readings. By Lieut.-Col. R. Shortrede.

It is well known that different persons vary sensibly in their micrometer readings, the difference between the means of a number of readings being considered as the *personal equation* for the observers.

Many years ago I hit upon a method of reading by which I could (to the extent of the optical power of the micrometer) always myself get the same readings, and, after a few minutes' training, could get other people to read the same as myself. I conceive, therefore, that personal equation of this sort arises not from difference in the eyes of the observers, but from difference in the method of using them in judging of the bisection of the object in view.

In trying carefully to make the cross-wires bisect the divisions of the limb, I noticed that the angular points of the limb visible between the division and the cross-wires formed a quadrilateral; and that when the intersection of the wires was in the middle of the division, the quadrilateral was necessarily a parallelogram, the exactness of which could be instantly tested by comparing the short sides above and below the crossing of the wires.

If the intersection of the cross-wires be not in the middle of the division, the four points will not form a parallelogram.

If the division of the limb bisect the angle of the cross-wires, the four points will form a rectangle.

If the bisection of the wires be oblique to the division on the limb, they will form not a rectangle, but a parallelogram.

To apply this test, it is important that both the divisions and the wires should be clean and sharp, but it is not necessary that they should be very fine; they should be thick enough to be easily seen in the worst light likely to be used.

I may mention that this method of reading was not known at the head-quarters of the Great Trigonometrical Survey in India till 1840, nor at a principal observatory in this country till 1848.

By this method of reading, the intersection may always be brought over the middle of the division, even though the wires be unequally inclined. Unequal inclination of the wires, however, is an evil. In order to avoid it, the usual way of adjusting is to turn the body of the micrometer till the angle of the wires appears to be equally bisected by the divisions. This often leaves a considerable personal error, to avoid which I have found it a good method to bring the micrometer zero between the pair of equal lines in the middle of the quarter-degrees, and then move the micrometer-wires so as to be equidistant from the upper ends of the equal lines, when the lower ends also will be equidistant if the wires be equally inclined, or, in other

words, if the divisions properly bisect the angle of intersection of the wires.

Of late years some observers profess to correct the runs of their micrometers for temperature. This seems to be a needless refinement; for the correction, though real, is in any particular case almost insensible, and vastly smaller in amount than another generally neglected, arising from the wear of the screw. As usually made, micrometer-screws are pressed up by a spring acting constantly on one side, in order to avoid slack: the consequence is, that the part of the screw commonly used gets worn away on one side; and after much use there may be a sudden change in the run, amounting, as I have found, to two or three seconds. I am not aware that this has received the attention it deserves.

February, 1860.

On the Shape of the Moon. By M. Gussew, Assistant at the Observatory of Wilna.

(*Communicated by W. De La Rue, Esq.*)

The author refers to a detailed account of an investigation on this subject, sent by him to the Academy of Sciences at St. Petersburg. He remarks that the sharpness of the lunar photographs obtained by means of the reflecting telescope of Mr. De La Rue is so well known to the Society that no justification is needed for making these lunar pictures the basis of scientific investigation. He mentions that he was led to the investigation by a peculiar egg-shaped appearance of the moon exhibited in the well-known stereographic lunar photographs of Mr. De La Rue. It appeared to him inexplicable how such an effect could be produced by two photographic pictures taken from the same sphere, and moreover entertaining a hope of finding a confirmation of the well-known theoretical results of M. Hansen, he commenced a series of micrometrical measurements, which were well suited to remove all doubts on the subject. The result obtained from the measurements is that the greatest mean deviation from the spherical form is about 0·05 of the radius, and the author concludes by remarking that his observations at least prove that the deviation of the form of the moon from that of a sphere is sufficiently great to be measurable by direct observation.

Morning Illumination of Hippalus (*Lunar Crater*).

By W. R. Birt, Esq.

This crater (225 on Webb's index-map) is a fine specimen of a class but rarely met with on the lunar surface, viz., those partially inclosed by a mountainous chain that had *apparently* at some former period been so perfectly ring-formed as to have entirely inclosed the interior; but now a portion of the ring is gone, and that so completely as not to leave a vestige of it remaining in its immediate locality, which, in the instance before us, is towards the south-east.

The crater *Hippalus*, situated on the north-western border of an arm of the *Mare Humorum*, lies on a ridge of craters that may be traced as far as *Tycho*. The western and northern portions of the border of the crater are more or less complete; they consist of peaks varying very considerably in altitude. The appearance presented at the age and libration of the moon specified below is that of a nearly semicircular range of mountains of low altitude on the south, gradually increasing on the western side, and attaining the culminating point on the northern or rather north-eastern, where an *abrupt* termination of the chain occurs; the length of the shadows *without*, contrasting strongly with those on the opposite side *within* the crater. The western half of the floor, bounded on the east by a nearly straight line terminating at the southern extremity of the semicircular mountains, appears to be of a somewhat *higher level* than the eastern, and is *rugged* from the presence of a number of hillocks and two very minute craters; whilst the eastern part is *smooth*, strongly reminding the observer of the general surfaces of the *maria*. *Only one small hill is seen on this part of the floor, near the abrupt termination of the mountains.* Not the vestige of a shadow is to be seen between the two extremities of the mountains. The light passes unobstructedly from the boundary of the irregular western half over the smooth surface of the eastern, until it reaches the terminator at some distance to the east.

A most interesting question is suggested by the appearance of this crater. Has the ring at any earlier or remote period completely surrounded the interior? The smooth eastern and rugged western halves are remarkable; and it is worth inquiry if the ruggedness of the western has resulted from the *débris* of the destroyed portion of the wall on the east (?), or from a portion of the wall on the west having given way as indicated by Mr. Nasmyth, see *Monthly Notices*, vol. xiv. p. 158.

Epoch 1860, March $2^{d}\cdot44$ G.M.T. Moon's age, $10^{d}\cdot12$,
Libration N.W., $10^{d}\cdot11$, Epoch March $12^{d}\ 13^{h}\ 19^{m}$,
Perigee, $4^{d}\cdot14$, Epoch March $6^{d}\ 14^{h}$.

On a Phenomenon connected with a Transit of Jupiter's Third Satellite. By C. Leeson Prince, Esq.

I am induced to forward a brief report of a phenomenon connected with a transit of *Jupiter's* third satellite, which I had never previously observed.

The atmosphere during the evening of January 31st was remarkably clear and favourable for astronomical observations.

Upon examining *Jupiter's* disk about $10^h\,40^m$, and just before the shadow of the third satellite appeared upon the disk, I could not see the satellite shining upon the belt, but I observed what I at first thought was the shadow of another satellite which had passed the centre of the planet.

Upon reference to the *Nautical Almanac*, however, I found that the shadow of the first satellite had emerged at $8^h\,20^m$.

I then considered this dark object to be a spot upon the belt, and was not undeceived until I found that it travelled too fast, and became much fainter as it approached the limb of the planet.

About $11^h\,50^m$ I left the telescope, to allow a friend to look at the planet; and upon my return, at $11^h\,56^m$, I found to my surprise that during this short interval the spot had entirely disappeared, and that the satellite itself was shining *brightly* in its place.

I was not aware that the third satellite ever looked opaque during its transit.

When first observed, it appeared like the shadow of the first satellite when there is much tremor in the air.

The planet *Saturn* was also a fine object the same night.

From frequent observations during the last two years, I am convinced that the nebulous ring is becoming more and more illuminated.

The Observatory, Uckfield, February 20th.

From the particulars communicated in letters from Captain Boyd, R.N., commanding H.M.S. Ajax, and from a conversation with Lieut. Grant, R.N., of the same ship, the Astronomer Royal has no doubt that some of *Jupiter's* satellites were seen by those gentlemen and by other officers of the ship, in Kingstown Harbour, Dublin, on the evening of Sunday, Jan. 15, with the naked eye.

The evening was at first hazy, but became afterwards unusually clear.

The satellites and *Jupiter* were in the order

3, 2, 1, *Jupiter*, 4

(measuring from left to right).

The first satellite was very near to the body of *Jupiter*.

Capt. Boyd, and perhaps others of the officers, saw 3 and 2 with the naked eye; Lieut. Grant saw 3 only. It does not appear that any of the party saw 4 or 1. It will be borne in mind that 3 is much brighter than the others.

The satellites were identified by comparing their appearance with that of the complete system as viewed in the 2-feet navy telescopes.

Royal Observatory, Greenwich,
February 4th, 1860.

In a letter to the Assistant-Secretary, Mr. Pollock, after referring to his letter giving an account of the apparent projection of *Saturn* on the moon's disk, as seen by the naked eye on the occasion of the occultation of the 8th May last, and to the explanation by the Astronomer Royal of the observed phenomenon (see *Monthly Notices*, vol. xix. pp. 323 and 340), writes:—

"Had the moon been past the full at the time of the occultation, so as to make the planet disappear behind the *bright* limb, there might be no difficulty; but in this case the disappearance of *Saturn* took place behind the *dark* limb of the moon. And having regard to the comparative brightness of the planet and the dark limb of the moon, it is not easy to see how there could be such an enlargement of the diameter of the latter as to make the planet appear *entirely* on the moon's disk."

Results of the Observations of Small Planets made with the Transit-Circle at the Royal Observatory, Greenwich, during the months of January and February, and part of March, 1860.

(*Communicated by the Astronomer Royal.*)

Hebe ⑥.

Mean Solar Time of Observation.			Apparent R.A.	Apparent N.P.D.
		h m s	h m s	° ′ ″
1860, Jan.	2	7 7 2·7	1 49 41·73	98 48 55·46
	4	6 57 16·8	1 51 43·90	98 19 47·76
	6	6 51 31·5	1 53 50·72	97 50 30·48
	7	6 48 40·3	1 54 55·69	97 35 49·63

Iris (7).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Jan.	17	14	4	11·2	9	51	3·73	84	50	58·43
	20	13	49	57·9	9	48	37·67	84	47	55·51
	26	13	20	56·1	9	43	10·49	84	36	51·88
	31	12	56	18·9	9	38	12·00	84	23	6·15
Feb.	6	12	26	29·4	9	31	56·93	84	2	6·87
	9	12	11	32·9	9	28	47·65	83	50	9·12
	10	12	6	34·5	9	27	44·96	83	45	59·29
	13	11	51	40·8	9	24	38·49	83	33	3·14
	23	11	2	46·5	9	15	1·74	82	47	14·29
	27	10	43	43·0	9	11	41·35	82	28	49·87
	29	10	34	19·9	9	10	9·76	82	19	47·03
Mar.	2	10	25	2·6	9	8	44·07	82	10	54·62
	5	10	11	18·7	9	6	47·55	81	57	10·35

Hygeia (10).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Feb.	28	12	49	54·6	11	22	10·18	91	18	37·33
	29	12	45	15·2	11	21	26·65	91	15	3·63
Mar.	2	12	35	54·4	11	19	57·38	91	7	40·88
	5	12	21	51·1	11	17	41·44	90	55	58·40

Parthenope (11).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Jan.	7	8	56	43·4	4	3	19·75	.....		
	12	8	35	53·9	4	2	9·67	74	5	17·87
	17	8	15	48·1	4	1	43·34	73	52	52·78
	21	8	0	15·4	4	1	54·34	73	41	31·54

Egeria (13).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Jan.	16	14	2	18·2	9	45	13·77	47	35	41·60
	17	13	57	31·3	9	44	22·65	47	26	43·69
	20	13	42	59·6	9	41	38·31	47	1	6·36
Feb.	6	12	16	54·0	9	22	20·02	45	24	6·34
	27	10	31	38·3	8	59	34·65	45	48	14·59
	28	10	26	54·0	8	58	46·13	45	53	13·77
	29	10	22	12·2	8	58	0·13	45	58	31·25
Mar.	2	10	12	54·4	8	56	33·88	46	9	56·83
	5	9	59	14·0	8	54	40·94	46	29	6·61

Urania (30).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Feb.	6	14	40	1·2	11	45	50·73	90	24	9·67
	28	12	57	36·4	11	29	53·26	89	5	37·58
Mar.	2	12	43	2·4	11	27	6·53	88	50	23·88
	5	12	28	24·6	11	24	15·99	88	34	34·84

Pomona (32).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Jan.	16	12	55	26·4	8	38	11·00	80	39	44·46
	17	12	50	36·7	8	37	17·06	80	37	35·51
	20	12	36	5·0	8	34	32·67	80	30	39·75
Feb.	9	10	58	58·7	8	16	1·59	79	22	23·36
	10	10	54	13·2	8	15	11·84	79	18	23·13
	27	9	35	33·3	8	3	20·47	78	9	48·47
	29	9	27	55·3	8	3	34·25	78	2	12·97

Leda (38).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Jan.	4	7	22	34·0	2	17	5·21	68	10	50·87

Lætitia (39).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Jan.	16	11	56	10·7	7	38	45·60	79	15	36·49
	20	11	36	55·2	7	35	13·11	78	55	16·25
	24	11	17	45·9	7	31	46·90	78	33	56·75
Feb.	29	8	38	27·2	7	13	58·05	75	19	53·78

Ariadne (43).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Feb.	27	12	20	32·2	10	48	46·43	88	47	29·48
	29	12	10	36·0	10	46	41·69	88	36	30·44
Mar.	2	12	0	39·1	10	44	36·32	88	25	2·82
	5	11	45	43·6	10	41	28·03	88	7	25·45

Nysa (44).

Mean Solar Time of Observation.				Apparent R.A.			Apparent N.P.D.		
		h	m	s	h	m	s	°	′	″
1860, Feb.	23	9	0	32·1	7	12	27·29	67	57	46·73
Mar.	2	8	31	16·8	7	14	39·57	67	42	31·21

Eugenia (45).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, Jan. 12	9 58 8·7	5 24 38·00	74 46 2·60

Doris (48).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, Feb. 13	13 21 12·7	10 54 22·11	87 41 4·50
29	12 7 4·2	10 43 9·33	86 8 35·74
Mar. 2	11 57 45·5	10 41 42·25	85 56 5·62

Pandora (55).

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, Jan. 20	13 5 47·5	9 4 20·02	63 2 28·39

All the observations of N.P.D. have been corrected for parallax.

Observed Places of the Small Planets Hygeia and Urania.

By M. Lépissier.

(*Communicated by the Astronomer Royal.*)

Hygeia (10).

1860.	Paris M.T. h m s	App. R.A. h m s		App. N.P.D. ° ′ ″		Ref.
Feb. 29	12 29 24·1	11 21 26·88	(8·419−)+Δ	91 15 0·3	(0·817−)+Δ	*a*
29	13 57 36·6	11 21 24·40	(9·070+)+Δ	91 14 57·0	(0·816−)+Δ	*a*
Mar. 1	8 44 26·9	11 20 49·46	(9·511−)+Δ	91 11 57·2	(0·812−)+Δ	*b*
1	12 21 27·1	11 20 42·98	(8·500−)+Δ	91 11 31·1	(0·816−)+Δ	*a*
2	11 3 46·3	11 20 0·48	(9·171−)+Δ	91 7 55·5	(0·815−)+Δ	*a*
3	10 18 11·8	11 19 16·85	(9·318−)+Δ	91 4 12·6	(0·814−)+Δ	*c*

Observer, Y. Villarceau, on March 1, second observation; on other occasions, Lépissier.

The observations are corrected for refraction.

Mean Adopted Positions of the Stars of Comparison, 1860·0.

			R.A. = h m s	N.P.D. = ° ′ ″
a = 353 Weisse	9th	Mag.	11 20 49·63	91 9 3·5
b = 312 Weisse	9th	,,	11 18 42·53	91 21 20·6
c = 3901 B.A.C.	6th	,,	11 20 44·63	90 55 48·3

Urania ⑳.

	Paris M.T.	App. R.A.	App. N.P.D.	No. of Comp.	Ref.
	h m s	h m s	° ′ ″		
8	11 34 36·3	11 21 24·98 (8·652 −)+Δ	88 18 15·7 −(0·797)+Δ	3 & 3	*a*
	12 51 52·6	11 21 21·73 (8·797 +)+Δ	88 17 59·9 −(0·797)+Δ	4 & 3	*a*

The observations are corrected for refraction.

Mean Adopted Position of the Star of Comparison, 1860·0.

a = 21805 Lalande 7th Mag. R.A. = 11 h 20 m 23·20 s N.P.D. = 88 ° 15 ′ 58·0 ″

The planet was a very faint object both from its smallness and the moonshine.

Occultations of Stars by the Moon, observed at Highbury.

(*Communicated by T. W. Burr, Esq.*)

Date.	Star.	Immersion. Dark Limb.	Emersion. Bright Limb.
		h m s	h m s
1860, Feb. 28	19 Tauri	7 17 2·5	8 23 35·5
	16 Tauri	7 19 5·	7 53 35·
	20 Tauri	7 42 30·5	not observed.
Mar. 4	ϑ Cancri	6 6 11·8	ditto

The disappearances of 19 and 20 *Tauri* and ϑ *Cancri* were instantaneous. That of 16 *Tauri* rather sluggish. The reappearances are probably rather late, owing to the brightness of the moon's limb. The time recorded is local sidereal time. Telescope, 4 feet 4 inches focal length; $3\frac{1}{8}$ inches aperture. Power, 128; approximate longitude, 23ˢ·8 W.

Occultations of Stars by the Moon, observed at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar T.	Observer.
			h m s	
1860, Jan. 4	17 Tauri disap.	Dark	16 6 9·9	T
	19 Tauri disap.	Dark	16 13 6·8	T
	20 Tauri disap.	Dark	16 21 22·9	T

The initial T is that of Mr. Talmage.

Occultation of the Pleiades, 1860, *Feb.* 28.

Phenomenon.	Moon's Limb.	Greenwich Mean Solar Time of Observation. Hour and Minute.	Seconds observed by M.	E.	J. C.
19 Tauri disapp.	Dark	8 46	5·2	...	5·0
16 Tauri disapp.	Dark	8 48	18·7	18·9	19·2
21 Tauri disapp.	Dark	9 7	35·4	...	...
Bessel (8) disapp.	Dark	9 8	59·2	...	...
22 Tauri disapp.	Dark	9 11	29·1	...	...
20 Tauri disapp.	Dark	9 11	31·1	31·3	31·3
16 Tauri reapp.	Bright	9 21	9·5	21·4	13·7
19 Tauri reapp.	Bright	9 51	54·4	46·9	49·6
20 Tauri reapp.	Bright	10 0	29·0	27·5	27·7

Notes by Mr. Main [M.]. Observed with the Sheepshanks' equatoreal. The dark limb was distinctly visible; the disappearances were instantaneous. The times of reappearance of 16 *Tauri* and 19 *Tauri* were estimated, the stars being first seen at a little distance from the limb.

From the result given for the reappearance of 20 *Tauri*, as compared with the observations by E. and J. C., I suspect that the counting of the clock was 1 second in error: the wind was so high, that the beats could with great difficulty be heard.

Notes by Mr. Ellis [E.]. The reappearances of 16 *Tauri* and 19 *Tauri* were considered several seconds late, the star being in both cases a little separated from the limb when first seen. The reappearance of 20 *Tauri* considered good.

Notes by Mr. Carpenter [J. C.]. The disappearance of 20 *Tauri* uncertain; 16 *Tauri* and 19 *Tauri* were separated from the limb at the time noted for their reappearance. The reappearance of 20 *Tauri* considered good.

Star Occultations and Lunar Eclipse of Feb. 6, 1860, *observed at the Hartwell Observatory.* By Norman Pogson, Esq. Director of the Observatory.

(*Communicated by Dr. Lee.*)

		G.M.T. h m s
Disappearance of σ^2 Cancri	Feb. 6	8 41 45·7
,, 18 Leonis	March 5	9 12 28·2
,, 19 Leonis	,,	9 52 45·7
,, R Leonis (3345 B.A.C.)	,,	10 6 10·8

Lunar Eclipse of Feb. 6.

Shadow advancing.	G.M.T. h m s	Shadow retreating.	G.M.T. h m s
Grimaldi entered	13 7 1	Grimaldi entered	14 51 34
Aristarchus bisected	13 9 55	Grimaldi cleared	14 55 17
Grimaldi covered	13 14 17	Kepler bisected	15 9 37
Bessarian bisected	13 15 6	Bessarian bisected	15 13 14
Kepler ,,	13 17 5	Aristarchus bisected	15 14 14
Plato entered	13 20 50	Copernicus entered	15 16 27
,, covered	13 22 38	,, cleared	15 18 3
Copernicus entered	13 23 37	Pytheas bisected	15 22 15
,, covered	13 25 48	Manilius ,,	15 31 28
Manilius bisected	13 38 15	Menelaus ,,	15 34 38
Posidonius entered	13 40 16	Plato entered	15 37 6
Menelaus bisected	13 41 37	,, cleared	15 38 28
Posidonius covered	13 42 11	Proclus bisected	15 44 57
Proclus bisected	13 55 6	Posidonius ,,	15 45 59
Picard ,,	13 58 39	Picard ,,	15 47 44

Remarks.

Owing to the moon being at the full on the evening of February 6, the disappearance of π^2 *Cancri* was far less satisfactory than the other cases, in each of which the star vanished instantaneously. No projection or any other unusual phenomenon was seen, but the reappearances were too uncertain to be worth recording. The equatoreal was charged with a power of 118 for these observations, and the time carefully determined immediately afterwards by means of the 5-foot transit instrument.

The lunar eclipse was watched throughout with the equatoreal and power 84. The loss of light in the north-east quadrant was very decided about $12^h\,33^m$. The deep shade of the penumbra caused the actual shadow to appear so indefinite that neither its first nor last contacts could be observed. The only remarkable feature in the eclipse was the visibility — it might almost be termed the brilliancy — of *Aristarchus*. *Kepler* and other spots were comparatively lost, or at most barely discernible as soon as they became enveloped in the shadow, but not so *Aristarchus*, which evidently shone either by intrinsic or retained illumination. At the time of greatest obscuration the shadow extended southward within about two minutes of *Tycho*.

ERRATA.

P. 99, line 9 from the bottom, the line of egress of the planet from the sun's disk is stated to be $5^h\,47^{m\cdot}2$ Orgères local time; it should be $5^h\,17^{m\cdot}2$ Orgères local time.

P. 101, line 7, *for* Scheuter, *read* Scheuten.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *April* 13, 1860. No. 6.

The Rev. R. MAIN, President, in the Chair.

Robert Bellamy Clifton, Esq., St. John's College, Cambridge;
Horace Deighton, Esq., Principal of the Collegiate School, Trinidad;
Benjamin Scott, Esq., Chamberlain of London; and
Lieut. W. L. Searle, H.M. Indian Navy,

were balloted for and duly elected Fellows of the Society.

In a letter to the Foreign Secretary, dated Gotha, 1860, March 7th (written in German), M. Hansen, after expressing his warmest thanks to the Royal Astronomical Society for the Medal awarded to him, and requesting to be furnished with some copies of the President's address, continues as follows:

"The Lunar Tables are a work which has occupied me for several years, partly on account of the great number of the perturbation inequalities, which can only be accurately obtained by successive approximations, and partly because the comparisons with observations which are necessary for the amendment of the elliptic elements are very laborious and take much time. But as I had proposed to myself to work out as well the theory as the Lunar Tables themselves, as completely and accurately as was possible for me, it was necessary, besides the assistance which I was able to dispose of for the mechanical calculations, to occupy myself almost continuously therewith, and even myself to work out a part of the numerical calculations.

"It would have been impossible to accomplish this great

work without pecuniary assistance, and I was fortunate enough at the commencement of the undertaking to obtain from the Danish Government an allowance, which, however, ceased long before the completion of the work. A sum for the same purpose was afterwards allowed me by the British Government, and by this means, and the application of what I could myself contribute, it is become possible for me to complete the Lunar Tables. I shall be always grateful to the above-mentioned governments for the assistance afforded me to the completion of the Lunar Tables, but I have not been able to finish them without the expenditure from my own resources of what is for me a considerable sum. It is therefore an especial pleasure for me to see that my Lunar Tables exhibit a very good agreement with observation, and that I may thus consider that I have [illegible] Astronomy and Navigation [illegible] the theoretical [illegible] Determinations of the Moon [illegible] ready for the press."

thirteen recorded eclipses, the computed times of twelve are smaller than the observed times, or that, on the average, the eclipses happened later than the times given by the tables by about 26^m. The difference is rather greater by Damoiseau's Tables, but not considerably so; and we may fairly assume that, if Dr. Hartwig is correct in his deductions, the coefficient of the lunar acceleration employed by Hansen, namely, $12''{\cdot}12$, is too large by about $2''$.

In connexion with this subject of the lunar acceleration, I am desirous to say a few words with reference to the controversy which has within the last few weeks sprung up between MM. Le Verrier and Delaunay; but only with the view of preventing further misunderstanding with regard to my opinion as expressed in what I have occasionally written on that subject. In the Paris *Cosmos* for the 16th March last, and in that for the 23d March, the editor (the Abbé Moigno), who joins with M. Le Verrier in disputing the accuracy of M. Delaunay's results, makes reference to my opinion, or rather to the reserve with which I have avoided the expression of a definite opinion, on this difficult subject, in a way which, I think, does not fairly represent my meaning; and I feel that it would be unjust towards M. Delaunay not to set myself right in this matter. In the first instance the reference is made to my paper "On the Present State of the Controversy respecting the amount of the Acceleration of the Moon's Mean Motion," printed in the *Monthly Notice* for June 10, 1859 (vol. xix. No. 8). In that paper I endeavoured to sum up as impartially as was possible the difficulties which are met with in accepting the conclusions of either of the contending parties; and I explained that the results only could guide us in our judgment, partly because the theories of Delaunay and Hansen are not yet published, and partly because, if they were before us, the enormous labour which would be necessary in verifying the whole of the complicated analysis, would be probably a complete prohibition of the attempt to do so, even assuming that an analyst skilful enough to attempt such a task were to be found. But in that paper I have distinctly signified the high opinion which I entertain of the labours of M. Delaunay and my expectation that his resulting value of the lunar acceleration, or of that part of it which depends on the diminution of excentricity of the earth's orbit, will ultimately be found to be correct. We have in fact evidence so strong for the truth of this result that nothing but the fact of the discrepancy between it and the results of other eminent analysts, who deduce a value which better satisfies ancient observations, could induce any hesitation in the acceptance of it. The argument stands thus. Mr. Adams, in a paper published in 1853, points out an error which he thinks has been committed by Plana, in the deduction of the value of the acceleration by La Place's method, consisting of the omission of the effect of the variation of the areal velocity.

He shows that this variation of areal velocity will produce a secular effect, and will modify the value of the acceleration obtained by La Place without consideration of it. The geometrical conception of the nature of the error committed is exceedingly simple, and I think that most persons competent to judge will on the *primâ facie* evidence agree with him, as to the necessity of accepting the correction which he proposes. Ultimately he carries his approximations to a very high order, and finds that another mathematician pursuing a process totally different not only from his own but from that of all other analysts, a process of the utmost generality, and which does not need at the commencement the special consideration of the variation of areal velocity,* arrives at precisely the same result as himself, and produces a numerical identity of which perhaps there is no similar example in a mass of computations of such complexity and magnitude. Now putting together these circumstances, 1st, the error in La Place's method, clearly enunciated by Mr. Adams, and admitting, as far as I can see, of no reply; 2d, the identity of the result for the acceleration corrected for this error with that deduced by another mathematician by a totally different process, and afterwards tested and verified by Poisson's method of the variation of elements, I contend that we have very great reason to accept the conclusion of these astronomers, and to believe, in spite of the difficulty which arises from the circumstance of Plana's value satisfying better the ancient observations of eclipses, that they have given the correct value of the acceleration depending on the diminution of excentricity of the earth's orbit. Still it is not to be denied that the difficulties that lie in the way of our acceptance of the result are not to be easily passed over, and that it is only as a *dernier ressort* that the necessity of admitting the action of a cause distinct from gravitation should be admitted. I do however protest against the unfavourable judgment which, before the publication of M. Delaunay's great work, it has been attempted to pass upon it on such a ground as this; and, whatever opinion we may entertain of the merits of the dispute between Plana, Hansen, and Pontécoulant on the one side, and Adams and Delaunay on the other, I consider that this prejudgment is in the highest degree to be deprecated as unphilosophical and injurious to science. At the best it is a small charge to make against an unpublished work, that, in a dispute on such a question as the lunar acceleration, the method employed leads to a result which agrees with that of one of the most accomplished of living analysts, and disagrees with the results of others, while at the same time the dispute is quite unnecessary,

* Mr. Adams also has explained that his method is perfectly general, and that the considerations respecting the variability of the areal velocity are introduced by him to show clearly, in a geometrical shape, the nature of the error which had been committed.

because we have every ground for hope that the theories both of Hansen and Delaunay will be soon given to the world.

An inference is also attempted to be drawn, unfavourable to Delaunay, from the silence respecting his labours which is observable in my address on presenting the Medal to Professor Hansen. With regard to this point I can only say that no such conclusion ought to have been arrived at; first, because I explained particularly that my object was not to discuss in any detailed way the various lunar theories which have been published, and, secondly, because it concerned me still less to give a prominent place to a disputed point of a speculative rather than a practical character. I have alluded with every possible respect to the labours of M. Delaunay, and I have alluded even to this subject of the lunar acceleration as one of those points which remain in the lunar theory still open for future investigators, but which can be profitably meddled with by none but the *most* skilful analysts. The actual value of the acceleration used does not affect the practical value of Hansen's Tables, so far as the interests of navigation are concerned; but it is, at least, a fortunate circumstance, even if we should be ultimately compelled to accept the value given by Delaunay, that the one employed in the tables as that deduced from the theory of gravitation satisfies ancient as well as modern observations, and therefore on practical grounds gives no cause for exception or dispute. I repeat, again, that nothing which I have said in praise of those tables was intended to detract in the slightest degree from the reputation of M. Delaunay; that the general silence which I maintained with regard to the disputed subject of the lunar acceleration arose from the necessity of reserve in speaking of an unpublished work; and that, finally, I look forward to the publication of his theory and that of M. Hansen with feelings of the most intense interest.

Reply to various Objections which have been brought against his Theory of the Secular Acceleration of the Moon's Mean Motion. By J. C. Adams, M.A., Lowndean Professor of Astronomy.

If I have hitherto published no reply to the "Observations" of M. Pontécoulant, contained in the *Monthly Notices* of July last, it is not because the task presented any difficulty, for the fallacies which pervade M. Pontécoulant's communication were perfectly evident to me from the very first. I thought that any competent person who chose to look into my memoir "On the Secular Acceleration," and into these observations upon it, might be safely left to form his own judgment on the matter. Again, I had some hopes that M. Pontécoulant might be led to see and acknowledge the errors into which he had fallen, and

with that object in view I sent to him, on more than one occasion, through a common friend, communications which appeared to me amply sufficient to expose the fallacies contained not only in his printed "Observations," but also in several private letters which he subsequently wrote upon the subject. I find, however, that M. Pontécoulant, in a letter which he has lately caused to be circulated among the members of the French Institute, has ventured to ignore these communications of mine altogether, and to speak as if his observations had been admitted without dispute. Under these circumstances, as my further silence might be misconstrued, I beg leave to offer to the Society the following remarks.

In order to give a more complete view of the subject, however, and to obviate the necessity of my returning to it in a controversial manner, I shall not confine myself to the observations of M. Pontécoulant, but shall likewise say a few words in reply to the objections of M. Plana and those of M. Hansen. I shall also take the opportunity of making some preliminary remarks which may tend to remove certain misapprehensions, which I have reason to believe exist in some minds with respect to the real nature of the matter in dispute.

First, then, I would call attention to the fact that the question is a purely mathematical one, with the decision of which observation has nothing whatever to do. It may be simply stated thus: if the excentricity of the earth's orbit be supposed to change at a given uniform rate and very slowly, what will be the corresponding rate of change, according to the theory of gravitation, in the mean motion of the moon? Now the solution of this question is effected by means of a purely algebraical process, the validity of each step of which admits of being placed beyond all possible doubt.

What conclusion must be drawn, then, supposing that ancient observations should show that the secular variation of the moon's mean motion is different from that which, according to theory, is due to the known change of the excentricity of the earth's orbit?

Why, simply this; that the mean motion of the moon is affected by some other cause or causes, besides the variation of excentricity which has been taken into account. This fact, if established, would be a most interesting one, and might put us on the traces of an important physical discovery. It is not difficult to imagine the existence of causes which may affect the mean motion of the moon, but whether this be so or not, any question respecting the validity of a mathematical process must be decided on mathematical grounds alone, quite independently of the agreement or disagreement of theory and observation.

In the case before us the mathematical question as stated above may be greatly simplified, without its ceasing to involve the point which is in dispute. The values of the secular acceleration given by M. Plana's theory and mine, differ in terms

which are independent of the eccentricity and inclination of the moon's orbit; consequently in deciding which of the theories is right, we may suppose the eccentricity and inclination to vanish.

In the next place I would remark that the error which I attribute to M. Plana's theory on this point is not one of calculation which might require long and complicated numerical processes to be gone through for its correction, but that it is an error of *principle*, about which a mathematician ought not to have much difficulty in making up his mind. I am therefore inclined entirely to agree with M. Pontécoulant's opinion, that the prolonged discussion of this subject would not be creditable to science, and indeed, considering the importance of the question, and the length of time which has passed since the publication of my memoir, I cannot but think it strange that any controversy respecting it should still exist at all. Some persons appear to be under the impression that the contest lies between two values of the secular acceleration, that M. Delaunay and I agree in one value, and that MM. Plana, Pontécoulant, and Hansen, agree in a larger value; but this is by no means the true state of the case. Between M. Delaunay's results and my own, indeed, there is a perfect agreement. He has carried the approximation much further than I have done, but all of the terms which I have calculated have been confirmed by him. Again, before publishing my memoir in 1853, I had obtained my result by two different methods, and I have since confirmed and extended it by means of a third. M. Delaunay arrived at his result by an independent method of his own, and he has lately found exactly the same result by following the method given by Poisson. On the other hand, among our opponents there is far from being the same satisfactory agreement.

In his theory of the moon, M. Plana obtained one value of the secular acceleration. In 1856 he printed a paper in which he admitted that his theory was wrong on this point, and actually deduced my result from his own equations. Soon afterwards, however, M. Plana retracted his admission of the correctness of my result, and obtained a third result, differing both from his former one and from my own.

Again, M. Pontécoulant, in the last communication which I received from him, gives two different values of the secular acceleration, one of which he has obtained by using the time, and the other by using the moon's longitude as the independent variable. Strange to say, however, he does not appear at all startled at obtaining two contradictory values, but seems fully inclined to defend *both*. Indeed, judging from the last paragraph of his letter in the *Monthly Notices*, he appears to have expected that the results of the two methods would differ from each other. One of the values which M. Pontécoulant thus obtains agrees with that given in M. Plana's theory, as of course it must do, being found by means of the same principles.

But he seems to be quite unaware that this value has been abandoned by M. Plana himself in his last paper above referred to, which is contained in the eighteenth volume of the Turin *Memoirs*.

M. Hansen's value of the secular acceleration is not given in an analytical form, like those of MM. Plana and Pontécoulant, and therefore we can only compare the final numerical results. This comparison, which I shall presently give, shows that M. Hansen's value of the acceleration considerably exceeds either of those found by M. Plana.

Here then we find nothing to inspire confidence; certainly nothing like the cumulative testimony which there is in support of M. Delaunay's result and mine.

I may now be permitted to make some remarks on another point. In the introduction to my memoir of 1853, I gave some general reasoning to show that a change in the excentricity of the earth's orbit had a tendency to produce a change in the mean areal velocity of the moon, and that M. Plana was therefore wrong in assuming this velocity to be constant, as in his theory he does. Now this seems to have led some persons to imagine that my analysis in the following part of the memoir depended in some way or other on the validity of the general reasoning which had gone before, and therefore that my conclusions could not be regarded as established with mathematical strictness. But this is quite a mistaken view of the case. I make no assumption respecting the variability of the mean areal velocity. I prove mathematically that this velocity does vary by finding the amount of its variation, and the general reasoning given in the introduction is simply the translation, so to speak, of my analysis into ordinary language, in order to make the nature of my correction to M. Plana's theory, more generally intelligible. It may be remarked too that even if I had started with the assumption that the mean areal velocity was variable, no error could have been caused thereby, for if this velocity had been really constant I should have found its variation equal to zero. In mathematics the terms "constant" and "variable" are not looked upon as *opposed* to each other, but a constant is regarded as a particular case of a variable quantity.

It may be as well to guard against the idea that the extreme minuteness of the quantities which we have to deal with in this investigation, gives rise to any uncertainty in the result. The present rate of approach of the moon to the earth which accompanies the acceleration of its motion, is less than one inch per annum, but the theory can determine this minute quantity to within, say, a thousandth part of its true amount, just as easily and certainly as if the quantity to be found had been any number of times greater.

I will now proceed briefly to explain the principles which

I employ in determining the secular acceleration, and to point out the errors which vitiate the several results of MM. Plana and Pontécoulant which have been already referred to.

The principle of my method is simply this, viz., that the differential equations must be satisfied, and that quantities which really vary must be treated as variable in all the differentiations and integrations which occur throughout the investigation.

Now if e' be variable, the differentiation or integration of any term which involves e' in its coefficient will produce, in addition to the term which would result if e' were constant, another term involving $\frac{de'}{dt}$ in its coefficient, supposing t to be the independent variable.

In consequence of the existence of these supplementary terms, the ordinary expressions for the moon's coordinates when substituted in the differential equations will not satisfy them, but will leave terms multiplied by $\frac{de'}{dt}$ outstanding. In order to destroy these terms, it is necessary to add terms of the same form to the usual expressions for the moon's coordinates. The values of these new terms may, if we please, be easily found by the method of indeterminate coefficients, each of the coefficients being obtained by means of a simple equation.

If n, the moon's mean motion, be variable, the double differentiation of the moon's coordinates will produce in the differential equations, terms involving $\frac{dn}{dt}$ of the same form as those already mentioned which involve $\frac{de'}{dt}$.

Thus the same system of simultaneous simple equations that gives the values of the indeterminate coefficients, determines likewise the value of $\frac{dn}{dt}$, which is what we want to find. If the moon's longitude v be taken as the independent variable, we must proceed according to the same principles, but there is one additional circumstance to be attended to. The differentiations and integrations will now introduce terms multiplied by $\frac{de'}{dv}$ instead of by $\frac{de'}{dt}$ as before.

Now the moon's disturbed motion in longitude is not uniform, and therefore, as e' is supposed to change uniformly, the value of $\frac{de'}{dv}$ cannot be considered constant. We must therefore substitute for it, its value $\frac{de'}{dt} \cdot \frac{dt}{dv}$, in which $\frac{dt}{dv}$ is a given function of v, and the remainder of the process will be exactly similar to that before described.

Let us now consider the method followed in M. Plana's theory, and also by M. Pontécoulant.

In this method the terms above described involving $\frac{de'}{dt}$ are *ignored*, and consequently the differential equations as developed by these astronomers furnish no materials whatever for determining the value of $\frac{dn}{dt}$. Hence they are forced to supply the lack of data by means of an ***assumption***, which is that one of the so-called constants introduced by integration is absolutely constant.

The value of this constant can be expressed in terms of n, e' and known quantities. If then this so-called constant were really so, we should be able by differentiating this relation to obtain $\frac{dn}{dt}$ in terms of $\frac{de'}{dt}$. But if on the other hand this supposed constant be really variable, we must take its variation into account, in order to obtain the true value of $\frac{dn}{dt}$ in terms of $\frac{de'}{dt}$.

In M. Plana's theory, in which ν is taken as the independent variable, the constant so employed is h^2, which is added to complete the integral $2\int r^2 \frac{dR}{d\nu} d\nu$, in the equation

$$r^4 \left(\frac{d\nu}{dt}\right)^2 = h^2 + 2\int r^2 \frac{dR}{d\nu} d\nu,$$

in which $2\int r^2 \frac{dR}{d\nu} d\nu$ is supposed to consist of a series of cosines of multiples of ν.

The quantity $r^2 \frac{d\nu}{dt}$ is equal to twice the area described in a unit of time, or to twice the areal velocity, so that h^2 is the non-periodic part of the square of twice the areal velocity, the periodic part being supposed developed in cosines of multiples of ν.

In M. Pontécoulant's theory, the constant h is introduced to complete the integral $\int \frac{dR}{d\nu} dt$ in the equation

$$r^2 \frac{d\nu}{dt} = h + \int \frac{dR}{d\nu} dt,$$

in which $\int \frac{dR}{d\nu} dt$ is supposed to consist of a series of cosines of multiples of t.

M. Pontécoulant's h is not identical with M. Plana's h, but there is a simple relation between these quantities.

M. Pontécoulant, however, does not employ the constant h in finding the value of the secular acceleration, but another

constant $\frac{1}{a}$, which is introduced to complete the integral in the equation

$$\frac{1}{2}\frac{d^2(r^2)}{dt^2} - \frac{1}{r} + \frac{1}{a} = 2\int d'\mathrm{R} + r\frac{d\mathrm{R}}{dr},$$

all the periodic terms of which are supposed to consist of cosines of multiples of t.

If we neglect the excentricity and inclination of the moon's orbit, and also omit all powers of m above the fourth, the relations between these several constants and the mean motion n will be expressed as follows:

$$\mathrm{h} = n^{-\frac{1}{3}}\left\{1 - \frac{1}{3}m^2 + \frac{719}{576}m^4 + e'^2\left[-\frac{1}{2}m^2 + \frac{2635}{384}m^4\right]\right\},$$

$$h = n^{-\frac{1}{3}}\left\{1 - \frac{1}{3}m^2 + \frac{11}{144}m^4 + e'^2\left[-\frac{1}{2}m^2 - \frac{185}{96}m^4\right]\right\},$$

$$\frac{1}{a} = n^{\frac{2}{3}}\left\{1 + \frac{2}{3}m^2 - \frac{1253}{288}m^4 + e'^2\left[\quad m^2 - \frac{5593}{192}m^4\right]\right\},$$

the sum of the masses of the earth and moon being supposed to be unity.

From these relations we find by differentiation

$$\frac{dn}{n\,dt} = -3\frac{d\mathrm{h}}{\mathrm{h}\,dt} + \frac{d(e'^2)}{dt}\left\{-\frac{3}{2}m^2 + \frac{2187}{128}m^4\right\},$$

$$\frac{dn}{n\,dt} = -3\frac{dh}{h\,dt} + \frac{d(e'^2)}{dt}\left\{-\frac{3}{2}m^2 - \frac{297}{32}m^4\right\},$$

$$\frac{dn}{n\,dt} = -\frac{3}{2}\frac{da}{a\,dt} + \frac{d(e'^2)}{dt}\left\{-\frac{3}{2}m^2 + \frac{5337}{128}m^4\right\},$$

having taken care to observe that, since $m = \frac{n'}{n}$ and n' is constant, we have $\frac{dm}{m\,dt} = -\frac{dn}{n\,dt}$.

If $\frac{d\mathrm{h}}{\mathrm{h}\,dt}$ be neglected in the first of these expressions, we obtain the value of $\frac{dn}{n\,dt}$ found in M. Plana's theory, and one of those found by M. Pontécoulant. If $\frac{dh}{h\,dt}$ be neglected in the second, the resulting value of $\frac{dn}{n\,dt}$ is what would have been found by M. Pontécoulant, if he had taken his own h to be constant instead of M. Plana's h.

If in the third expression $\frac{da}{a\,dt}$ be neglected, we obtain the value of $\frac{dn}{n\,dt}$ which M. Pontécoulant communicated to me as the result which he had found by using t as the independent variable.

It is obvious that these several values of $\frac{dn}{n\,dt}$ contradict each other, and the reason is that the quantities h, k, and a are really variable, and that therefore $\frac{dh}{h\,dt}$, $\frac{dk}{k\,dt}$, and $\frac{da}{a\,dt}$ have been wrongly neglected. In order to find the true value of $\frac{dn}{n\,dt}$ we must therefore determine the values of these last-mentioned differential coefficients, and substitute them in the several expressions for $\frac{dn}{n\,dt}$ given above.

Now the supplementary terms involving $\frac{de'}{dt}$ which I have shown to exist in the expression for the moon's coordinates, will introduce into the integral

$$2\int r^2 \frac{dR}{dv}\,dv,$$

besides periodic terms, a non-periodic one of the form

$$\int H \frac{d(e'^2)}{dt}\,dt, \quad \text{or } H e'^2,$$

consequently, since in the equation

$$r^4\left(\frac{dv}{dt}\right)^2 = h^2 + 2\int r^2 \frac{dR}{dv}\,dv,$$

M. Plana considers h^2 to denote the whole of the non-periodic part of $r^4\left(\frac{dv}{dt}\right)^2$, h^2 must consist of an *absolutely constant* part together with the variable quantity $H e'^2$ just mentioned

$$\text{and} \quad \therefore \quad \frac{d(h^2)}{dt} \text{ must be equal to } H\frac{d(e'^2)}{dt}.$$

Similarly $\frac{dk}{dt}$ may be found by determining the non-periodic term which is in the same way introduced into the integral

$$\int \frac{dR}{dv}\,dt$$

in the equation

$$r^2 \frac{d\nu}{dt} = h + \int \frac{dR}{d\nu} dt;$$

and $\frac{d\left(\frac{1}{a}\right)}{dt}$ may be similarly found by means of the non-periodic terms introduced into the integral $\int d'R$, in the equation

$$\frac{1}{2}\frac{d^2(r^2)}{dt^2} - \frac{1}{r} + \frac{1}{a} = 2\int d'R + r\frac{dR}{dr}.$$

When all this has been done, and the proper substitutions made, the three expressions for $\frac{dn}{n\,dt}$ are found to agree in giving

$$\frac{dn}{n\,dt} = \frac{d(e'^2)}{dt}\left\{-\frac{3}{2}m^2 + \frac{3771}{64}m^4\right\},$$

which is the result obtained by M. Delaunay and myself.

The supplementary terms in the moon's coordinates which involve $\frac{de'}{dt}$ are of the order of the disturbing force, and therefore the terms which they introduce into the integrals.

$$\int r^2 \frac{dR}{d\nu} d\nu, \int \frac{dR}{d\nu} dt, \text{ and } \int d'R,$$

will be the order of the square of the disturbing force.

This is the reason why $\frac{dh}{h\,dt}$, $\frac{dk}{k\,dt}$, and $\frac{da}{a\,dt}$ are all of the order m^4.

It may be well to mention, in order to prevent any misapprehension, that in my memoir of 1853, k has not the same signification as the h of M. Plana's theory.

It is proved in Art. 11 of the Memoir that

$$2\int r^2 \frac{dR}{d\nu} d\nu$$

contains the non-periodic terms

$$k^2\left\{-\frac{285}{8}m^4e'^2 + \frac{495}{64}m^4e'^2\right\},$$

$$= k^2\left\{-\frac{1785}{64}m^4e'^2\right\}$$

and the k^2 employed in the memoir is the *absolutely constant* quantity added to complete the integral, so that if for the sake

of distinction h_0^2 be written for the h^2 of the memoir, we shall have

$$h^2 = h_0^2 + h_0^2 \left\{ -\frac{1785}{64} m'^4 e'^2 \right\}$$

$$\text{or } h^2 = h_0^2 \left\{ 1 - \frac{1785}{64} m'^4 e'^2 \right\}$$

The following relation exists between the h of M. Plana and the h of M. Pontécoulant:—

$$\frac{h}{h} = 1 + \frac{75}{64} m' + e'^2 \left[\frac{1125}{128} m'^4 \right].$$

Now this relation at once shows that if e' be variable, h and h cannot *both* be constant; and since no *à-priori* reason can be given why one of these quantities should be constant rather than the other, we are not justified in *assuming* that either of them is so.

This argument, however, does not appear convincing to M. Pontécoulant.

In the two methods which, as I mentioned before, I employed previously to the publication of my Memoir of 1853, the value of $\frac{dn}{n\,dt}$ was deduced from those of $\frac{dh}{h\,dt}$ and $\frac{dh}{h\,dt}$ respectively. In the method which I now employ, $\frac{dn}{n\,dt}$ is determined by direct substitution in the differential equations, without introducing either the quantity h or h, that is, without taking into consideration the mean areal velocity at all.

In M. Plana's Memoir, contained in the eighteenth volume of the *Turin Memoirs*, he no longer maintains the constancy of his quantity h, but he determines its variation incorrectly, only taking into account part of the terms which produce this variation. M. Plana here recognises the reality of the supplementary terms involving $\frac{de'}{dt}$, which I proved to exist in the expressions for the moon's co-ordinates; and he finds values for δu and $\delta n\,t$ in pp. 14 and 20 of the Memoir, which coincide with mine, except in the terms with the argument $c'm$, in which a mistake occurs in his coefficients, which, however, does not affect the coefficient of m'^4 in the expression for the secular acceleration. It is very remarkable, however, that although he finds these values of δu and $\delta n\,t$, he does not substitute them in his equations, but puts $\delta u = 0$ and $\delta n\,t = 0$ instead of them. It is only by this strange process of suppressing part of the results which he himself has found, that M. Plana arrives at a different value of the secular acceleration from mine. Indeed, in the first form of this Memoir, as I have already mentioned, M. Plana did actually obtain a value coincident with mine.

M. Plana is led to make this suppression of his own results by a supposed *à-priori* proof that a certain integral which is equivalent to $2\int r^2 \frac{dR}{dv} dv$ can contain no such terms as those which would arise from the substitution in it of the true values of δu and $\delta n t$. Now, even if this proof had been ever so convincing, M. Plana was surely bound to show in what manner the terms thus arising from δu and $\delta n t$ were destroyed, as the different parts of his investigation would otherwise contradict each other.

In fact, however, this proof is entirely fallacious, for it rests on the assumption made at the top of p. 43 of the Memoir, that the terms multiplied by p, p^2, &c., in the equation given on the preceding page, may be neglected; and these are precisely the terms which are equivalent to those which M. Plana suppresses.

It may be as well to make another remark on this part of the investigation. In p. 42, M. Plana puts

$$e'^g \cos g\tau = \Sigma M \cos (pv + q),$$
$$e'^g \sin g\tau = \Sigma M \sin (pv + q),$$

and he assumes that *all* the coefficients p will be *small* quantities. But this will not be the case when $e'^g \cos g\tau$ and $e'^g \sin g\tau$ are thus expressed in terms of the *moon's longitude*. If these functions were similarly expressed in terms of the *time*, viz., if we were to put

$$e'^g \cos g\tau = \Sigma M \cos (pt + q),$$
$$e'^g \sin g\tau = \Sigma M \sin (pt + q),$$

all the coefficients p would be small.

The result which M. Plana obtains in this Memoir is

$$\frac{dn}{n\,dt} = \frac{d(e'^2)}{dt}\left\{-\frac{3}{2}m^2 + \frac{351}{64}m^4\right\},$$

and the difference between this result and mine arises in the way I have explained, viz., from his having neglected to take into account the term

$$-\frac{285}{8}m^4 e'^2$$

which is shown in Art. 11 of my Memoir to constitute part of the non-periodic term of $\frac{1}{a} 2\int r^2 \frac{dR}{dv} dv$.

M. Hansen's value of the secular acceleration is not exhibited in an analytical form, like those of MM. Plana and Pontécoulant, and we can therefore only compare the nume-

rical results. These differ considerably, and, in fact, much more than appears at first sight, on account of a reason which I will explain.

If we put

$$\frac{dn}{n\,dt} = \mathrm{K}\frac{d(e'^2)}{dt}.$$

where K is the coefficient found from theory, the secular equation to be applied to the mean longitude will be

$$\mathrm{K}\int (e'^2 - \mathrm{E}'^2)\, n\, dt.$$

E′ being the excentricity of the earth's orbit at the epoch from which t is reckoned.

Now I find that M. Hansen uses a smaller value of the integral $\int (e'^2 - \mathrm{E}'^2)\, n\, dt$ than M. Plana does; that is, he supposes a slower change in the excentricity of the earth's orbit: and yet his resulting value of the secular equation is larger than those of M. Plana.

It may be inferred, either from the data in the Introduction to M. Hansen's Solar Tables, or from other data in the Introduction to his Lunar Tables, that the value of the integral

$$\int (e'^2 - \mathrm{E}'^2)\, n\, dt$$

which he employs is $-1212''{\cdot}5\ t^2$, t being expressed, as usual, in centuries.

Now M. Plana, in his Theory of the Moon, supposes the value of the above integral to be $-1264''{\cdot}1\ t^2$, and in his Memoir in vol. xviii. of the Turin Memoirs he gives it the value $-1297''{\cdot}7\ t^2$.

If, then, we reduce the coefficients of the secular equation given by these authors, so as to make them correspond with the value $-1270''\ t^2$ of the above integral, which is that employed in my Memoir of 1853, they will become

Coefficient according to	M. Plana's theory	10″·60,
,, ,,	M. Plana's memoir (1856)	11·24,
,, ,,	M. Hansen's theory	12·76.

The difference between M. Hansen's coefficient and either of M. Plana's is much greater than could possibly arise from imperfect approximation, if both had been formed on correct principles.

My value of the same coefficient, which was communicated to the French Institute in January, 1859, is 5″·70. And M. Delaunay, while perfectly agreeing with me in the terms

which I have calculated, has added a great number of others depending on the excentricity and inclination of the moon's orbit, and thus increases the coefficient to 6″·11.

As M. Hansen's method of obtaining his coefficient has not yet appeared, it is, of course, impossible for me to point out the reason of the difference between it and my own, as I have done in reference to the results of MM. Plana and Pontécoulant. I have very little doubt, however, that it arises from M. Hansen having tacitly assumed, like M. Plana, that one of his constants introduced by integration is an absolutely constant quantity.

M. Hansen has suggested that the difference between his result and that obtained by M. Delaunay and myself may arise from want of convergency in the series proceeding according to powers of m, by means of which we determine the coefficient denoted above by K.

If we confine our attention to the terms of K which are independent of the excentricity and inclination of the moon's orbit, and which are admitted by all to constitute by far the largest part of that quantity, we find that the terms involving the successive powers of m taken into account by me give rise to the following parts of the coefficient of the secular equation:—

m^2.....	10″·66,
m^4.....	−2·34,
m^5.....	−1·58,
m^6.....	−0·71,
m^7.....	−0·25.

The sum of these is 5″·78. The convergence, although slow at starting, becomes more rapid in the later terms; and I inferred, in my communication to the French Institute above mentioned, that the remainder of the series would be very nearly equal to −0″·08.

Now M. Delaunay has since calculated the *next term* of the series, and finds it = −0″·06, which is in exact accordance with my anticipations.

Although I think that there can remain no doubt with respect to the convergency of the series, yet, in order to remove all possible objection, I have calculated the value of K by a method which does not require any expansion in powers of m, and the resulting coefficient of the secular equation is

5″·70,

exactly agreeing with that found by means of the series of powers of m.

A very few words will now suffice in reply to the objections which M. Pontécoulant brings forward in his observations in

the *Monthly Notices*. In fact, almost all of them have been virtually answered in what I have said before.

Referring to my statement that, according to my method, I obtain the same value of the secular acceleration, whether the time or the moon's longitude is taken as the independent variable, M. Pontécoulant considers that the fact which I thus announce is "un fait mathématiquement *inadmissible*." Now I confess that it appears to me absurd to suppose that the result of an investigation ought to be different when a different independent variable is employed in obtaining it.

M. Pontécoulant says in p. 308, "En effet M. Adams convient quelque part, je crois, et d'ailleurs je le démontrerai bientôt jusqu'à l'evidence, que la considération de la variabilité de l'orbe terrestre n'exerce aucune influence sur la détermination de *l'inégalité séculaire* lorsqu'on emploie pour l'obtenir les formules directes que j'ai adoptées dans ma théorie:" now here M. Pontécoulant makes me admit what is expressly denied in so many words in my Memoir of 1853. I there say, "When *both* processes have been corrected by taking into account, &c." For the demonstration of M. Pontécoulant I am not responsible.

It should be remarked that in the algebraical portion of his paper, M. Pontécoulant expressly leaves out of consideration all the terms which give rise to the difference between my result and M. Plana's. Thus, at the bottom of p. 311, having found from an assumed term in $\frac{dR}{dv}$, that

$$\int \frac{dR}{dv}\,dt = -\frac{A}{f}\,e' \cos(ft + l) + \frac{A}{f}\,\frac{de'}{dt}\,\sin(ft + l),$$

he incorporates the term involving $\frac{de'}{dt}$ with the preceding under the form

$$-\frac{A}{f}\,e' \cos\left(ft + l + \frac{de'}{e'\,dt}\right),$$

and then remarks:—

"On voit donc que la considération de la variation de l'excentricité de l'orbite terrestre ne fait qu'altérer d'une manière insensible la partie constante des angles des diverses inégalités lunaires multipliées par e', elle ne change en rien la forme des séries qui déterminent les co-ordonnées du mouvement troublé . . ."

Now these alterations of the constant part of the angles on which the several lunar inequalities depend, which are neglected as insensible by M. Pontécoulant, are the very terms which give rise to the terms in the moon's co-ordinate involving $\frac{de'}{dt}$, which I have been the first to take into account.

The term $\frac{A}{f}\,\frac{de'}{dt}\,\sin(ft + l)$ is not destroyed by being

incorporated with the preceding term $-\frac{A}{f}e'\cos(ft+l)$, as M. Pontécoulant seems to suppose.

Again, in order to show that the integral $\int\frac{dR}{d\nu}dt$ can contain no non-periodic term depending on e', M. Pontécoulant assumes, at the foot of p. 310, that $\frac{dR}{d\nu}$ is made up of terms of the form

$$A e' \sin(ft+l).$$

But $\frac{dR}{d\nu}$ is a function of r and ν; and since these quantities contain terms depending on the disturbing force and multiplied by $\frac{de'}{dt}$, $\frac{dR}{d\nu}$ will contain, in addition to the terms of the form considered by M. Pontécoulant, other terms in the order of the square of the disturbing force, and of the form

$$B\frac{de'}{dt}\cos(ft+l);$$

among these there will be a term in which the angle $ft+l$ vanishes; that is, one of the form

$$Ce'\frac{de'}{dt},$$

and consequently $\int\frac{dR}{d\nu}dt$ will contain the non-periodic term $\frac{1}{2}Ce'^2$.

M. Pontécoulant characterises the process which I have employed at the bottom of p. 402 in my Memoir, in order to find the non-periodic parts of certain integrals, as "une véritable *superchérie analytique*." Now this "superchérie" only consists in taking account of the variability of $\frac{de'}{d\nu}$, by putting for it the identical quantity $\frac{de'}{dt}\cdot\frac{dt}{d\nu}$.

In equation [10], p. 12, of M. Plana's Memoir, he finds, for the terms thus objected to by M. Pontécoulant, exactly the same values that I have done, but by an entirely different process. On this same point, in a note to p. 315, M. Pontécoulant makes the objection that in the last step of the integrations referred to I make $d\nu = n\,dt$, contrary to the supposition I had previously employed. But my object was simply to find the non-periodic parts of the integrals concerned; and it is obvious that if I had put for $d\nu$ its complete value $n\,dt - \varphi(\nu)d\nu$, where $\varphi(\nu)$ is a periodic function of ν, this function would only introduce periodic terms in the integrals, and would cause no change whatever in the terms which I have found.

But one of the most remarkable objections in the whole course of M. Pontécoulant's communication occurs in p. 316,

where he says he is going to put his finger on the error I have committed. From an equation in my Memoir he deduces the following:—

$$e' = q + q'\left\{v - \frac{11}{8}m^2 \sin(2v - 2mv) - \frac{77}{16}m^2 e' \sin^2(2v - 2mv - e'mv) + \&c.\right\}$$

and then adds the remark,—

"C'est à dire, que l'excentricité de l'orbite terrestre, outre sa variation séculaire, serait soumise à toutes les inégalités du mouvement lunaire; c'est à dire, à des variations dont le période serait d'un mois, d'une année &c. ce qui est contraire, quelque pétitesse qu'on suppose au coefficient q', à tous les principes de la théorie."

Now it is astonishing that M. Pontécoulant does not see that the quantity enclosed within brackets, in the above equation, is simply the expression of the moon's mean longitude nt in terms of the true longitude v, so that the equation is equivalent to

$$e' = q + q'nt;$$

that is, the excentricity of the earth's orbit varies uniformly with the time, which agrees with the supposition with which we started.

On the other hand, M. Pontécoulant, by making

$$e' = q + q'v,$$

that is, by supposing the change in e' to be proportional to the moon's true motion in longitude, would evidently cause the excentricity of the earth's orbit to be affected by all the inequalities of the lunar motion.

All attempts to express e' in terms of v, without introducing periodic terms, lead to this absurdity.

I have already alluded to the strange notion expressed at the end of M. Pontécoulant's paper, that there may be two values of the secular acceleration, one applicable to the true longitude and the other to the mean longitude. The difference between the true and the mean longitudes consists wholly of periodic quantities, and cannot contain any term increasing continually with the time.

How M. Pontécoulant could have so far deceived himself as to imagine that this paper settled the question of the secular acceleration, "sans contestation possible désormais," is, I confess, beyond my comprehension.

A short Note on the same subject has been received from M. Delaunay, which is as to the points therein adverted to in perfect agreement with the paper by Prof. Adams: this Note has already appeared in the *Comptes Rendus*. M. Delaunay remarks, first, that the argumentation of M. Pontécoulant in a

passage where he says that he neglects for greater simplicity the terms of an order superior to m^2, only applies to the quantities not affected by the correction indicated by Prof. Adams; secondly, that M. Pontécoulant's expression for e' in terms of the true longitude of the moon (by which he points out with his finger the error fallen into by Prof. Adams) in that it contains periodic terms the arguments of which depend on the true longitude, is really an expression varying uniformly with the time.

M. de Pontécoulant, in a letter dated Paris, 19 April, 1860, addressed to the President, maintains the correctness of his results, which he states are verified by the calculation which he has made of the coefficient of the secular inequality directly from the differential equations with the time as the independent variable, and he refers to a Note (which, as appears by the postscript, is to be shortly sent) indicating the result of his researches, and to a Memoir which he is about to print containing the details of the calculation, and forming an appendix to Chap. 7 of his "Theory of the Moon."

Extract of a Letter, dated Feb. 14, from Lieut. Gilliss, of Washington, to Mr. Carrington, read at the Meeting of the Society.

"There seemed no probability of preparations to observe the July eclipse on this continent, until it was shown to Professor Bache how easily he could cause it to be done on our west coast. In consequence thereof, he has determined to despatch a party to the mouth of the Columbia river [129° W. 46° 20′ N.],* from whence its members will proceed to the central line of the shadow, some 10′ or 15′ further north. The duty will be assigned to Mr. Davidson, to whom my eldest son is assistant. The latter embarked for California (*viâ* Panama) on the 6th, having been duly instructed in his subordinate part.

"I hope to be able to get up a party for Cape Chudleigh [65° W., 60° N.], as the most appropriate place for us on this side the water to occupy, and have only been deterred from pressing it during the present month; firstly, from bad health; and secondly, because of doubt whether the land will be free enough of ice to permit a landing so early in the season. This doubt will be settled in a day or two; and if favourably, I trust the journey may effectually drive out the lingering remains of the Peruvian and Isthmus attacks in 1858. I have been almost unfitted for work ever since.

"Do you know whether there are any instruments at York

* The longitudes and latitudes in brackets are not in the original, but are added for convenience.—R.C.C.

Factory and persons capable of using them properly? It will be easy to ascertain at the office of the Hudson's Bay Company, and your countrymen are too liberally inclined towards science to deny the Astronomical Society any service its officers can render. Then, with Bache's party near Astoria, the Hudson's Bay force near York Factory [91° 8′ W., 56° 52′ N.], my own at Cape Chudleigh, yourselves near Santander, and the French on the N.E. coast of Spain and in Algeria, an amount of information may be obtained, far exceeding that derived from all previous eclipses. May the day prove auspicious for all!"

The above intelligence will be highly acceptable to this Society, and meet with its hearty wish for the success of so much astronomical enterprise.

The hint thrown out by Lieut. Gilliss has not been lost on Mr. Carrington, who put himself in immediate communication with the Secretary of the Hudson's Bay Company. He learns that, although probably no instruments are at York Factory fit for the observation desired, the British Commissioner will leave Montreal for his annual expedition early in April, and will, as far as may be in his power, endeavour to have carried into effect any simple instructions which may be supplied him for the direction of intelligent but not astronomical persons, in the Company's service. Mr. Hind has kindly supplied accurate data for the American continent, from which it appears that Fort York is a little within the north limit of totality, and Fort Severn very near the south boundary of the shadow. A landing at some intermediate point may probably be effected, and the wishes of Lieut. Gilliss more or less carried out. As the journey to Fort York is partly effected by boat and is tedious, it is very possible that Sir G. Simpson may select one of the trading stations on the Saskatschewan in preference. The selection of the best station can only be made by an officer of the Company acquainted with the relative capabilities of the subordinates at the different points.

The fifteenth volume Part I. of the *Beobachtungen der Kaiserlichen Universitäts Sternwarte Dorpat*, which has recently appeared, contains an article by Dr. Mädler, on the Solar Eclipse of the 18th July next, with the circumstances of the eclipse calculated for 112 places in the Peninsula and the neighbouring countries, and illustrated by a map, and a plate of the position of the heavens during the eclipse, for the horizon of Saragossa. The Campvey of the Isle of Iviça (which is nearly on the central line) is indicated as an elevated situation likely to afford a magnificent spectacle of the passage of the shadow and the general phenomena of the eclipse.

The current volume of the *Atti della Accademia de' Nuovi Lincei* (January 1860) contains an article by P. Secchi, "Intorno all' Atmosfera Solare e ad alcune proprietà ottiche della Luce reflessa dalla Luna" (8 pages). After mentioning some experiments in support of the existence of a solar atmosphere, the author remarks that even if the existence of such atmosphere be proved, this will not completely account for the *corona* surrounding the sun during a total eclipse, and that it consequently occurred to him to inquire whether the moon had any part therein, and that he was thus led to study the modifications undergone by light reflected by the moon — the most important of these being the polarisation — which subject is discussed in the remainder of the paper. Some account of these researches is given in P. Secchi's letter in the January Number of the *Monthly Notices;* but the observations and reasonings are given in greater detail.

On the Appearance of Jupiter.

(*Extract of a Letter by J. W. Long, Esq. to one of the Secretaries.*)

"I send a copy of some views of *Jupiter*, lately taken, which I promised to forward to you when I last saw you in London.

"The point of most interest is the oblique marking, which you will notice has considerably altered in position and form since first seen. The colour of these markings is also interesting. The belts on the upper or south half of the planet are of a variety of yellowish red, while those in the lower or north half are of a bluish black, particularly the oblique marking."

In the annexed Plates, figures 1, 2, 3, which are the views referred to in Mr. Long's letter, are, by the permission of Messrs. Long and Baxendell, reproduced from the Memoirs of the Literary and Philosophical Society of Manchester. Figure 4 is taken from a drawing which accompanied Captain Jacob's notice of Mr. Fletcher's new equatoreal, (*post*, p. 247) and figure 5 from one of two drawings subsequently communicated by Mr. Baxendell, who, in a letter to the Editor, dated April 21, 1860, writes as follows:—

"With reference to these sketches, I may remark that since Mr. Long first observed the oblique streak on the 29th February, it has gradually extended itself in a preceding direction, or in the direction of the planet's rotation, at an average rate of 3640 miles per day, or 151 miles per hour; the two extremities of the belt remaining constantly on the same

parallels of latitude. The belt has also gradually become darker and broader, some portions, however, being generally much darker than others. On the 14th of March I observed *Jupiter* with Mr. Worthington's 13-inch reflector, power 301, and noticed a faint, curved, dark mark, extending across the bright equatoreal belt from the upper end of the oblique belt to the small dark spot shown in the preceding part of the large belt in my sketch of March 5th. Perhaps I may here be allowed to remark that a transit of the third satellite occurred on the same night (March 14), when it appeared as a *dark* spot on the disk, and was estimated to be three or four times darker than the darkest part of the large belt; but as it approached the limb of the planet its darkness rapidly diminished, and at the moment of internal contact the satellite appeared as a *bright* spot. It was on the bright belt next above the large dark belt. Similar appearances were also presented during a transit of the same satellite on the 21st March. At the ingress the satellite appeared as a bright spot, and at least fully as bright as the brightest part of the planet's disk; but when it had advanced about one-third of its course across the disk it appeared as a large *dark* spot at least *three* times darker than the darkest part of the large belt. These observations had been made before I was aware of Mr. Prince's interesting observation of January 31st, reported at page 212 of the last number of the *Monthly Notices*.

"To return to the oblique belt. On March 21st the spot at the lower end of the belt had increased considerably in size and depth of shade, and appeared to consist of two spots in contact. The sketch of April 9 shows the belt, now considerably lengthened, extending completely across the planet's disk. The sketch of April 20 shows the preceding end of the belt in the middle of its path across the disk. This end is now very dark, and was estimated last night to be two or three times darker than the darkest part of the large belt. Its colour was also remarkable, being bluish-black when contrasted with the dull yellowish red colour of the large belt."

Remarks on the Appearance of Jupiter.

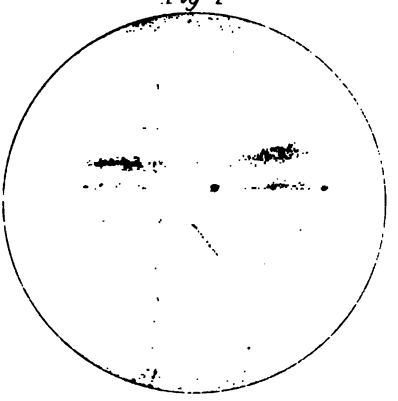
By the Astronomer Royal.

On viewing *Jupiter* with the S.E. equatoreal of the Royal Observatory, Greenwich, on 1860, March 26, about $9^h\ 30^m$, the following appearances were remarked:—

1. Near the apparent lower or north pole there were insignificant belts, as usual.

2. A little below the equator there was a broad discontinuous belt, consisting of about six unconnected light clouds,

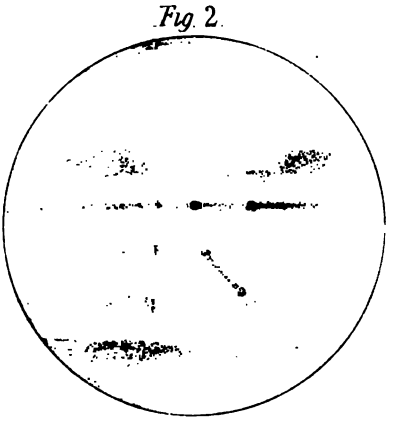
Fig. 1



1860. Feb. 29
h. m.
7 45 G.M.T.

M.r Long. with
5 in Achromatic
power 305

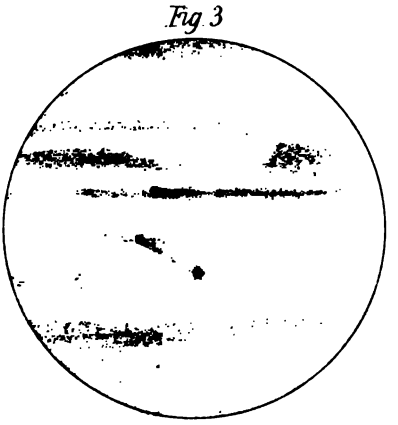
Fig. 2.



1860. March 2nd
h. m.
9 24. G.M.T

M.r Baxendell.
with 5 in Achromatic
power 223.

Fig. 3

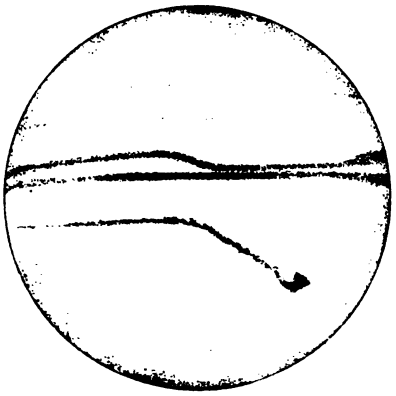


1860 March 5th
h. m.
7 18 G.M.T

M.r Baxendell
with 13 in Reflector
power 301.

Views of Jupiter

Fig. 4.



March 12.
m G.M.T.

As seen in M.r Fletchers Equatorial 9.5 in. power 250.

Fig. 5.

April 9.
r G.M.T.

M.r Baxendell with 5 in. Achromatic power 223.

M.r Dawes' Satellites of Jupiter.

The third Satellite

Fig. 3.

1860. Jan. 31
4 inch refractor.
power 530.

Fig. 2.

1849. Feb. 11.
6⅔ inch refractor
power 460.

Fourth Satellite

Fig. 1.

1849. Feb. 12.
6⅔ inch refractor
power 460.

each of a rude circular form (with great irregularities); reminding me of one of Professor Piazzi Smyth's drawings of *Jupiter*, as seen on the Peak of Teneriffe.

3. In the region above the equator there was a double belt, inclined perhaps 15° to the equator and to the belt which I have just described. Its right hand or eastern end was nearly on the equator, but its left hand or west end was considerably above it or south of the equator. The left-hand ends were broader than the right-hand ends. The outlines irregular. The colour had a most decided reddish tinge; a pale brick-red.

4. Above this, three or four belts parallel to the equator, stronger than those of the lower part.

On the Appearance of Jupiter's Satellites while transiting the Disk of the Planet. By the Rev. W. R. Dawes.

That some of the satellites of *Jupiter* while passing over the planet's disk appear as dark spots, is noticed by Sir John Herschel (see *Outlines of Astronomy*, Art. 540) as having been first observed by Schröter and Harding; probably, therefore, some thirty or forty years ago: but it is not stated where their observations of the phenomenon were published. For more than twenty years the fact has been, I believe, very generally known in this country; and it has been remarked that the third and fourth satellites sometimes appear so dark as at first sight to be mistaken for their shadows. From my own observations it seems that the shade of darkness in the same satellite varies considerably, being greatly influenced by the degree of *contrast* with the planet's surface, as the satellite may happen to transit along a bright zone or a dark belt; and also by the condition of the sky, which when at all hazy causes a diffusion of light over the dark spot of the satellite. I have noticed the *fourth* satellite to be always much the darkest, and though the dark spot is never quite round, yet that it is more nearly so than the third ever is. And this agrees with the appearance of the disk of the satellite when off the face of the planet, as examined under a fine sky and with high powers. I have occasionally noticed that portions of this satellite near its edge are scarcely visible and almost appear to be wanting, from their small reflective power; giving the disk of the satellite the form depicted in the accompanying sketch, copied from one made in my observatory journal at the time. (Fig. 1.)*

On 1843, Dec. 8, I observed the transit of the *third* satellite with Mr. Bishop's 7-inch refractor under fine circumstances,

* See figures of *Jupiter's* satellites in the second Plate.—Ed.

and in my journal have noted that it appeared as a *black* spot, which when approaching the western edge very rapidly changed to a bright one. The power used was only 108, and no irregularity of figure in the black spot was noted. But on 1849, Feb. 11, under a fine sky, with powers 323 and 460 on my 6¼-inch Munich refractor, I observed the irregular form of the dark part of the satellite, and made a sketch of its appearance, of which an enlarged copy accompanies this paper. (Fig. 2.)

On the 31st of last January I observed, under favourable circumstances, the transit of the third satellite. My 8¼-inch refractor bore well a power of 530, and the shape of the dark portion of the satellite was satisfactorily shown as in the accompanying sketch. (Fig. 3.) A streak of light entered the dark part from the south side, and the north and north-east parts were decidedly the darkest. At $11^h\ 47^m$ G.M.T. the satellite was approaching the western limb, and had begun to change its appearance. The *southern edge* was then *rather brighter* than the part of the planet on which it was projected, and appeared as an *irregular bright crescent;* while the centre, and still more decidedly the northern part, remained *darker* than the planet. And when the satellite was so near the edge of the planet that the whole of it appeared brighter than that part of *Jupiter*, the form of the dusky region was still visible, and continued to be so after the satellite had moved off the disk. On the 24th of the same month, with Alvan Clark's 8-inch object-glass I obtained excellent observations of this satellite, both on and off the planet, with powers 300 and 400; and the sketch I then made of the dark portion agrees so nearly with the sketches on the 31st, and on 1849, Feb. 11, as to render it probable that the same aspect of the satellite was turned towards the earth on all those occasions.

The *first* satellite sometimes appears as an irregular gray spot, but never so dark by many shades as the third usually is. I have however been frequently unable to see it at all during the central half of its transit.

The *second* satellite has always seemed to me to be the brightest of the four *for its size;* and I have never been able to detect the slightest shading upon its disk during its transits over the planet, or at any other time.

It would seem to be quite beyond all reasonable expectation that so bright a satellite as the third looks when off the disk, and even when not far within it, should be so much less bright than the darkest parts of the darkest belts upon the planet; and also that when it reaches a certain part of the disk, its change from a dark spot to a bright one, or from a bright to a dark one, should be so rapid. Yet no more delicate photometer could be applied to the different parts of the planet's surface. If however the appearances are merely the effect of *contrast* between the brightness of the satellite and the less or greater brightness of different portions of the planet, it seems difficult

to account for the fact, that if *two images* are produced by a double-image micrometer, and a satellite in one is brought upon the planet in the other, the satellite appears as a *bright* spot on whatever part of the planet it may be placed. And this is the case even when, by an unequal division of the cone of rays from the object-glass, the image to which the satellite belongs is made decidedly fainter than the other.

Observatory, Haddenham, Bucks, 12 April, 1860.

On the Appearance of Jupiter's Third Satellite on the Disk of the Planet. By Captain Noble.

At page 57 of the present volume of the *Monthly Notices*, is an account by Mr. Lassell of an observation of *Jupiter's* fourth satellite as a dark spot upon his disk. I wish to call the attention of observers to the fact that a similar phenomenon is presented by the *third* satellite. On the night of Wednesday, March 21, I watched the third satellite as it began its transit over the planet. At $9^h\ 50^m$ G.M.T., when it was fairly on *Jupiter's* limb, it presented the appearance which it theoretically should have done, that of a brilliant point of light. I left the telescope soon after this. On my return to it at $11^h\ 30^m$, I saw the satellite (now as nearly as possible half way across the disk of the planet) as a well-defined distinct dark spot, "almost as dark" (as I find from the entry in my observatory book) "as the shadow of the satellite itself on an indifferent evening." The contrast of tint was most marked between the satellite and the portion of the great southern belt upon which it was projected; and the bifurcations and markings of which were uncommonly well seen. I, unfortunately, could not observe the egress: a fact I lament, as it would have been a point of great interest to determine at what portion of its passage across *Jupiter's* disk the satellite exchanged its dark appearance for a brilliant one. I am informed by Mr. C. Leeson Prince that he and another Fellow of the Society, Mr. F. Brodie, witnessed a similar appearance of the third satellite during one of its transits some weeks since.

Forest Lodge, Maresfield, Sussex, March 24, 1860.

Account of the Performance of Mr. Fletcher's New Equatoreal. By Capt. Jacob.

Having lately had the opportunity of trying one of the largest telescopes yet made in England, it has occurred to me that some account of its performance may be interesting to the

Society, and may serve to show that it is not now needful to go abroad in order to procure a first-rate instrument.

The telescope in question is the property of our worthy Fellow, J. Fletcher, Esq., of Tarnbank, where it has been but recently erected by him; the optical portion by Cooke, of York; but the mounting, a long polar axis of cast-iron in the form of a double cone, with 3 feet circles, has been executed under Mr. Fletcher's own direction at an engineering foundry at Whitehaven, belonging to one of his brothers.

The object-glass is of $9\frac{1}{2}$ inches aperture and $12\frac{1}{4}$ feet focal length; the telescope is thus nearly identical in dimension with the great Dorpat telescope, but a trifle shorter.

The trial was made under some disadvantages, the adjustments being only approximate, the driving clock and tangent-screws not as yet fixed, and the glass not kept perfectly clean, the workmen being still employed about the Observatory, and making much dust. The weather also was not very favourable, for though the air was on two or three occasions pretty transparent for a short time, it was never quiet enough to admit of high powers being used with advantage. However, after several vain attempts, on Saturday, 10th March, a good view of *Saturn* was obtained with powers 190 and 250; the obscure ring, streaking of the south hemisphere, and faint line on the outer ring, were very well seen, and a small point of light was discerned just clear of the preceding *ansa*, which proved to be *Enceladus*.

On the 12th a good glimpse was also obtained just about dusk, and *Enceladus* was seen at its east elongation at $7^h\ 45^m$. *Jupiter* was also examined, and presented the unusual appearance shown in the accompanying sketch.* Nearly the same appearance was again seen on the 14th at 10^h; but the oblique belt had then extended itself and encroached on the equatoreal belt, forming a darkish spot at the point of junction. On the same night nearly as good a view of *Saturn* was obtained as on the 10th, and the whole of the seven old satellites were seen at once, as shown in the accompanying diagram A. *Enceladus* having come to his west elongation at $6^h\ 55^m$, while there was yet daylight enough to read the address of a letter! *Mimas* was judged to be at east elongation at about $10^h\ 48^m$ G.M.T. Though the features of the planet and ring were best brought out by a power of 250, it was found that a lower power answered better for the small satellites, and they could be steadily viewed only with an eye-piece of 134. The whole seven were again seen on the 15th, as in diagram B. The exhibition of so small a point as E, in such close proximity to the ring, is perhaps as good a test of the defining power of the object-glass as could be desired. *Mimas* appeared to be at his elongation about $9^h\ 25^m$.

* See second Plate, fig. 4. The other diagrams referred to are not printed.

The points marked *h* in the diagrams were at first suspected to be *Hyperion*, but as they are too far distant, and their place also does not agree with Lassell's observations, they must have been two distinct minute stars; they were about the minimum visible, certainly not half as bright as *Mimas*.

A few double stars were examined, but no extremely close or difficult ones, as the instrument was not in order for finding or following such objects, and the night would not bear power. *ζ Cancri* and *η Orionis* were about the closest tried; the former was, of course, easily separated, and the latter also appeared to be so by glimpses when the atmosphere was steady for an instant.

Procyon was examined for focussing and centering with satisfactory results, the image being round and concentric both in and out of focus on both sides, and the disks very small when in focus with powers up to 400.

The mounting seemed remarkably firm, and so smooth in its movement, that, with the telescope slightly under-counterpoised, a star could be well followed and kept pretty near the centre of the field by gentle pressure with the finger. On the whole, I should say it was creditable both to the designer and the maker, and that in such able hands it is likely to prove a most efficient instrument; but Mr. Fletcher will, doubtless, give a fuller description of it himself when he has got it into working order.

Malvern, April 5, 1860.

On the Satellite of Saturn, "Mimas."

By the Rev. W. R. Dawes.

I got a capital view of *Mimas* on March 21, $12^h\ 0^m$ G.M.T. a little past his western elongation. I saw him well, though not without many interruptions from atmospheric disturbance, for an hour and twenty minutes, when his approach to the ring and the planet's diminished altitude rendered him no longer visible. I had only looked for him once before this apparition, and I now find that he was then on the face of the planet.

Alvan Clark has seen him frequently with his new 12-inch object-glass; and told me he was sure I could see him in a fine sky.

March 21 was very clear, though the planet was tremulous.

Hopefield Lodge, Haddenham, Thame,
April 12, 1860.

Note on the Projection of Saturn on the Dark Limb of the Moon. By the Rev. Prof. Baden Powell.

In the last number of the *Notices* (March 1860, p. 213), Mr. Pollock has recurred to the phenomenon described by him in a former number, of the projection of *Saturn* on the *dark* limb of the moon on May 8th, 1859, and to the remarks of the Astronomer Royal, which do not seem to recognise the appearance as described.

As some doubt, however, still seems to attach to the case, perhaps I may be allowed to observe, that the subject of such projections was referred to by me long ago in two papers printed in the Society's *Memoirs*, vols. xvi. and xviii. For projections on the *bright* edge there may be little difficulty in theory; but practically, the point to be accounted for is, why they should be seen in some instances and not in others, under conditions apparently the same.

For the *dark* edge, in my first paper (p. 309), I pointed out generally a *peculiar optical principle* which might *possibly* afford an explanation. And in my second paper (pp. 84 and 90), I have referred to all the recorded instances of projections on the *dark* edge; yet how far *any* optical cause can here apply may be very doubtful; and it would seem more probably due to some atmospheric effect.

The appearance described by Mr. Pollock is, in my opinion, a very remarkable one, especially in regard to the magnitude of the effect; so striking as to be noticed by a person totally unprepared for it, and with the naked eye; while it was not visible in the telescope. All the recorded observations of this kind are telescopic, and this singular fact would seem rather to point to some *ocular* cause.

In reference to Mr. Pollock's note on the Occultation of *Saturn* in the last *Monthly Notice*, the Astronomer Royal observes:—

"Mr. Pollock cannot fail to remark that what we call the dark limb of the moon, though relatively dark, as compared with the bright limb, is absolutely bright, and brighter than the surrounding sky; and its image is, therefore, swelled beyond its geometrical limits upon the retina by any cause of indistinctness, whether ordinary irradiation, faulty adjustment of the eye, or any other cause; in the same manner as that of the bright limb, though probably not to the same visible extent. The proportion of its brightness to the brightness of *Saturn's* disk is unimportant, just as the proportion of the brightness of

the moon's bright limb to the specific brightness of a star's spurious planetary disk is unimportant. The explanation of the phenomenon of projection is, therefore, exactly the same in the two cases."

Royal Observatory, Greenwich, 1860, April 7.

Suggestions for a true Estimate and Register of the Colours of the Stars. By S. M. Drach, Esq.

The author remarks, "The valuable chapters on this subject in Admiral Smyth's 'Hartwell Cycle,' and the conflicting opinions of simultaneous observers on the same single or double stars, induce me to suggest that the practical observer be furnished with a binocular eye-piece, whereof one shall have at its object-end a disk wherein the several gradations of one colour shall be marked as in Saussure's Cyanometer. By turning this disk till the tint be judged equal to the star-tint viewed by the other eye through the other eye-piece, an independent scale would be attainable. The disk might be made of transparent horn, so as to admit of illumination, and a number of silk thread patterns be disposed circularly round it, producing the effect known to inspectors of silk-fringe pattern-cards. These colours would probably be more permanent than oil or water paintings. We might introduce an interior smaller circle with tinted threads of another colour (*e. g.* various tints of red and green). A little daylight practice on this disk would be an excellent apprenticeship to judgment by the single eye-piece directed to the stars."

March 12, 1860.

Mr. Dawes, in a letter to the Editor relating to the employment, in a communication to the Society, of the expression "the Hodgson solar eye-piece," sends the following extract and remarks of his own on the subject:—

Extract from Sir John Herschel's "Results of Astronomical Observations at the Cape of Good Hope," p. 436.

"The image formed at the first surface" (of a transparent glass reflector) "being received by a ***small plane glass reflector,*** will, if the reflection from the second surface of that mirror be also got rid of)"—[viz. by leaving its back transparent and

open to the air]—"be finally reduced before reaching the eye-glass to little more than $\frac{1}{900}$th part of the original intensity, &c."

Acting upon this suggestion soon after Sir John's work appeared, viz. in 1848, I constructed an eye-piece on this plan, and used it for some years with my 6½-inch Munich refractor. I afterwards got one made by Dollond upon the same plan, except that *both* surfaces of the glass reflector were *plane*, and the second surface was made rough in order to destroy any distinct reflection from it.

After the Meeting of the Royal Astronomical Society in December 1854, while examining the eye-piece which Mr. Hodgson had placed upon the table, and which was constructed upon the same principle, I adverted to Sir John's proposal, and expressed my opinion, that if any one's name were given to this form of diagonal solar eye-piece, it ought to be *Sir John Herschel's*.

Hopefield, Haddenham, March 1860.

Note respecting the Parallax of α Herculis. By Capt. Jacob.

I have lately been informed that objections have been made on the Continent to my discussions of the Parallax of *α Herculis*;* first, on the ground that it is absurd to expect a difference of parallax between two stars having a large and nearly identical proper motion; second, because there is a certain bias or personal equation to which the eye is liable in measuring double stars, depending partly on the distance from the meridian, which bias must be determined and corrected for, before any trustworthy results can be derived from the measures, as to parallax, or change of angle in any way.

Now, with regard to the first objection, it is simply inapplicable; *α Herculis* not having a large proper motion, the amount assigned in the British Association Catalogue being 0″·05; while, to assume the identity of proper motion between the pair, is simply begging the question, a factor for difference of proper motion having been introduced by me in both of my discussions.

As to the second objection, without pretending to deny the fact of such a bias generally existing, I may state that, in my own case, its influence has been found to be very small; in fact, nearly insensible, excepting when the object has been near the zenith, or otherwise so situated that it could not be observed comfortably, and such positions I have, therefore, habitually avoided when practicable.

* In volume of *Madras Results* for 1848–52 (Appendix), and in vol. xxviii. of the *Mem. R. Ast. Society*.

But to obviate any possible objection on this score, in the case of *α Herculis*, I was careful to take all the measures as nearly as possible under similar conditions; viz., nearly all by daylight, and all those which would tell strongly on the parallax, on the same side and about the same distance from the meridian; considering that prevention was (in such a case emphatically) better than cure. This has been already stated in the first discussion in the Appendix to the Madras volume for 1848–52, at p. 21, but as the statement seems to have been overlooked, I have taken the liberty of calling the attention of astronomers to it.

The parallax being but small, had its amount depended on the application of any such uncertain corrections, I should, for my own part, have considered the determination as of very small value.

Malvern, April 5, 1860.

Note on one of the Minute Companions of σ Orionis.

By the Rev. T. W. Webb.

My attention was some time ago directed to the smallest star of the principal group connected with *σ Orionis*, designated as *a* in the Bedford Catalogue, by a communication received from Thomas Warner, Esq., of Brighton, in which he states that, while pointing his telescope, of $3\frac{1}{4}$ inches aperture and 4 feet 4 inches focal length, for a friend, on November 22, 1859, he was surprised to see, at the first glance, what he had not seen before; something which a moment's attention showed him to be a star, on the left of the chief star in the group. He was not thinking at the time that there was a small star to be looked for, and he certainly had not any knowledge where to look for it. He has since seen it several times, but never so clearly as on that day; however, in the beginning of the present year, a boy, who was staying with him, said he saw three stars in a row with the bright one, and that the smallest was to the left of the others. When Mr. Warner saw it, it was about as easy an object as the companion of *ζ Persei*, and certainly at that time easier than that of *κ Geminorum*. At the date of his communication (January 12) the latter was much less difficult than *a* in *σ Orionis:* hence, though atmospherical circumstances may have been the cause, he is rather disposed to infer variable light; and supposes that it may have been near its minimum when the elder Herschel, and subsequently Herschel junior and South, observed the others; that it may have been brighter when Struve said of it, "quanquam non est valde debilis," and classed it 10·3, like the companion of *λ Geminorum;* that it may have been brighter still when Smyth classed it 11, and the

companion of λ *Geminorum* 12; and that it may have been near its maximum when it caught Mr. Warner's eye, November 22, 1859, as little, if at all, smaller than the companion of ζ *Persei*, rated 9·3 by Struve, and 10 by Smyth. He adds, however, that he had not compared the latter star with the suspected variable star under the same circumstances.

In consequence of this communication, I intended to have kept my eye upon this object, but the unusually cloudy and boisterous weather of the recent season, especially near a mountainous region where snow has been almost uninterruptedly visible for five months, has repeatedly frustrated my wish. I was able to see it, however, four times with my 5·5 inch object-glass, and once (at Gloucester) with an 8¼-inch speculum by Sir W. Herschel: but on no occasion did it appear to me sufficiently conspicuous to warrant the idea that it could have been perceived, at least without very great difficulty, in the telescope employed by Mr. Warner.

On two cases of Solar Spots in High Latitudes, and on the Surface Currents indicated by the Observations. By R. C. Carrington, Esq.

At pages 76 and 77 of the present volume I stated an opinion, based on pretty extensive research, that solar spots had not hitherto been observed in heliographical latitudes of anything like 70°, and that the assertion had probably originated in some numerical error. Lahire is the observer with whose name the observation is coupled; but I have not succeeded in finding any passage among his various notes on the subject, in which he himself states such a conclusion. It is important, however, to put on record, in a form not admitting of dispute, cases of the highest latitudes known to have been reached by veritable well-developed solar spots; and though the two following may already be surpassed by instances known to other observers, I offer them as satisfactory and indubitable in the evidence.

The first case is that of a large single spot, of a form generally round, with a nucleus of about 20 seconds diameter and average width of penumbra about 60 seconds, followed at a distance by one, and afterwards by three small detached spots, sufficiently removed not to affect the motion of the principal spot. This was observed at the Capo di Monte Observatory at Naples, in the year 1846, by my esteemed correspondent, Dr. Peters, now Professor of Astronomy in Hamilton College, New York, who has favoured me with exquisite copies of his drawings and the original numbers of his observations; so that

I have been enabled to verify his concluded positions by reductions effected in my usual way, and with the elements of solar rotation which I have hitherto adopted.

The centre of the nucleus was observed by Dr. Peters in right ascension and declination, by differences in time and angle from the sun's limbs; from which I deduce the following angles and distances from centre in decimals of the sun's radius, and lastly the heliographical longitudes from node and latitudes:—

Naples, 1846.

	Naples M.T.	ϱ	θ	l	λ
	h m		° ′	° ′	° ′
June 8	0 52	·9041	19 32	134 20	+50 2
13	0 26	·7799	339 32	199 42	+50 55

The spot was observed on these two days only, but the agreement and the check thus obtained are amply sufficient to establish its position beyond a doubt. If the longitudes be compared by deducting from the second 70° 40′ for the interval between the observations, on the supposition that the sun's rotation is effected in 25·380 mean solar days, we find the following differences in 5 days:—

$-5°\ 18'$ in longitude and $+53'$ in latitude,

or daily drifts at 50° N. by this one spot,

$-64'$ in longitude and $+11'$ in latitude;

conclusions which we will recur to below.

The second case is that of a smaller spot, elongated and at first with two small nuclei contained within one penumbral boundary, but which diverged and separated into two detached spots, both of which were sufficiently observed and sketched by myself in the month of July 1858. This group was not visible on the 30th of June. The motion of divergence, a kind of mutual perturbation, of which I have before given instances illustrated by drawings to scale, will here require to be allowed for, as will be seen. First of all, the result of observation is as follows:—

Redhill, 1858.

	G.M.T.		ϱ	θ	l	λ
	h m			° ′	° ′	° ′
July 1	3 54	Q	·7540	187 47	215 32	−44 46
2	4 16	q	·7986	201 16	231 42	−44 15
		Q	·7910	198 23	228 27	−44 50
4	0 39	q	·8996	215 27	255 27	−43 59
		Q	·8836	212 30	250 59	−44 53

If we now correct the longitudes for rotation to noon on

July 1, and with the same value of solar rotation 25°·380, we shall have,—

For Noon on July 1	Q	212° 59′	−44° 46′	by July 1
	q	214 44	−44 15	by July 2
	Q	211 29	−44 50	,,
	q	212 16	−43 59	by July 4
	Q	207 48	−44 51	,,

In the next place it will be observed that the differences of longitude and latitude of *q* and Q are

On July 2	3° 15′	and	35′
On July 4	4 28	and	52

so that they diverged at the rates of 36′ and 8′ per diem. Without insisting on the utmost exactness for these values, yet taking them as generally quite in accordance with previous conclusions, and correcting the previous positions by dividing the divergence equally between the two nuclei, we shall lastly have the following positions, corrected for all but the mean surface drifts:—

For Q	July 1	212° 59′	−44° 46′
	2	211 47	−44 46
	4	208 38	−44 43
and			
For *q*	July 2	214 26	−44 19
	4	211 26	−44 11

and from which the daily drift at 45° S. by the observations of this one group, is

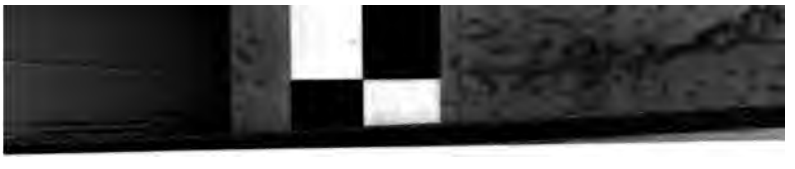
− 85′ in longitude,

and uncertain (or slightly towards the equator) in latitude.

If, now, the reader will refer to page 83 of the preceding volume of the *Monthly Notices*, from which I bring forward the remainder of the following table, he will see the extreme value of collecting cases such as the present.

Mean Surface Drifts per Diem.

At 50° N	−64	in long.	+11′	in lat.	by 1 spot
30 N	−25	...	+ 5	...	4 series
18 N	−14	...	+ 1	...	4 ,,
8 N	+ 8	...	− 5	...	14 ,,
11 S	+10	...	− 3	...	6 ,,
19 S	−10	...	+ 1	...	5 ,,
29 S	−21	...	+ 4	...	8 ,,
45 S	−85	...	(− 2)	...	by 1 group



The two lines now added will, I think, be allowed to add as much to the importance of the table, as they extend its range in latitude. The daily drifts in longitude above given are, however, relative and not absolute, being derived by the use of 25·380 days as the period of solar sidereal rotation. If a period of 25·000 days be used, the motions in longitude will all be more negative by 13′ per diem, and by graphical interpolation we should infer the following as a general sketch of the surface currents:—

With Period 25 Days.

At 50°	N	−94′	in Long.	+2′	in Lat.	towards N. Pole
40	N	−64	...	+5	...	
30	N	−40	...	+5	...	
20	N	−22	...	+1	...	
10	N	−10	...	−4	...	towards Equator
0		0	...	0	...	
10	S	−10	...	−4	...	
20	S	−22	...	+1	...	towards S. Pole
30	S	−40	...	+5	...	
40	S	−64	...	+5	...	
50	S	−94	...	+2	...	

I am inclined, after some consideration, to think the above second table, in which all the daily drifts in longitude are retrograde, the more faithful representation of the actual motions, though the numerical values will for some time bear modification. And I cannot altogether refrain on this occasion from adding what I conceive are the inevitable conclusions to be derived from it. The matter which forms the photosphere, and which, undoubtedly, floats suspended in the sun's atmosphere, with as definite a lower surface as the upper visible one, appears by my observations to flow slowly towards the poles in each hemisphere, from the latitude of about 15°, this polar westerly drift becoming more and more westerly till at 50° or 60° the polar tendency is insensible. An accumulation must result, and as certainly find its vent. I conceive that this is probably effected by subsidence (any more definite expression might merely provoke objection,) into the second stratum, the surface of which we see as the penumbra of spots, and that as part of the second stratum, similarly floating above the sun's body in its atmosphere, and at its own lower level, the matter thus transferred passes again towards the equator, acquiring as it approaches it the velocity of rotation lost in its passage towards the pole in the upper stratum. On its arrival, or on the arrival of an equivalent portion of the lower stratum at the latitude of about 15 degrees, I conceive that a corresponding ascent takes place into the upper surface, the mo-

tion being still towards the equator, and in longitude again suffering retardation by the general cause (whatever for the moment that may be) which causes the marked retrogradation of the higher latitudes. At the equator I conceive that a second subsidence of the upper stratum into the lower takes place, and that the motion of the lower stratum is towards the pole from the neighbourhood of the equator to about 15 degrees of latitude, where an ascent again takes place to supply the loss of the upper current consequent on the polar drift we set out with. Those who are acquainted with Lieut. Maury's conclusions respecting the circulation of the under and upper currents of our own winds will, I think, see more analogy than difficulty in the above notions. He had to infer the upper currents from the observed lower currents: the case here is the exact converse. But whence the lost velocity of rotation which the increasing retrogradation in higher latitudes indicates? And how is the lost velocity in the upper current regained through the lower current? I imagine by the sensible contact of the upper stratum with a vast atmosphere or medium which does not wholly partake of the rotation of the body it surrounds; and that the compensation is by the action of the body beneath the second stratum rotating faster than that stratum. In other words, I conceive that if we arrive at the conclusion that 25 days is the least time in which any zone of the upper stratum rotates, it must be the greatest limit of the time of rotation of the body beneath.

According to the view here thrown out as a speculation for discussion, the system of under-currents of the second stratum will be represented by the same table as that last given for the upper currents, changing only the signs in latitude. Beyond 50° of latitude I imagine the motions of both strata to be the same and simply retrograde around the poles: the crossing of the two strata (consequent on the opposite signs in latitude) occurring only within the region frequented by spots.

Many will be reminded of the following words of Sir J. Herschel:—"The spots in this view of the subject would come to be assimilated to those regions on the earth's surface in which, for the moment, hurricanes and tornadoes prevail."

The immediate object of this paper was, however, the publication of two indisputable cases of spots in high latitudes, the highest known to me, and to challenge the production of cases equally authenticated which may have surpassed them.

Note.—Several drawings were exhibited at the meeting in illustration of the above paper.—Ed.

Results of Meridional Observations of Small Planets; Occultations of Stars by the Moon; and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

Iris (7).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, Mar. 21	9 2 25·9	9 0 48·35	81 1 5·19
23	8 54 21·3	9 0 35·51	80 55 43·91
26	8 42 27·6	9 0 29·50	80 48 29·20
28	8 34 40·0	9 0 33·75	80 44 12·19

Hygeia (10).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, Mar. 21	11 6 57·4	11 5 40·24	89 46 46·28

Victoria (12).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, Mar. 21	13 8 28·1	13 7 30·93	107 18 26·24
26	12 44 42·1	13 3 23·83	106 49 0·29

Egeria (13).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, Mar. 28	8 25 35·7	8 51 27·95	49 55 11·51

Urania (30).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, Mar. 21	11 10 32·5	11 9 15·98	87 7 32·76

Doris (48).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, Mar. 23	10 21 43·9	10 28 12·46	83 50 0·09

All the observations of N.P.D. have been corrected for parallax.

Occultations of Stars by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
			h m s	
1860, Mar. 4	♑ Cancri disapp.	Dark	7 15 31·5	D.
4	♑ Cancri reapp.	Bright	8 15 30·6	D.
5	18 Leonis disapp.	Dark	9 13 27·0	C.
5	B.A.C. 3345 disapp. (*a*)	Dark	10 7 22·3	C.
5	18 Leonis reapp.	Bright	10 18 59·4	C.

(*a*) Doubtful, the star very faint.

Eclipses, Occultations, and Transits of Jupiter's Satellites.

Day of Observation. 1860.	Satellite.	Phenomenon.	Mean Solar Time.	Observer.
			h m s	
Jan. 1	IV	Ecl. disapp. (*a*)	7 42 56·3	J. C.
6	III	Ecl. disapp. (*b*)	8 56 4·9	D.
7	I	Occ. reapp. last cont.	10 54 47·0	E.
16	II	Ingress, first cont.	11 31 57·7	C.
16	II	,, central bis.	11 33 27·5	C.
16	II	,, last cont.	11 35 12·2	C.
16	II	Egress, first cont. (*c*)	14 18 30·4	C.
16	II	,, last cont. (*c*)	14 21 29·9	C.
24	I	Egress, last cont. (*d*)	6 6 20·3	C.
24	III	Egress, first cont.	8 44 26·4	T.
24	III	,, central bis. (*b*)	8 46 56·0	T.
24	III	,, last cont.	8 48 25·7	T.
31	I	Egress, first cont.	7 46 17·7	C.
31	I	,, central bis.	7 47 47·4	C.
31	I	,, last cont.	7 49 32·1	C.
31	III	Ingress, first cont.	8 38 39·1	C.
31	III	,, central bis.	8 41 8·6	C.
31	III	,, last cont.	8 44 8·1	C.
31	III	Shad. Ingr. last cont.	10 48 2·8	C.
31	III	Egress, last cont.	12 5 5·2	C.
Feb. 6	I	Ecl. reapp.	12 51 32·3	E.
7	III	Ingress, first cont.	11 59 10·8	D.
7	III	,, central bis.	12 3 10·1	D.
7	III	,, last cont.	12 7 9·5	D.
12	II	Ecl. reapp. (*e*)	6 51 14·6	C.
12	IV	Shad. Egr. first cont. (*f*)	7 18 50·1	C.
13	I	Occ. disapp. first cont. (*g*)	11 39 10·4	J. C.
13	I	,, central bis. (*g*)	11 42 24·9	J. C.
13	I	,, last cont. (*g*)	11 45 9·5	J. C.

Day of Observation. 1860.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
Feb. 13	I	Ecl. reapp. (*h*)	14 46 40·7	J. C.
14	I	Ingress, central bis.	9 4 13·2	E.
23	I	Egress, central bis.	7 37 12·2	E.
25	III	Occ. disapp. first cont. (*i*)	8 41 40·9	C.
25	III	,, central bis. (*i*)	8 45 40·2	C.
25	III	,, last cont. (*i*)	8 49 39·6	C.
28	IV	Ingress, first cont.	11 50 54·9	E.
28	IV	,, bisected	11 55 54·0	E.
28	I	Ingress, central bis.	12 39 16·9	E.
29	I	Occ. disapp. last cont.	9 49 3·2	C.
29	I	Ecl. reapp.	13 5 21·0	C.
Mar. 2	I	Ecl. reapp.	7 34 25·1	D.
2	I	Ecl. reapp.	7 34 8·0	M. D.
2	II	Ingress, first cont. (*k*)	14 32 42·6	D.
2	II	,, central bis. (*k*)	14 35 42·1	D.

(*a*), Somewhat uncertain; clock beats nearly inaudible from high wind. (*b*), Disappearance very gradual. (*c*), Image much blurred and very tremulous. (*d*), Planet very tremulous. (*e*), Image of the planet much blurred. (*f*), Very cloudy; the planet disappeared before the shadow was bisected; the time noted was satisfactory. (*g*), Very tremulous. (*h*), Cloudy; planet very faint. (*i*), Cloudy. (*k*), Unsatisfactory; observed through thin cloud.

The initials D., E., C., J. C., M. D., and T., are those of Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Carpenter, Mr. Dolman, and Mr. Talmage.

Observations of Minor Planets. By Norman Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

Polyhymnia (68).

wich Mean Time.	Magnitude.	Apparent R.A.	Apparent N.P.D.	Log (Par. × Δ) R.A.	Log (Par. × Δ) N.P.D.	Comparisons.	Powe
h m s		h m s	° ′ ″				
28 9 39 24	11·1	3 59 19·62	66 43 7·3	−9·266	−0·636	7 with *b*	5

Proserpine (26).

28 12 39 28	11·5	2 20 0·36	75 52 57·6	+9·383	−0·743	12 with *a*	

Ariadne (43).

1860.										
Feb. 19	14	14	14	10·9	10 56 41·54	89 26 27·8	+9·034	−0·824	8 with *g*
	14	33	7	...	10 56 41·10	89 26 29·1	+9·133	−0·823	5 with *g*
23	13	39	43	10·8	10 52 48·70	89 8 2·9	+8·937	−0·822	6 with *f*
	13	57	21	...	10 52 47·64	89 8 0·7	+9·052	−0·822	6 with *f*
27	11	40	17	11·2	10 48 48·63	88 47 39·8	−8·828	−0·820	5 with *d*
	11	55	23	...	10 48 48·06	88 47 36·5	−8·640	−0·819	4 with *e*
28	11	27	31	10·4	10 47 46·57	88 42 19·1	−8·898	−0·819	7 with *d*
29	10	39	30	10·5	10 46 45·79	88 36 51·4	−9·154	−0·819	10 with *e*

Leucothea (85).

1860.									
Feb. 29	11	0	20	11·3	10 43 4·76	77 31 50·9	−8·897	−0·735	6 with *e*
Mar. 2	13	18	1	11·0	10 41 8·59	77 28 39·7	+9·082	−0·738	7 with *e*

Hygeia (10).

1860.									
Feb. 29	12	2	4	9·2	11 21 27·81	91 15 7·7	−8·856	−0·834	10 with *h*
Mar. 2	13	52	7	...	11 19 55·45	91 7 27·7	+9·046	−0·833	6 with *h*

Urania (30).

1860.									
Mar. 2	12	2	15	10·4	$*-2^{m}\ 1^{s}\cdot84$	$*+12'\ 50''\cdot9$	−8·838	−0·820	10 with *i*

The observations were made with the Hartwell equatoreal and two ring-micrometers, and have been corrected for refraction and proper motion as usual. The adopted radii of that used with power 52 have been given on page 7 of this volume. The determinations for the other, used with power 84, fully bear out the remarks there made as to the accuracy attainable in such cases with ordinary care.

From stars in the *Pleiades*, as measured by Bessel, we have

Inner Edge of the Ring-Micrometer used with Power 84.

Determining Pair of Stars.	Radius.	Measures.	Probable Errors of One Measure.	Mean Result.
Anonyma 24 and 39	392″·47	10	±0″·16	±0″·05
26 Tauri and Atlas	392·22	10	±0″·28	±0″·09
Adopted Mean	392·34	20	±0″·24	±0″·05

For the outer edge, we have in like manner,

Electra and Maia	489″·91	5	±0″·19	±0″·08
Asterope and Anonyma 24	490·17	5	±0″·21	±0″·09
Adopted Mean	490·04	10	±0″·19	±0″·06

Besides the magnitudes recorded with the preceding observations, the following estimations were made, on nights when other duties prevented the use of the micrometer:—

Polyhymnia	1859, Nov. 21,	11·0 mag.;	Nov. 24,	11·2 mag.
Parthenope	Nov. 28,	9·5 mag.		
Lætitia	Dec. 8,	10·0 mag.;	Dec. 14,	9·8 mag.

The comparison star used with *Urania* on March 2 proved to be unobserved in any standard catalogue. The assumed mean places of the rest reduced to 1860 were as follows:—

Reference.	Authority.	Magnitude.	Mean R.A. h m s	Mean N.P.D. ° ′ ″
a	B. Z. 281, 9	9·0	2 17 50·02	76 5 22·0
b	B. Z. 393, 49 = 7481 Lalande	8·3	3 55 49·97	66 58 33·9
c	337 Greenwich Observations in 1854	7·3	10 43 46·96	77 40 45·2
d	55 Leonis, Gr. Obs. in 1856	6·0	10 48 30·21	88 31 2·9
e	57 Leonis { Maedler's Bradley, 2379 Armagh }	7·0	10 48 59·68	88 49 15·3
f	2394 Armagh	8·0	10 55 22·65	89 20 32·5
g	p^2 Leonis { Maedler's Bradley, 2400 Armagh }	6·5	10 56 26·59	89 14 53·0
h	Weisse xi. 290 = 21727 & 8 Lalande	6·5	11 17 49·74	91 26 28·6
i	(Approximate Place)	9·0	11 29 10	88 37 45·

Hartwell Observatory, March 31, 1860.

On the Figure of the Earth. By Capt. A. R. Clarke, R.E.*

The investigation was undertaken in consequence of the remarks of the Astronomer Royal in the *Monthly Notices* for January last, on General T. F. de Schubert's *Essai d'une Détermination de la Véritable Figure de la Terre.* The conclusion there arrived at is that the earth is an ellipsoid with three unequal axes, the dimensions of which are given: Captain Clarke remarks that these results are dependent on so small a number of observed latitudes that it becomes highly important to ascertain the probable errors before they can be received as representing the real figure of the earth, and that he had therefore repeated the calculation in a more precise manner, using a greater number of observed latitudes and bringing out the probable error of the elements. The arcs used are the same as those in General de Schubert's memoir, with the omission of the

* This paper will appear *in extenso* in the *Memoirs* of the Society.—Ed.

Prussian and Pennsylvanian arcs. It is noticed that whatever the real figure of the earth may be, if in the investigation we suppose it to be an ellipsoid, it is clear that the arithmetical process must bring out an ellipsoid of some kind or other, which ellipsoid will agree better with all the observed latitudes than any spheroid of revolution would do. Nevertheless it would scarcely, he conceives, be correct to say that we had proved the earth not to be a solid of revolution; to do this would require data which we are not in possession of at present, and several arcs of longitude would be necessary. But in the meantime it is interesting to ascertain what ellipsoid does actually best represent the existing measurements. The calculations are performed accordingly, and the conclusion is that from the magnitude of the probable errors which they exhibit, it appears that we are not yet in possession of data to bring out the excentricity of the equator with anything like certainty. The author mentions that the calculations were made in connexion with the geodetical operations of the Ordnance Survey, but that Colonel Sir Henry James had kindly given him permission to present them to the Royal Astronomical Society.

It will be recollected that the characteristic feature of General de Schubert's investigations is the employment, for the independent determination of the axes of any meridian, of the lengths of two or more sections of the meridian arc and latitudes of the extremities of such sections. In a paper, "Sur l'Influence des Attractions Locales dans les Opérations Géodésiques et particulièrement dans l'Arc Scandinavo-Russe," (*Ast. Nach.* Nos. 1245–1247), General de Schubert remarks that the publication of the Ordnance Trigonometrical Survey of Great Britain and Ireland has opened a new field to geodesy, by introducing into the measurements and calculations an element heretofore neglected, "the deviation of the plumb-line," which, if it does not entirely destroy the discrepancies between the different measurements of meridian arcs, promises at least to reduce them very materially; and, with a view to ascertaining *à posteriori* the extent to which this correction is required, he calculates by means of the various combinations of the observed latitudes of thirteen stations on the arc above referred to, and the distances, as deduced from the geodetical operations, the values of the semi-axes, which values, as obtained from the several different combinations, present discrepancies which afford ground for suspecting at several of the stations errors of latitude arising from a deviation of the plumb-line.—Ed.

MISCELLANEOUS INTELLIGENCE.

The passage of a planet over the sun at the time of Dr. Lescarbault's observation of the 26th March, 1859, is positively denied by M. Liais, in an article in No. 1248 of the *Ast. Nachrichten*. M. Liais states that on the day in question he was observing the sun (with a view to the determination of the relative intensities of the different parts of the disk) at San Domingos, in the Bay of Rio Janeiro, from $11^h\ 4^m$ to $11^h\ 20^m$, and from $12^h\ 42^m$ to $1^h\ 17^m$, [S. Domingos mean solar time]. Taking account of the difference of three hours between the longitudes of Orgères and S. Domingos, the ingress of the spot, according to the observation of Dr. Lescarbault, should have been at $1^h\ 5^m$ and the egress at $2^h\ 23^m$, S. Domingos. At $1^h\ 17^m$ S. Domingos, the spot should have entered on the sun for 12 minutes, and from the rate of progress, according to Dr. Lescarbault, it should have travelled a space of 1′4 on the disk, and its smallest distance from the limb should have exceeded 20″, a quantity too great to be annihilated by the differences of the parallaxes for Orgères and S. Domingos; and M. Liais argues that if the spot had been visible at Orgères, he must have seen it at S. Domingos at the close of his observation, the more so that according to Dr. Lescarbault the spot entered at about 11° from the north pole of the disk, the region which M. Liais was exploring.

There is contained in the *Berichte* of the Royal Saxon Academy of Sciences for 1859 an account, by M. Hansen, of the establishment of the new Ducal Observatory at Gotha.

Situation.—The new observatory lies in the Erfurt suburb of the town of Gotha, on the east side of the Jägergasse, and on the north bank of the Leina Canal. It is found by trigonometrical measurements that the centre point of the tower of the new observatory is in the direction of the meridian 1022·6 meters to the north, and in the perpendicular direction 1342·5 meters to the west of the centre point of the transit-instrument on the Seeberg. The difference of latitude from the old observatory is therefore 33″·10, and the difference of longitude $4^s{\cdot}58$; giving provisionally for the latitude of the new observatory 50° 56′ 38″, and for the longitude from Paris $33^m\ 30^s{\cdot}2$. As the centre point of the transit-instrument on the Seeberg has been used in various measurements of degrees and triangulations, its position was, previous to the removal of the instrument, fixed by a cross on a block of stone buried three or four feet below the surface.

Arrangement. — The observatory contains, as shown on plan, first, a meridian-room, 18 feet from north to south, and $21\frac{1}{2}$ feet from east to west, with two meridian-slits, each 2 feet wide; secondly, an observing-room, of the same size as the meridian-room, with a slit in the prime-vertical; thirdly, a

tower with a revolving roof; the inner diameter of the observing-room in the tower is 15 feet, and the height of the summit from the floor $12\frac{1}{4}$ feet; fourthly, a corridor surrounding the tower, with entrances into the three observing-rooms, and containing a part of the library: it leads also into a second library-room, which forms part of the dwelling-house.

Construction. — The observatory and dwelling-house are built of stone — the observatory of *quaderstein* (sandstone), and the dwelling-house of *bruchstein* (a limestone). The foundations for the instruments are carried down to the ground, detached from the remaining wall-work. There is a commodious dwelling-house fronting the Jägergasse, and the whole is surrounded on the other sides by a small garden.

Instruments.—Under the western slit of the meridian-room is placed the Ertel's meridian-circle, which is so contrived that the requisite number of divisions is brought down to a very small number — 72 on the circle, and 61 on each of the auxiliary graduations — and the complete determination of the graduation-errors is thus made practicable. The arrangement is different in detail, but the same in principle with that described by M. Hansen (*Ast. Nachr.*, tom. xvii.). The meridian-circle is also so arranged that the object-glass and eye-piece of the telescope can be interchanged — affording a simple method for determining the flexion of the telescope and circle. The object-glasses and eye-pieces of the microscope are by Schieck.

Under the eastern meridian-slit there is placed at present an astronomical theodolite by Reichenbach, and the slit serves in particular for observations in or near the meridian, by means of moveable instruments.

The clock in the meridian-room is by Tiede.

The same room contains a galvanic register-apparatus, for the graphical record of the observations with the meridian-circle: this consists of a telegraphic writing-apparatus by Siemens and Halske, provided with two electric magnets, one of which marks down on a continuously moving band of paper the seconds of clock-time, and the other the instants of observation, by means of points.

Under the slit in the prime-vertical is placed the Ramsden's 8-foot transit-instrument, the construction of which is essentially the same with that now made use of. The only fault of the instrument is, that the object-glass does not quite come up in goodness to the Munich object-glass.

In the tower is placed at present the Fraunhofer's heliometer, in which, however, the parallactic arrangement has been, by the Gotha mechanician Ausfeld, renewed, and provided with a larger and better-contrived circle; but the place is intended to be occupied by an equatoreal, with a 6-foot telescope and 2-foot circle given by the Duke, in preparation by the brothers Repsold. A detailed account is promised of

this instrument, which, besides the improvements which the makers have introduced into their equatoreals, is to have others contrived by M. Hansen; and on this account, and from the skill and care of the makers, M. Hansen hopes that he will be able to employ the equatoreal for absolute observations.

The heliometer is being employed by Professor Habicht and M. Gussew, of Wilna (who has been staying at the observatory), in a series of observations of the distances and position-angles of the lunar spots, which are to be applied to the determination of the moon's figure, and of the libration.

As regards smaller instruments, the observatory possesses a number of telescopes, some old reflecting telescopes, an astronomical theodolite by Reichenbach, reflecting sextants, &c.

It is noticed, in conclusion, that as well the dwelling-house as the observatory is lighted by gas, and that the illumination of the clocks and of the fields of view in the principal instruments is effected in the same manner.

The following notice has been circulated by the Astronomer Royal among observers who had expressed their intention of observing the Eclipse of July 18 in the north-east part of Spain.

"I have written officially to the Board of Admiralty, on the subject of furnishing a steamship for conveyance of observers of the solar eclipse of July 18 to the north coast of Spain, and have also had an interview with one of the Lords of the Admiralty.

"There is every disposition in the Board to give all facilities in their power to observers.

"It appears that the accommodation on board the ships of the Royal Navy is so limited, that it would be almost impossible to find one which would convey the number of observers likely to desire a passage on this occasion. The Admiralty, therefore, contemplate as a probable course, the chartering of a merchant-steamer for the voyage; which would be in many respects more convenient. But nothing is yet officially determined.

"There will be no difficulty in landing a portion of the party at Bilbao, and proceeding with the rest to Santander (which port probably will be preferable for the vessel to lie at). And it is much to be wished that observers may be distributed between these two points.

"*Royal Observatory, Greenwich, 1860, May 5.*"

ERRATA.

Page 195, line 2, for Radan, *read* Radau, and between lines 9 and 10 from the bottom *insert* $x + x^2 = 0{\cdot}521\left(1 + \frac{\lambda}{1-x}\right)$, and for a circular orbit.

Page 201, line 32, *for* $x = 918000°$, *read* $x = 9{,}180{,}000°$.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *May* 11, 1860. No. 7.

The Rev. R. MAIN, President, in the Chair.

Murray Gladstone, Esq., Broughton, near Manchester;
Norman Robert Pogson, Esq., Director of the Hartwell Observatory; and
James Buckingham, Esq., Westmoreland House, Walworth Common,

were balloted for and duly elected Fellows of the Society.

Observations sur les Formules employées jusqu'à présent par les Géomètres et les Astronomes pour déterminer par la Théorie le Coefficient de l'Equation Séculaire du Moyen Mouvement Lunaire, et sur la Nécessité de le calculer désormais par des Formules Directes. Par M. G. de Pontécoulant.

Dans une note insérée dans les *Monthly Notices* de la Société Royale Astronomique de Londres (No. 9, vol. xix. 8 Juillet, 1859), et qui avait pour titre "Observations sur les Nouveaux Termes que M. Adams a proposé d'introduire dans l'expression du Coefficient de l'équation séculaire de la Lune," après avoir montré la nécessité de reprendre à nouveau tous les calculs qui avaient été faits pour déterminer *l'équation séculaire* du moyen mouvement lunaire, parceque ces calculs, déduits des formules de Laplace où l'on a pris pour variable indépendante la *longitude vraie* de notre satellite, ne donnaient pas *directement l'équation séculaire* de la *longitude moyenne* dans l'expression de la *longitude vraie* (qui est la seule cependant qu'il soit nécessaire de connaître pour la formation des tables astronomiques, ou pour la comparaison des anciennes

observations aux modernes), et qu'il était à craindre que la manière tout-à-fait arbitraire dont on déduit cette équation d'une formule inverse, ne laissât toujours échapper quelque partie (quelque minime qu'elle put être), de la quantité cherchée, comme il arrive, qu'on me permette cette comparaison grossière mais exacte, à un liquide qu'on transvase, d'un vase dans un autre, et qui perd toujours quelque chose dans cette opération, j'ai dit: "La détermination de *l'équation séculaire* par la formule directe sera encore plus laborieuse que par la méthode inverse, si l'on veut pousser l'approximation jusqu'au même ordre de quantités, mais l'importance de l'objet doit faire affronter toutes les difficultés *et si personne ne se présente pour entreprendre cet utile travail, je ne balancerai pas à m'en charger moi-même.*"

J'ai depuis cette époque profité de quelque loisir pour remplir l'engagement que j'avais pris avec les lecteurs des *Monthly Notices;* j'ai déterminé *l'équation séculaire* du moyen mouvement lunaire par une formule directe, déduite des équations différentielles du mouvement troublé sous la forme où je les ai employées dans ma théorie de la Lune,* c'est à dire en supposant qu'on a pris le temps pour la variable indépendante du problème, j'ai compris dans mon approximation des termes même que M. Plana n'avait point considérés, et je suis arrivé à une nouvelle expression du coefficient de l'équation séculaire du moyen mouvement, *très différente* de la sienne, comme je devais m'y attendre d'après ce que j'avais dit à la fin de ma note, mais qui doit mériter désormais la confiance des géomètres et des astronomes, puisqu'elle est la seule *jusqu'ici* qui résulte directement des formules du mouvement troublé, sous la forme généralement adoptée pour la détermination de toutes les inégalités planétaires, et enfin des principes d'une saine théorie.

Je vais exposer ici, aussi succinctement que possible, les résultats auxquels je suis parvenu, je donnerai tous les détails de mes calculs dans un mémoire qui s'imprime en ce moment, de sorte que chacun pourra aisément les vérifier, et qu'on parviendra à donner ainsi à l'un des éléments les plus essentiels de la théorie du mouvement lunaire, toute l'exactitude qu'exige son importance.

Designons par n le moyen mouvement de la lune pendant l'unité de temps dans son orbite réelle, par $n_,$ la même quantité relative à l'orbite elliptique, c'est à dire à l'orbite que la lune décrirait autour de la terre sans l'action du soleil, nommons a et $a_,$ les demi-grands axes correspondants qui s'en déduisent par la loi de Kepler, en sorte qu'on ait, par exemple, en prenant pour unité la somme des masses de la terre et de la lune, $a^3 n^2 = 1$ et $a_,^3 n_,^2 = 1$; la détermination du coefficient de *l'équation séculaire* exige que l'on connaisse le rapport de

* *Théorie Analytique du Système du Monde*, t. iv.

n à $n_{,}$, ou ce qui revient au même celui de a à $a_{,}$, c'est donc de la détermination analytique de ce rapport que nous devons d'abord nous occuper.

Pour faciliter cette recherche nous introduirons dans nos formules une nouvelle arbitraire a, que nous supposérons représenter la distance moyenne de la lune à la terre dans son orbite troublée, nous admettrons que cette quantité a été introduite à la place du demi-grand axe $a_{,}$, dans les formules même du mouvement elliptique, et que le développement de la fonction perturbatrice R, ou de la différence $\frac{d\mathrm{R}}{d\nu}$ a été effectué dans la même hypothèse, nous donnerons du reste le moyen de déduire ces nouveaux développements de ceux de ces mêmes fonctions donnés dans les chapitres ii et iii, du 7^{e} livre de la *Théorie Analytique;* cela posé, il sera facile de déterminer le rapport de a à $a_{,}$ et par suite celui de n à $n_{,}$, en désignant par n le moyen mouvement qui repond à l'arbitraire a ; en effet en représentant par r le rayon vecteur de la lune dans son orbite troublée et par R la fonction perturbatrice due à la seule action du soleil, on aura, par les formules ordinaires employées dans la théorie des perturbations planétaires *

$$\frac{d^{2}r^{2}}{2dt^{2}} - \frac{1}{r} + \frac{1}{a_{,}} = 2\int d\mathrm{R} + r\frac{d\mathrm{R}}{dr}.$$

Si l'on fait abstraction dans cette équation des termes périodiques, en observant que, d'après la signification que nous avons donnée à l'arbitraire a, on a alors $\frac{1}{r} = \frac{1}{a}$, on trouve

$$\frac{1}{a} = \frac{1}{a_{,}} - r\frac{d\mathrm{R}}{dr},$$

cette équation fera connaître le rapport de a à $a_{,}$ et si pour plus de simplicité, en observant que $r\frac{d\mathrm{R}}{dr} = 2\mathrm{R}$, on fait

$$p = 2a\mathrm{R}, \qquad (1)$$

on aura

$$\frac{a}{a_{,}} = 1 + p. \qquad (2)$$

Il suffira de substituer dans cette formule à la place de p sa valeur qu'on déduira immédiatement du développement de R, effectué comme il a été dit plus haut, pour en conclure la relation cherchée entre a et $a_{,}$.

Ceci admis, on a pour déterminer la longitude vraie ν de notre satellite, dans son orbite troublée, la formule

$$\frac{d\nu}{dt} = \frac{h}{r^{2}} + \frac{1}{r^{2}}\int\left(\frac{d\mathrm{R}}{d\nu}\right)dt.$$

* *Théorie Analytique*, liv. vii. No. 1.

Supposons le second membre de cette équation développé en une suite de *sinus* et de *cosinus* d'angles dépendants des longitudes moyennes nt et $n't$ de la lune et du soleil, ou de leurs multiples, et ne considérons que le premier terme, c'est à dire la partie constante de ce développement : admettons qu'on ait trouvé ainsi

$$\frac{dv}{dt} = \frac{h}{a^2}(1 + Q).$$

Ou bien—en substituant pour h sa valeur que nous supposerons de cette forme $h = \sqrt{a_,}\,(1+q)$, en representant par q une fonction déterminée des élémens e, e', γ, des orbites elliptiques de la lune et du soleil ordonnée par rapport aux puissances ascendantes de la très petite quantité m, qui représente le rapport des moyens mouvements du soleil et de la lune, et en observant qu'on a en faisant abstraction des inégalités périodiques $\frac{dv}{dt} = n$,—on pourra supposer

$$n = \frac{\sqrt{a_,}}{a^2}(1 + \Pi),$$

ou bien en substituant dans cette formule pour a^2 sa valeur tirée de l'équation (2),

$$n = a_,^{-\frac{3}{2}} \frac{(1 + \Pi)}{(1 + p)^2} \qquad (3)$$

formule dans laquelle il suffira de substituer pour p et Π leurs valeurs pour avoir le rapport cherché de n à $a_,^{-\frac{3}{2}}$, ou de n à $n_,$, élement essentiel pour la formation de *l'équation séculaire*.

En effectuant les opérations que nous ne pouvons qu'indiquer ici, mais dont le détail sera rapporté dans le mémoire que nous avons annoncé plus haut, et en portant les approximations jusqu'aux quantités du 8e ordre par rapport à m, nous avons trouvé

$$\begin{aligned} n = n_, \Big\{ & 1 - m^2 + \frac{471}{64} m^4 + \frac{679}{32} m^5 + \frac{3407}{96} m^6 + \frac{15163}{288} m^7 \\ & - e^2 \left(\frac{9}{8} m^2 + \frac{675}{32} m^3 + \frac{49293}{512} m^4 + \frac{700293}{2048} m^5 \right) \\ & + e'^2 \left(-\frac{3}{2} m^2 + \frac{5913}{128} m^4 + \frac{18333}{64} m^5 + \frac{1796123}{1536} m^6 + \frac{1715825}{384} m^7 \right) \\ & - e^2 e'^2 \left(\frac{27}{16} m^2 + \frac{825}{32} m^3 + \frac{2838199}{4096} m^4 + \frac{90396389}{8192} m^5 \right) \\ & + \gamma^2 \left(\frac{9}{8} m^2 - \frac{27}{32} m^3 - \frac{3915}{512} m^4 - \frac{22965}{2048} m^5 \right) \end{aligned}$$

$$+ e'^2\gamma^2\left(\frac{27}{16}m^2 - \frac{153}{32}m^3 - \frac{71901}{1024}m^4 - \frac{9015061}{65536}m^5\right)$$

$$+ e^4\left(-\frac{3}{32}m^2 + \frac{1665}{256}m^3\right)$$

$$+ e'^4\left(-\frac{15}{8}m^2 + \frac{1075675}{6144}m^4\right)$$

$$- \frac{9}{64}m^3e^4e'^2 + \frac{171}{64}m^3e^2e'^2\gamma^2 - \frac{99}{64}m^2e'^2\gamma^4$$

$$- \frac{a^2}{a'^2}\left(\frac{45}{64}m^2 + \frac{1035}{256}m^3 - \frac{1125}{512}m^2e'^2\right)\Big\}.$$

Si l'on compare l'expression précédente avec celle donnée par M. Plana dans la Théorie de la Lune, et qui résulte, comme nous l'avons dit, des formules inverses de celles que nous avons adoptées, c'est à dire des formules où la *longitude vraie* de la lune a été prise pour la variable indépendante, on voit que les valeurs de n déterminées par ces deux procédés diffèrent considérablement et même dans les parties les plus essentielles à la détermination exacte de *l'équation séculaire*. Ainsi, par exemple, le coefficient du terme multiplié par e'^2, dans l'expression du rapport $\frac{n}{n'}$, en négligeant les quantités de l'ordre m^5 et au-dessus, serait selon M. Plana

$$-e'^2\left(\frac{3}{2}m^2 - \frac{2763}{128}m^4\right)$$

au lieu de

$$-e'^2\left(\frac{3}{2}m^2 - \frac{5913}{128}m^4\right)$$

que nous avons trouvé pour son expression. La fraction qui multiplie le terme en m^4, n'aurait ainsi selon M. Plana que la moitié de sa valeur réelle, et *l'équation séculaire* qui en depend, éprouverait un accroissement de *plus d'une seconde* séxagésimale par suite de cette diminution. Cependant les calculs du savant Astronome de Turin sont d'une exactitude irréprochable, nous nous en sommes assuré en reprenant en entier la détermination de la valeur de n déduite des formules que l'on trouve dans le beau mémoire de Damoiseau,* et qui n'ont eu besoin que de très legères corrections et de quelques additions pour reproduire par leur développement cette quantité sous la forme même que M. Plana lui a attribuée. Il suit donc, du simple rapprochement que nous venons de faire, que les formules indirectes qu'avaient employées jusqu'ici les astronomes et les géomètres, qui ont traité la théorie de la lune et qui leur ont donné les expressions exactes de toutes les iné-

* *Mémoires de l'Institut, Savants Etrangers*, t. 1.

galités périodiques de cet astre, ainsi que celles des mouvements des nœuds et du périgée de son orbite, paraissent insuffisantes pour déterminer exactement le coefficient de *l'équation séculaire* ou du moins pour donner sa véritable expression analytique; soit que la méthode dont il s'agit ne soit pas en effet applicable à cette détermination, soit que l'hypothèse par laquelle on concède qu'on peut conclure le coefficient de *l'équation séculaire* de la *longitude moyenne* par le seul renversement de l'expression qui donne *l'équation séculaire* de la *longitude vraie* dans les formules où cette longitude a été prise pour la variable indépendante, soit trop arbitraire et contredite par les résultats d'une analyse rigoureuse.* Laplace qui ne s'était occupé que du terme de cette équation qui dépend de la première puissance de la force perturbatrice, ne s'était point aperçu de l'inconvénient que nous venons de signaler et que nous avions déjà rencontré dans la détermination de plusieurs inégalités à longues périodes; mais les géomètres et les astronomes, qui sont venus après lui, et qui ont poursuivi le développement de ses formules, ne se sont peut-être pas montrés assez scrupuleux en adoptant sans examen et comme hors de doute une supposition dont le principal mérite était de se plier sans peine aux besoins de leur analyse.†

Quant à M. Adams, dont les recherches sur *l'équation séculaire* ont vivement préoccupé les astronomes et les géomètres, dans ces derniers temps, par *l'étrangété* des résultats qu'il avait obtenus, comme il a suivi la même méthode que MM. Plana et Damoiseau, méthode dont nous venons de démontrer l'insuffisance, on peut être assuré d'avance que ses résultats ne sauraient s'accorder avec ceux que nous avons déduits des formules directes; mais les nouveaux termes, introduits par ce géomètre dans l'expression du coefficient de *l'équation séculaire*, par des considérations dont nous avons à plusieurs reprises signalé l'inexactitude, rendent la divergence bien plus sensible encore, elle ne va pas à moins de doubler et quelquefois de décupler la valeur de chacun des coefficients numériques qui entrent dans l'expression de n présentée plus haut. En sorte qu'on pourrait dire que la formule donnée par M. Adams n'offre avec la véritable formule d'où dépend *l'équation séculaire* aucun rapprochement possible; il suffira pour que

* Il y a dans la théorie du système du monde, disait Laplace, des choses qui échappent à toutes les prévisions du raisonnement et que le calcul seul peut nous apprendre.

† Il est à remarquer que Laplace dans son célèbre mémoire présenté le 19 Dec^{bre}, 1747, à l'Académie des Sciences de Paris, et où il a pour la première fois annoncé qu'il avait trouvé la cause tant cherchée de l'accélération séculaire du moyen mouvement lunaire, s'était servi d'une *formule directe* pour en déterminer le coefficient, sans doute pour mettre sa grande découverte à l'abri de toute objection. Malheureusement sa formule ne s'appliquant qu'aux termes du premier ordre, dans la *Mécanique Céleste*, liv. 7^{e}, il est revenu aux formules indirectes, mais sans pousser plus loin, comme nous l'avons dit, l'approximation.

chacun puisse s'en convaincre de déduire de la formule la valeur de n sous la forme dans laquelle nous l'avons présentée ; en n'ayant égard qu'aux termes multipliés par e'^2, les seuls que M. Adams ait encore calculés, ou du moins publiés (*Monthly Notices*, No. 6, tome xix.), on trouve ainsi

$$\frac{n}{n_,} = e'^2 \left(-\frac{3}{2} m^2 + \frac{4059}{64} m^4 + \frac{34047}{64} m^5 + \frac{280405}{96} m^6 + \frac{15161743}{1152} m^7 \right),$$

valeur qu'il suffira de rapprocher de celle que nous avons présentée plus haut, pour en juger la dissidence.

Supposons maintenant qu'on différentie l'expression de n trouvée plus haut, par rapport à l'excentricité e' de l'orbite terrestre qu'elle renferme, et dont les variations sont, comme on sait, la véritable cause de l'accélération du moyen mouvement lunaire, remarquée par les astronomes longtemps avant que les géomètres fussent parvenus à en démêler le principe physique ; en observant d'ailleurs que m est variable à raison de la variabilité du moyen mouvement n que cette quantité renferme, puisqu'on a en effet $m = \frac{n'}{n}$, ce qui donne

$$\frac{\delta n}{n} = -\frac{\delta m}{m},$$

on trouvera pour $\frac{\delta n}{n}$ une expression de cette forme

$$\frac{\delta n}{n} = K \delta . e'^2,$$

dans laquelle on fait pour abréger

$$\begin{aligned} K = {} & \frac{3}{2} m^2 - \frac{5337}{128} m^4 - \frac{18333}{64} m^5 - \frac{2073035}{1536} m^6 - \frac{2119151}{384} m^7 \\ & + e^2 \left(\frac{27}{16} m^2 + \frac{825}{32} m^3 + \frac{2879671}{4096} m^4 + \frac{92066789}{8192} m^5 \right) \\ & - \gamma^2 \left(\frac{27}{16} m^2 - \frac{153}{32} m^3 - \frac{61533}{1024} m^4 - \frac{10286869}{65536} m^5 \right) \\ & + \frac{27}{4} m^4 e'^2 + \frac{9}{24} m^2 e^4 - \frac{171}{64} m^2 e^2 \gamma^2 \\ & + \frac{99}{64} m^2 \gamma^2 - \frac{1125}{512} m^2 \left(\frac{a}{a'} \right)^2 \end{aligned}$$

En multipliant par $d t$ l'expression précédente et en l'intégrant ensuite, après avoir remplacé K par la valeur que cette

lettre représente, ou en a conclu *l'équation séculaire* $\int \delta n\,dt$ qui doit être ajoutée au moyen mouvement $\int n\,dt$ dans l'expression de la longitude vraie », exprimée en fonction de la longitude moyenne nt. On a trouvé ainsi

$$\int \delta n\,dt = \left\{ \begin{array}{l} \frac{3}{2}m^2 - \frac{5337}{128}m^4 - \frac{18333}{64}m^5 - \frac{2073035}{1536}m^6 - \frac{2119151}{384}m^7 \\ + e^2\left(\frac{27}{16}m^2 + \frac{825}{32}m^3 + \frac{2879671}{4096}m^4 + \frac{92066789}{8192}m^5\right) \\ - \gamma^2\left(\frac{27}{16}m^2 - \frac{153}{32}m^3 - \frac{61533}{1024}m^4 - \frac{10286869}{65536}m^5\right) \\ + \frac{27}{4}m^4e'^2 + \frac{9}{64}m^2e^4 - \frac{171}{64}m^3e^2\gamma^2 \\ + \frac{99}{64}m^3\gamma^4 - \frac{1125}{512}m^2\left(\frac{a}{a'}\right)^3 \end{array} \right\} \int (e'^2 - E'^2)\,n\,dt.$$

Dans cette formule E' représente l'excentricité de l'orbite terrestre au bout d'un temps quelconque t, e' la valeur de cette excentricité au commencement de l'année que l'on a choisie pour époque.

Cette formule réduite en nombres, a donné :—

Equation Séculaire de la Longitude moyenne de la Lune.

$$\int \delta n\,dt = \left\{ \begin{array}{l} 0{,}008392850\ (2) - 0{,}001353514\ (4) \\ - 0{,}000622120\ (5) - 0{,}000154749\ (6) \\ + 0{,}000009844\ (7) = 0{,}006272311 \end{array} \right\} \int (e'^2 - E'^2)\,n\,dt.$$

En comparant ce résultat à celui que M. Plana a déduit de ses formules, on voit qu'il en diffère d'une manière notable, et que l'espèce de compensation qui devait s'établir selon ce géomètre, entre les quantités du 4^e ordre et celles des ordres supérieurs, et qui semblait permettre de s'en tenir comme l'avait fait Laplace, aux termes résultant de la première approximation, n'existe pas réellement. La considération des puissances supérieures de la force perturbatrice altère sensiblement, au contraire, la valeur du coefficient qu'on obtient en faisant abstraction des quantités qui en dépendent, et comme tous les termes de la formule jusqu'aux termes du 7^e ordre, sont affectés d'un signe *négatif*, la grandeur du coefficient qu'on s'était habitué à supposer à *l'équation séculaire* d'après les indications de Laplace, doit être considérablement *diminuée*.

En effet, en substituant à la place de l'intégrale $\int (e'^2 - E'^2) n\,dt$ sa valeur $1274''{\cdot}185\,i^2$,* i représentant le nombre de siècles écoulés depuis l'année ou l'on compte le temps, *l'accélération séculaire* de la lune pendant le premier siècle, ou *l'équation séculaire* du moyen mouvement, se réduit à $7''{\cdot}99218$, c'est à dire que cette équation se trouvera diminuée de trois secondes à peu près de la valeur qu'on s'était habitué à lui attribuer.

Au reste il faut remarquer que les astronomes sont loin d'être d'accord entre eux sur l'évaluation exacte de cet élément si important du mouvement lunaire. Dunthorne, qui le premier a tenté de le déterminer par la comparaison des anciennes observations aux modernes, le faisait de $10''{\cdot}0112$; Mayer l'a réduit à $6''{\cdot}998$ dans ses premières tables ; il l'a porté à $9''{\cdot}0072$ dans ses dernières. Lalande par une discussion nouvelle de cet objet a été conduit au résultat de Dunthorne. Enfin Laplace dit que Bouvard par une nouvelle discussion des éclipses anciennes déjà commes et de celles qu'il a extraites d'un manuscrit arabe d'Ibn Iunis, a trouvé un résultat qui diffère peu de celui que lui-même avait obtenu de la théorie, c'est-à-dire de $10''{\cdot}18162$ (nous ne connaissons pas exactement le résultat de Bouvard). On voit que le coefficient que nous avons déduit pour *la première fois* d'une analyse *directe* et *rigoureuse*, tient le milieu entre les deux déterminations de Mayer ; il serait donc possible qu'une nouvelle discussion des anciennes éclipses, fit définitivement pencher la balance du coté de cet habile astronome et rétablit sur ce point important du système du monde, l'exacte concordance, toujours si désirable, entre la théorie et l'observation.

N. B.—Il résulte comme une conséquence désormais *incontestable* de notre analyse que la méthode jusqu'ici employée par les géomètres et les astronomes pour déterminer par la théorie le coefficient de l'équation séculaire du moyen mouvement lunaire, tendait à augmenter de $2''{\cdot}50$ à peu près le coefficient de cette inégalité ; on doit donc accueillir avec une extrême réserve toutes les évaluations fondées sur l'emploi de ces formules défectueuses. C'est ainsi que le coefficient $12''{\cdot}120$ adopté par M. Hansen dans ses nouvelles tables lunaires, est évidemment trop fort de $3''$ *au moins*, ce qui s' explique aisément, puisque ce coefficient est deduit, sans aucun doute, des anciennes formules de Laplace dont nous avons démontré l'insuffisance pour une détermination si délicate ; mais je ferai d'ailleurs une remarque sur le procédé même par lequel M. Hansen dit avoir déduit ce coefficient numérique de ses formules analytiques. Il est évident par les résultats obtenus par M. Plana, et par le dévéloppement des formules de Damoiseau que j'ai complétement effectué, comme je l'ai dit dans un passage de ma note, que la correction que la considération des termes dépendants

* *Théorie Analytique*, Livre 7, No. 135.

du carré et des puissances supérieures de la force perturbatrice, fait subir au coefficient de *l'équation séculaire*, est affectée d'un signe négatif; nous sommes parvenu par l'analyse précédente au même résultat; le coefficient de *l'équation séculaire* de quelque manière qu'on envisage la question et quelle que soit l'importance de l'altération qu'on lui fera subir, devra donc toujours tomber *audessous* de 10″·70, valeur qui lui serait attribuée par la première approximation où l'on n'a considéré que les termes dépendants de la première puissance de la force perturbatrice. D'après cela les trois valeurs donnés par M. Hansen dans le No. 7, vol. xix. des *Monthly Notices*, pour le coefficient de *l'équation séculaire* savoir 11″·93; 11″·47, et 12″·12, qu'il dit avoir obtenus par trois procédés différents, et dont la dernière est celle qu'il a employée dans les Tables de la Lune, sont donc également suspectes d'erreur, puisqu'elles tombent toutes trois audelà de la limite fixée. M. Damoiseau, dont j'ai développé la formule comme je l'ai dit plus haut, ce qui m'avait conduit comme M. Plana à une correction négative, a trouvé le coefficient déjà un peu trop fort 10″·7232 (tandisque d'après les données employées par cet astronome, ce coefficient devait tomber audessous de la limite 10″·196) il est évident que cette anomalie ne peut tenir qu'à la méthode des substitutions successives que ces deux astronomes ont employée pour résoudre leurs équations de condition, et en déterminer les arbitraires, méthode dont l'honorable M. Main a si judicieusement signalé naguère tous les inconvénients.* La méthode des développements en séries, plus pénible peut-être mais plus analytique, me semble offrir beaucoup plus de sécurité, et je ne partage nullement les idées de M. Hansen à cet égard†, elle se prête à une révision continuelle des formules employées et l'exactitude d'ailleurs avec laquelle elle reproduit les coefficients de toutes les inégalités *périodiques*, à *longues périodes*, ou *séculaires* du mouvement lunaire, et donne des résultats parfaitement concordants avec ceux qu'on déduit des autres méthodes ou de l'observation, montre que l'objection qu'on avait voulu tirer contre elle de l'incertitude qui peut exister sur la convergence des séries est, du moins dans le cas général, sans fondement et sans valeur; c'était aussi à cette opinion qu'était revenu Laplace après une longue étude de toutes les méthodes employées pour résoudre les différents problèmes de la théorie de la lune, comme on peut le voir par ce qu'il dit dans un mémoire inséré dans la *Connaissance des Temps* pour 1823 (Voir page 229).

* *Monthly Notices of the Royal Astronomical Society*, vol. xx. No. 4.
† Idem, vol. xix. No. 7.

*Postscript to the "Reply to various Objections which have been brought against his Theory of the Secular Acceleration of the Moon's Mean Motion."** By J. C. Adams, M.A., Lowndean Professor of Astronomy and Geometry, Cambridge.

In the *Compte Rendu* of April 9, 1860, which has appeared since the foregoing paper was read, M. de Pontécoulant gives the value of the secular acceleration of the moon's mean motion, which he has obtained by taking the time as the independent variable, and which he considers to be "désormais à l'abri de toute objection."

This result, however, of M. de Pontécoulant's is the same as that which he formerly communicated to me, the error of which I have already pointed out.

M. de Pontécoulant thus describes his method, "En développant la formule qui donne l'expression de la longitude vraie en fonction de la longitude moyenne, et en n'ayant égard qu'au premier terme de ce développement, c'est-à-dire à sa partie non-périodique j'en ai conclu le rapport du moyen mouvement de la lune dans son orbite troublée au moyen mouvement relatif à son orbite elliptique, c'est-à-dire à l'orbite que cet astre décrirait autour de la terre sans l'action du soleil. . . . En différentiant ensuite cette valeur par rapport à l'excentricité e' de l'orbite terrestre qu'elle renferme, . . . j'ai obtenu une expression de cette forme :

$$\frac{\delta n}{n} = \mathrm{H}\,\delta . e'^{2} . t$$

The value of H thus obtained is

$$\mathrm{H} = -\frac{3}{2}m^{2} + \frac{5337}{128}m^{4}$$

which, as I have shown in p. 232, is the result that would be found by differentiating the relation between n and a, and then neglecting the variation of a. The fallacy of M. de Pontécoulant's reasoning consists in his treating the moon's "orbite elliptique, c'est-à-dire, l'orbite que cet astre décrirait autour de la terre sans l'action du soleil," as if it were a real elliptic orbit with an unalterable semi-axis major, whereas the semi-axis major of the elliptic orbit spoken of by M. de Pontécoulant, which is the same quantity as that above denoted by the symbol a, is really variable, and its variation must be found by means of the differential equations in the way which I have before described.

The numerical value of the coefficient of the secular equation which M. de Pontécoulant obtains in this paper,

* For the "Reply, &c." see the preceding number of the *Monthly Notices*. The paper in the *Compte Rendu* of April 9, 1860, is to the same effect as the foregoing paper by M. de Pontécoulant, but the method is less fully explained, and the development is not carried quite so far; the letter H is there used in the place of the K of the foregoing paper.

when reduced so as to correspond with the value $-1270'' t^2$ of the integral $\int (e'^2 - E'^2)\, n\, dt$ is

$$7''.96$$

which, as we see, differs widely from the similarly reduced values of the coefficient according to the theories of M. Plana and M. Hansen, given in p. 236, as well as from the values obtained by M. Delaunay and myself.

After giving his formula for the secular equation, M. de Pontécoulant remarks, "En comparant ce résultat à celui que M. Plana a déduit de ses formules, on voit qu'il en diffère d'une manière notable, et que l'espèce de compensation qui devait s'établir, selon ce géomètre, entre les quantités du quatrième ordre et celles des ordres supérieurs, et qui semblait permettre de s'en tenir, comme l'avait fait Laplace, aux termes résultans de la première approximation, n'existe pas réellement. La considération des puissances supérieures de la force perturbatrice altère sensiblement, au contraire, la valeur du coefficient qu'on obtient en faisant abstraction des quantités qui en dépendent, et comme tous les termes de la formule, jusqu'aux termes du septième ordre, sont affectés d'un signe négatif, la grandeur du coefficient qu'on s'était habitué à supposer à l'équation séculaire d'après les indications de Laplace, doit être considérablement diminuée."

It is needless for me to point out how totally inconsistent these remarks of M. de Pontécoulant are with the conclusion at which he arrives in his paper in the *Monthly Notices*, "Il résulte, je pense, sans *contestation possible désormais*, de la discussion précédente, qui les formules employées jusqu'ici pour déterminer l'équation séculaire de la lune, ont toute la correction nécessaire à cet important objet."

On the Early Morning Illumination of a portion of the Mare Imbrium contiguous to Plato, and designated Newton by Schröter, but unnamed in Beer and Mädler's Map. By W. R. Birt, Esq.

This portion of the *Mare Imbrium* of an elliptical form, just to the south of *Plato*, and about the same size as that crater, presents under the early morning illumination all the characteristics of a plain surrounded at some former period by a *ring*, some few portions of which still remain.

On the evening of April 28, 1860, I had an opportunity of observing the progression of the terminator over at least half the length of the plain, viz., from 0·25 reckoned from the western extremity at $7^h\ 45^m$ G.M.T. to 0·75, also reckoned from the same extremity at $9^h\ 15^m$ G.M.T. approximate in both cases, the major axis of the ellipse, viz., from west to east $= 1{\cdot}0$.

During the [redacted] the observations the northern part of

the plain adjoining *Plato* was in shadow from a high mountain-peak and from smaller masses filling the angle between the rims of the plain and *Plato*.

Various points of light beyond or on the night side of the terminator were so disposed that the ring could be distinctly traced. On the north-east part of this imperfect ring a strong and somewhat large point of light, with a *double* summit, stood out boldly, a minute point existing between the mountain mass and *Plato*. On the south, and forming part of the ring, *Pico* was exceedingly brilliant, its shadow being scarcely discernible, its summit appeared to be double. A minute point of light eastward of *Pico* and situated on the apparent rim was seen.

On the south-west and west the rim appeared to be continuous but very low, and exhibited much of the character of the *soft ridges* so frequent on the *Maria*. Two ridges extend from this part of the rim, one towards the crater A, Beer and Mädler's map; the other towards the mountain mass B on the same map, this ridge passes between the mountain mass and *Pico*.

The floor of this plain has the appearance of being a portion of, and is evidently at the same level as the general surface of of the *Mare Imbrium* to the south, it is however *under this illumination* slightly darker than the surface of the contiguous portion of the *Mare:* its colour appeared to be *greenish*, the general appearance being very similar to that of most of the *Maria* when very near the terminator. I have frequently observed the surfaces of the *Maria* to present a peculiar greenish tint when very obliquely illuminated, becoming *darker* the nearer they are to the edge of the shadow; this of course is the effect of the oblique incidence, it is however under such an illumination that the *green* tint is most perceptible. I have thus seen it in the *Mare Crisium*, *Mare Serenitatis*, *Mare Nectaris*, *Mare Humorum*, and others, while in a further advanced stage of the lunar day a more decidedly *greyish* tint creeps over the same surface, interspersed with numerous lighter patches giving the surfaces of the *Maria* a generally *unsettled appearance*.

Two minute points, apparently craters, were seen in the western area of the floor, they appeared to rise abruptly and nearly perpendicularly from the surface, as if they had been *surrounded* by the material forming the surface of the plain at a period subsequent to that of the formation of the craters. Several craters in the *Maria* present this feature. A very minute point of light on the floor near *Pico* may possibly be of a similar nature.

The fact that the *ring form* of this part of the *Mare Imbrium* can only be seen at early morning or just before sunset, combined with the circumstance that numerous formations of a somewhat similar character occur in various localities, mostly on the surfaces and near the borders of the *Maria*, invest all such formations with a high degree of interest. The m[illegible]

difference existing between them as a class and the well-known craters *Plato*, *Tycho*, *Copernicus*, *Kepler* and others, seems to point, not so much to a difference of origin as to a remarkable modification which they may have undergone at some period of their history, which appears in some way to be connected with their proximity to the darker and mostly lower plains known as *Maria*.

The *greenish* tint above alluded to as being characteristic of the lunar plains is strikingly in contrast with the brilliantly *white* illumination of all mountain masses. *Pico* and the mountain on the north-east of the rim, which I propose designating by the letter S for future reference, show most brilliantly in the midst of the green tint of the plain.

Mr. Carrington exhibited a map on which were laid down the maxima and minima of frequency of the solar spots between the years 1740 and 1860. The ordinates were expressed throughout in the relative numbers of Prof. Wolf, the nature of which was briefly explained, and the principal data were taken from the *Mittheilungen* of the same astronomer. The series of numbers derived from the observations of Hofrath Schwabe and of Prof. Wolf was taken as the standard of the latter portion, and extended backwards to 1813 by the records of Stark, of Tevel, and of Mr. Adams, of Edmonton. For the latter half of the eighteenth century the series obtained from the observations of Staudacher, of Nuremberg, was taken as the standard; and it was explained how the shorter series of Oriani, Mallet, Horrebow, Zucchoni, and De Hagen, were employed as corroborating and further extending the conclusions. The principal gap was shown to lie between the years 1798 and 1813, being nearly coincident with the wars of the first French Republic and Empire,—a period during which no observations of any continuity have yet been found. Mr. Carrington explained the grounds on which Prof. Wolf was led to conclude that one maximum, about the years 1803 and 1804, preceded by a long-continued minimum, had alone occurred in this interval; and stated that, having generally examined the grounds for this result, he was inclined to concur in its correctness. The curve drawn through the summits of the ordinates showed in the clearest manner the variability of the period from minimum to minimum, while ocular inspection served to show that its mean duration was about eleven years. The height of the ordinates laid down indicated that, on the whole, the summits representing maxima were less elevated for the latter half of the eighteenth century than for the first half of the present; and it was remarked that this might be attributable to the use of less powerful telescopes during the former period, or to more careful enumeration in the latter,

though no means apparently existed of deciding if the differences were real. Mr. Carrington named the two principal hypotheses which had been thrown out for the origin of the periodicity exhibited; but remarked that neither of them was, in his opinion, satisfactory, and that the curve laid before the meeting was still a grand riddle to be read, which deserved the consideration of all astronomers whose attention was directed to the subject of Solar Physics.

Notes on certain Variable Stars of Unknown or Doubtful Periods. By Norman R. Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

First approximations, — whether to a stellar parallax, a proper motion, the orbit of a comet, planet, or binary star, or to the period of a variable star, — are ever interesting; and I therefore take leave to forward a few of the latter class, arrived at from a cursory inspection of the unreduced records in my variable-star note-book; which, though liable to be slightly modified by future projection and discussion, are, nevertheless, sufficiently near for present purposes.

I will here remark, that, in compliance with a valuable suggestion of Sir John Herschel's, I now employ "*Julian dates*," agreeably to the table on page 679 of the new edition of his *Outlines of Astronomy*, for a time co-ordinate, instead of "*Day of century*." By this means the interval in days, between observations centuries apart, is found by a simple subtraction, while convenient tables are easily constructed, by which to change ordinary years, months, and days into seven-figure Julian dates. As the Julian date 2400000 corresponds to 1858, Nov. 16, by omitting the initial figures 24, five figures will suffice for a time record until the year 2131.

V *Virginis*. — This interesting star, discovered as a 7th magnitude by M. Hermann Goldschmidt, in May 1857, was stated in my *Ephemeris of the Variable Stars for* 1860 (page 42) to be due at a maximum, "*possibly in March and November*." It attained its greatest brightness in the middle of February, a little above the 9th magnitude. In June 1859 it did not exceed the 10·5 magnitude. The greatest brilliancy is, therefore, unsteady, but for provisional elements we may employ the formula,

Maximum = 1860, Feb. 19 + *n* . 252 days.

R *Libræ*.—This star, announced as variable in the *Monthly Notices*, vol. xviii. p. 283, recently reappeared and passed a faint maximum (about 10·3 magnitude) towards the end of April. The interval elapsed since the previous maximum is 722 days, or nearly two years, and from frequent records of

invisibility, this must be one entire period, and no multiple of a shorter time! The star is only visible for about a twelfth part of its period. In consequence of obtaining this first approximation to the elements of R *Libræ*, I have the pleasure to state, that not one of my ten variable stars now remains indeterminate in epoch or period!

R *and* S *Scorpii*.—These highly interesting adjacent variable stars, mapped by Admiral Smyth in 1837 and 1839, when engaged upon his *Celestial Cycle*, and found to be variable by M. Chacornac in 1853 and 1854 respectively, have each reappeared considerably earlier than was to have been expected from the scanty data available for future prediction. R has just passed its maximum; S was seen brightest at the end of February. Neither star exceeded the 11·5 magnitude, and S has now completely vanished. The positions of these objects have hitherto been so imperfectly known, and so unfortunately mystified by accidental errata, that I was glad to obtain some ring-micrometer comparisons of each with a neighbouring star on March 18. Both variables were then of the 12th magnitude; R increasing; S diminishing; and from their low altitudes anything but easy objects to observe, even with a dark field and the 84 power ring. Their places were,—

	Mean R.A. 1860.	Mean N.P.D. 1860.
	h m s	° ′ ″
R	16 9 19·13	112 35 11·6
S	16 9 20·22	112 32 59·5

The assumed position of the comparison star for the same epoch was deduced from Argelander's Zones, 213,19, 8·5 mag. = 382,12, 9 mag. = 386,6, 8 mag.; the mean of the three giving —

	h m s	° ′ ″
8·5	16 8 54·60	112 34 13·3

U *Geminorum*, after failing to appear on March 13 when due, has just shone forth nearly half a period behind time; but brightly and briefly as ever. It was well watched by Mr. Baxendell at Manchester, and also at Hartwell. Doubtless there will yet be found a compound periodicity even in these seeming irregularities.

It may save needless trouble and disappointment to remind observers that no complete maxima of R or S *Ophiuchi*, R *Sagittarii*, or U *Capricorni*, are obtainable this year; their approximate or provisional elements being as follows:—

	Epoch of Maximum.	Julian Date.	Period in Days.
S Ophiuchi	1859, May 31	2400196	232
R Ophiuchi	1858, May 11	2399811	301
R Sagittarii	1859, Oct. 27	2400345	467
U Capricorni	1859, Dec. 22	2400401	420

In a future communication I shall request leave to propose a new and simple nomenclature for the variable stars, which is inexhaustible, and which has already received the unanimous approbation of several of the highest authorities in England.

Hartwell Observatory, May 10, 1860.

On the supposed Variability of the closest Companion of σ Orionis. By the Rev. W. R. Dawes.

When so acute an observer as Sir William Herschel overlooked an object which has since been seen with little difficulty, with the same or even less powerful means, we have at once some ground to suspect variability of light; and it becomes a matter of interest to decide the question. With reference, therefore, to the smallest and closest star of the quadruple group constituting *σ Orionis*, I beg to lay before the Society an extract or two from my astronomical note-books bearing on the point.

In the year 1829, while I was residing at Ormskirk, and previously to the erection of my 5-foot equatoreal, Mr. Lassell lent me a 7-foot Newtonian reflector of his own making, having an aperture of $7\frac{1}{4}$ inches, which I generally reduced to $6\frac{1}{4}$. With this instrument the observation is made which is thus recorded:

"1829, Dec. 15, *σ Orionis*, on meridian with a new negative eye-piece magnifying about 300 times, and aperture $6\frac{1}{4}$ inches, a *new star* was immediately recognised near *σ*. It is closer than the nearest of the bright triple set, and much fainter; but becomes far more positive with high powers. The negative power 360 showed it better still; and the highest, 600, rendered it very conspicuous. It was even steadily visible with *five inches aperture*. With $6\frac{1}{4}$ and power 120 it was barely visible —only by glimpses; with 180, just steady. But it was neat and steady with concave eye-glasses magnifying 140 and 250. If this new star had formerly been as bright, I conceive it would have been detected by myself in my former observations, if not by South with his 7-foot refractor [of 5 inches aperture]. At any rate Sir W. Herschel would have seen it."

It was not until Sir John Herschel presented me with Struve's large Dorpat Catalogue in 1833 that I was aware that this star had been seen by any other observer.

With my 5-foot refractor I obtained the following measures of position and distance, (see *Mem. R.A.S.*, vol. viii., p. 79),

$$1831{\cdot}09, \quad P = 234^{\circ}{\cdot}84; \quad D = 11''{\cdot}478.$$

In the notes, I have remarked, "Bears a moderate illumi-

nation." . . . "Is seen better for a little illumination, but does not bear magnifying." . . . "Much better seen with 140 than with 226."

Subsequently I saw it well with a much smaller telescope, viz., a very fine 43-inch refractor of 2·7 inches aperture, by Peter Dollond, and have recorded as follows:—

"1843, Oct. 26. With 200 and 130 the smallest companion was occasionally discernible. I think that in fine circumstances and near the meridian it would be pretty steadily visible, perhaps more so with a lower power.

"Oct. 28. Very bad night. With power 200, the smallest companion occasionally visible. Power 77, it is sometimes steadily seen nearly throughout the field of view. I am surprised to see it so well on so indifferent a night."

With the larger telescopes I have since used it has always been visible; and any diminution in its apparent brightness I have attributed to the combined effect of its moderate altitude and varying atmospheric circumstances acting unfavourably on its delicately pale light. Yet the evidence taken altogether is perhaps sufficiently strong to warrant its being placed upon the list of *suspected* variables.

Observatory, Haddenham, Bucks,
1860, *May* 10.

A Catalogue of the Positions and Distances of 398 *Double Stars.* By Lord Wrottesley.

The following is extracted from the introductory observations which accompanied the Catalogue:—

"The positions and distances of the stars contained in the subjoined catalogue are the results of observations made by my assistants, Messrs. Simms, Philpott, and Frederick Morton, but the far greater number of the stars have been observed since the beginning of the year 1854 by Mr. Morton, to whose industry, ability, and skill I can bear ample testimony. He is a good observer and an accurate computer.

"The telescope employed is the equatoreal mentioned in my former communication, *Memoirs*, vol. xxiii., as forming part of the furniture of my observatory. It is 10 feet 9 inches in focal length, with a clear aperture of 7¼ inches; the mounting is in the usual English form; the declination and hour-circles are each 3 feet in diameter, and their verniers read to 10″ in space and to seconds of time. A wire micrometer only has been employed, the screw-heads of which are divided into 100 parts, the value of each of which is 0″·1564, and the position circle of 4 inches diameter is graduated in silver with a vernier

The Astronomer Royal has received from Lord John Russell, Principal Secretary of State for Foreign Affairs, the following translation of a Letter, relating to the admission into Spain of astronomical, meteorological, and photographical apparatus, for the observation of the Total Eclipse of 1860, July 18, addressed to his Lordship by Don Xavier e Isturiz, Ambassador of the Queen of Spain:—

"*Spanish Legation, London, May* 31, 1860.

"My Lord,—The Government of my Queen, my august Sovereign, transmitted to me, on the 12th inst., a despatch, of which the following is a copy:—

"'With the object of enabling foreign astronomers to study with facility in Spain the total eclipse of the sun, which will take place on the 18th of July next, her Majesty's Government has determined that all of them shall be allowed to bring into the kingdom free of import duties the apparatus or instruments they desire to have; such as one or two chronometers; a large telescope on a stand, which may be either refractor or one with a mirror instead of a lens; a theodolite or sextant; photographic apparatus; meteorological instruments, such as a barometer, thermometer, &c.; each importer proving his quality as a foreigner by means of his passports, and giving security for the amount of the import duty of the said instruments until their re-exportation; the Custom-houses of Santander, Bilboa, San Sebastian, Yrun, Eligondo, La Junguera, Barcelona, Tarragona, Castellon, and Valencia, being empowered for the introduction of the same. Instructions also have been given to the Governors of those provinces in which that phenomenon will be most remarkable, that they afford to the said astronomers all the co-operation and assistance in their power to carry their observations into effect, and that they cause the same to be done by the Alcaldes and other functionaries and servants of the Administration in the places of their respective territory. The Rectors also of the Universities of Valladolid, Oviedo, Valencia, Barcelona, and Saragossa, have been directed to appoint, each of them, a Professor of the faculty of exact sciences, who shall furnish the astronomers and scientific commissions with whatever information they may want in reference to the means of best carrying their observations into effect.

"'I communicate the above to your Excellency by Royal Order, that you may acquaint the British Government therewith, in order that it may be made known to learned men and scientific bodies of that country what are the principal measures adopted by her Majesty's Government for the abovementioned purpose.'

"I have the honour to bring the above to your Excellency's knowledge, in order that you may be pleased to make the same known to the persons or corporations interested in this matter.

"I have, &c.

"Xavier e Isturiz.

"*The Lord John Russell.*"

reading to 6′ in space. The power generally employed was 450, but one of 320 was often used, and occasionally for close stars 600 or 820.

"The telescope is driven by clock-work. Notwithstanding some drawbacks it is quite capable of contending with others of its own class, and in a favourable and calm state of the atmosphere its power of showing faint companions leaves nothing to be desired. The working catalogue of stars for observation was selected from Struve's great Catalogue of 1837; and contained, 1st, stars confessedly binary; 2nd, others as to which there was strong presumptive evidence of their being so; 3d, stars of which the observations by different observers gave anomalous or discordant results. As these objects were worked out, more were added from time to time from among the finer and closer of the double stars, and from those which had been wholly observed at one epoch. Some of these latter exhibit orbital motion.

"The first of the catalogues contains the results of the observations of each star made on separate nights, the second the general mean of all the sets obtained in the same year. The catalogues are followed by notes, directing attention to any particulars worthy of remark, in reference to any star, but no notes have been made to such stars as *Castor*, *γ Virginis*, &c., which have received due attention from other observers, unless the fresh evidence seemed to require it."

Apparent Right Ascensions of the Moon's Limb and of Moon-Culminating Stars, observed with the Transit Instrument of the Observatory, Washington.

(*Communicated by Lieut. M. F. Maury, U. S. N.*)

Date. 1859.	Object.	No. of Threads.	Apparent Right Ascension. Observed. h m s	Computed. s
Jan. 12	101 Piscium	10	1 28 14·892	
	Moon's 1st Limb	15	1 45 49·826	49·95
	β Arietis	10	1 46 52·040	
	B.A.C. 607	25	1 51 47·175	
13	ι Arietis	15	1 49 39·713	
	α Arietis	15	1 59 14·583	
	θ Arietis	15	2 10 18·057	
	Moon's 1st Limb	14	2 41 6·533	6·36
15	α Tauri	15	4 27 51·662	
	τ Tauri	13	4 33 48·903	
	Moon's 1st Limb	25	4 48 59·266	59·25
17	α Aurigæ	5	6 6 25·668	

Date. 1859.	Object.	No. of Threads.	Apparent Right Ascension. Observed. h m s	Computed. s
Jan. 17	48 Aurigæ	15	6 19 32·550	
	49 Aurigæ	15	6 26 21·439	
	Moon's 1st Limb	25	7 10 39·788	40·37
	ν Geminorum	25	7 27 16·265	
	κ Geminorum	15	7 35 58·262	
18	ν Geminorum	15	7 27 16·171	
	κ Geminorum	15	7 35 58·273	
	λ Cancri	15	8 12 11·224	
	Moon's 2d Limb	15	8 21 4·219	4·09
	π Cancri	15	8 24 35·350	
	γ Cancri	15	8 35 9·650	
19	π Cancri	25	8 24 35·345	
	γ Cancri	15	8 35 9·623	
	δ Cancri	15	8 36 42·283	
	83 Cancri	25	9 11 8·642	
	Moon's 2d Limb	25	9 23 49·105	48·76
	ρ Leonis	25	9 50 40·135	
	π Leonis	25	9 59 40·571	
Feb. 10	ζ Arietis	15	3 6 48·788	
	σ' Arietis	15	3 13 6·255	
	Moon's 1st Limb	15	3 17 41·175	41·07
	11 Tauri	15	3 32 22·348	
	η Tauri	15	3 39 7·529	
14	53 Geminorum	25	7 7 11·095	
	ι Geminorum	25	7 17 0·460	
	Moon's 1st Limb	25	7 43 14·466	14·53
	ψ^2 Cancri	5	8 1 59·905	
	χ Cancri	5	8 12 11·403	
	λ Cancri	15	8 35 9·777	
16	π^2 Cancri	25	9 7 29·047	
	83 Cancri	5	9 11 8·916	
	ξ Leonis	10	9 24 23·004	
	Moon's 1st Limb	25	9 47 59·392	59·74
	α Leonis	25	10 0 54·043	
Mar. 12	Moon's 1st Limb	15	6 9 47·027	47·02
	μ Geminorum	15	6 14 27·436	
	49 Aurigæ	15	6 26 20·932	
16	B.A.C. 3345	25	9 40 0·649	
	ρ Leonis	25	9 51 40·571	
	α Leonis	25	10 0 54·071	
	Moon's 1st Limb	25	10 16 36·397	36·32
	45 Leonis	5	10 20 14·268	
	ϱ Leonis	25	10 25 25·550	

Date. 1859.	Object.	No. of Threads.	Apparent Right Ascension. Observed. h m s	Computed. s
April 9	Moon's 1st Limb	15	6 55 39·004	39·30
	ι Geminorum	15	7 16 59·644	
	υ Geminorum	15	7 27 15·542	
12	ν Leonis	15	9 50 40·339	
	Moon's 1st Limb	15	9 55 10·728	11·01
	α Leonis	15	10 0 53·860	
16	ψ Virginis	5	12 47 3·861	
	γ Virginis	15	13 0 33·358	
	α Virginis	15	13 17 48·727	
	69 Virginis	15	13 19 58·843	
	Moon's 1st Limb	15	13 22 58·256	58·30
May 12	Moon's 1st Limb	15	12 12 48·099	48·14
	f Virginis	14	12 29 34·129	
	28 Virginis	13	12 34 42·768	
13	*f* Virginis	25	12 29 34·144	
	28 Virginis	25	12 34 42·735	
	Moon's 1st Limb	25	13 3 10·249	10·12
	58 Virginis	20	13 0 6·724	
	α Virginis	25	13 17 48·750	
June 9	η Virginis	25	12 12 43·725	
	Moon's 1st Limb	15	12 47 16·339	16·55
	58 Virginis	15	13 10 6·589	
	α Virginis	15	13 17 48·712	
11	86 Virginis	15	13 38 28·352	
	89 Virginis	15	13 42 15·539	
	Moon's 1st Limb	15	14 30 11·095	11·28
	20 Libræ	15	14 55 52·662	
	l' Libræ	15	15 4 14·497	
	β′ Scorpii	25	15 57 17·988	
17	h^2 Sagittarii	25	19 28 11·082	
	f Sagittarii	25	19 38 11·556	
	Moon's 2d Limb	25	20 4 50·706	50·59
	σ Capricorni	25	20 11 18·651	
	ζ Capricorni	25	20 20 52·188	
July 9	a^2 Libræ	25	14 43 7·889	
	20 Libræ	15	14 55 52·545	
	l' Libræ	15	15 4 14·475	
	Moon's 1st Limb	25	15 7 30·455	30·59
	39 Libræ	25	15 28 31·729	
	42 Libræ	15	15 32 0·517	
	λ Libræ	25	15 45 12·597	
11	Moon's 1st Limb	25	16 59 37·512	37·52
	A′ Ophiuchi	25	17 6 44·825	

Date. 1860.	Object.	No. of Threads.	Apparent Right Ascension. Observed. h m s	Computed. s
July 11	θ Ophiuchi	25	17 13 25·129	
	d Ophiuchi	25	17 18 25·379	
12	θ Ophiuchi	10	17 13 25·110	
	d Ophiuchi	5	17 18 25·330	
	Moon's 1st Limb	25	17 56 22·386	22·59
	μ' Sagittarii	25	18 5 23·944	
	ζ Sagittarii	5	18 12 2·365	
	λ Sagittarii	20	18 29 20·365	

The clock-stars used are those of the *British Nautical Almanac* for the mean equinox of 1860·0.

The moon's computed place is taken from the *American Ephemeris*.

Results of Meridional Observations of Small Planets, and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, in the month of April, 1860.

(*Communicated by the Astronomer Royal.*)

Iris (7).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, April 2	8 15 40·9	9 1 14·27	80 35 31·28
3	8 11 57·9	9 1 27·20	80 34 8·18
5	8 4 36·7	9 1 57·89	80 31 44·99
7	7 57 21·3	9 2 34·49	80 29 41·18

Hygeia (10)

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, April 12	9 28 30·2	10 53 41·10	88 22 2·22
17	9 7 23·4	10 52 13·57	88 8 27·89
28	8 22 52·0	10 50 56·98	87 48 46·18

Victoria (12).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, April 2	12 10 53·0	12 57 5·04	105 58 27·87
9	11 36 47·7	12 50 30·00	104 58 47·74
16	11 2 51·2	12 44 3·77	103 52 56·13
17	10 58 2·2	12 43 10·57	103 43 11·49
27	10 10 50·2	12 35 16·37	102 5 33·61

Egeria (13).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, April 2	8 7 42·2	8 53 14·31	50 49 11·73
7	7 50 33·0	8 55 45·06	51 45 6·95

Thetis (17).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, April 16	12 53 14·5	14 34 45·23	94 36 36·72
21	12 29 26·9	14 30 36·54	94 9 38·73
28	11 55 47·9	14 24 27·89	93 35 31·25

Urania (30).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, April 12	9 29 35·2	10 54 46·30	85 36 53·02
16	9 12 27·5	10 53 21·99	85 27 13·57
17	9 8 14·4	10 53 4·77	85 25 8·13

Ariadne (43).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, April 7	9 10 44·9	10 16 10·11	85 7 40·39
10	8 58 5·9	10 15 18·74	84 57 12·59
17	8 29 45·2	10 14 29·24	84 38 25·08

Doris (48).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, April 7	9 17 9·2	10 22 35·43	82 41 13·33
17	8 36 35·4	10 21 20·56	82 10 31·38

No occultations of stars by the moon were observed during the month of April 1860.

Eclipses and Transits of Jupiter's Satellites.

Day of Observation. 1860.	Satellite.	Phenomenon.	Mean Solar Time.	Observer.
			h m s	
April 1	III.	Ecl. disapp.	8 42 27·3	E.
1	I.	Ecl. reapp.	9 44 46·0	E.

Date. 1860.	Object.	No. of Threads.	Apparent Right Ascension. Observed. h m s	Computed. s
July 11	θ Ophiuchi	25	17 13 25·129	
	d Ophiuchi	25	17 18 25·379	
12	θ Ophiuchi	10	17 13 25·110	
	d Ophiuchi	5	17 18 25·330	
	Moon's 1st Limb	25	17 56 22·386	22·59
	μ' Sagittarii	25	18 5 23·944	
	β Sagittarii	5	18 12 2·365	
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16	11 2 51·2	12 44 3·77	103 52 56·13
17	10 58 2·2	12 43 10·57	103 43 11·49
27	10 10 50·2	12 35 16·37	102 5 33·61

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1860, April 12	9 29 35·2	10 54 46·30	85 36 53·02
16	9 12 27·5	10 53 21·99	85 27 13·57
17	9 8 14·4	10 53 4·77	85 25 8·13

Ariadne (43).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, April 7	9 10 44·9	10 16 10·11	85 7 40·39
10	8 58 5·9	10 15 18·74	84 57 12·59
17	8 29 45·2	10 14 29·24	84 38 25·08

Doris (48).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, April 7	9 17 9·2	10 22 35·43	82 41 13·33
17	8 36 35·4	10 21 20·56	82 10 31·38

No occultations of stars by the moon were observed during the month of April 1860.

Eclipses and Transits of Jupiter's Satellites.

Day of Observation. 1860.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
April 1	III.	Ecl. disapp.	8 42 27·3	E.
1	I.	Ecl. reapp.	9 44 46·0	E.

Day of Observation. 1860.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
April 2	IV. shadow	Ingress, bisected	9 45 31·8	J. C.
7	I.	Ingress, last cont.	11 1 14·7	E.
16	I.	Egress, last cont.	9 40 37·1	C.
17	I.	Ecl. reapp.	8 4 26·5	E.
30	II.	Ecl. reapp.	11 27 41·7	K.

The initials E., C., J. C., and K., are those of Mr. Ellis, Mr. Criswick, Mr. Carpenter, and Mr. Kerschner.

Observations of the Satellite of Saturn, Hyperion.

By W. Lassell, Esq.

I send herewith a few observations of *Hyperion*, the recently discovered satellite of *Saturn*, which I acknowledge I have of late somewhat neglected.

I do not find that any observations of this satellite have been published since the rather extensive series I obtained in Malta (*Monthly Notices*, vol. xiii. p. 181), nor have I learned that *Hyperion* has ever been seen, except in Mr. Bond's telescope and mine. Perhaps the possessors of some of the admirable refractors which have lately been constructed may now give it a passing notice. I find it faint and difficult to observe, except in a clear and dark sky, with which I am not here too frequently favoured.

The last observation (April 25th) was made with the 4-foot equatoreal; the haziness of the sky and the neighbourhood of the young moon rendering it unlikely that *Hyperion* would be measurable in the 2-foot equatoreal.

The period of revolution I deduce from the enclosed, compared with the Maltese observations, is 21·286 days.

Referring back to the measures of 1848, immediately upon the discovery of the satellite, and concluding its saturnicentric longitude at 1848, Sept. 19·76, to have been 82°, the period would be 21·284 days.

As somewhat connected with the subject, I may take the opportunity of remarking that I do not find that the generally received period of the closest satellite, *Mimas*, satisfies my observations. Comparing those made in Malta with some recently obtained, I deduce a period of 0·9427843, fraction of a day.

The measurements are referred to the apparent transverse axis of *Saturn's* ring produced, and to the centre of the planet.

1860, April 14·406,	Satellite east of Saturn's centre		204″·2
	and north of axis of ring (estimated)		35·0

1860, April 15·385,	Satellite east of Saturn's centre	166″·9
	and north of axis of ring	43·5
16·417,	Satellite east of Saturn's centre	84·9
	and north of axis of ring	50·1
20·458,	Satellite west of Saturn's centre	151·6
	and north of axis of ring	39·1
21·417,	Satellite west of Saturn's centre	192·2
	and north of axis of ring	33·5
23·396,	Satellite west of Saturn's centre	213·5
	and south of axis of ring	3·7
24·437,	Satellite west of Saturn's centre	198·5
	and south of axis of ring	16·0
25·427,	Satellite west of Saturn's centre	143·9
	and south of axis of ring	32·0

Bradstones, Sandfield Park, near Liverpool, May 10, 1860.

On the Value of the Constant of Aberration as determined by Eight Years' Observations of γ Draconis, made with the Reflex Zenith Tube at the Royal Observatory, Greenwich. By the Rev. Robert Main, M.A., President.

This paper, the author remarks, must be taken in connexion with a paper by him printed in the twenty-fourth volume of the *Memoirs* of the Society, which gave the discussion of the observations from 1837 to 1848, made with the 25-foot zenith tube of the Royal Observatory, for the determination of the constants of aberration and nutation. The results then found were unsatisfactory, the value of the constant of aberration being considerably smaller than the received value, and the value of the annual parallax being a negative quantity amounting to nearly four-tenths of a second of space. It became, therefore, a matter of interest to determine what result for the aberration would be given by the eight years' observations which have been since that time made with the reflex zenith tube, since from the simplicity of construction of the instrument no errors are to be dreaded arising from mechanical or instrumental causes.

For the tables on which the computations are based reference is made to the preceding paper, as well as for a general explanation of the methods used in forming and solving the equations of condition. These are 106 in number, arising from groups formed from 486 observations, and contain three unknown quantities, namely, the correction to the assumed mean

north zenith distance for the epoch chosen; the correction to the assumed value of the constant of aberration; and the constant of annual parallax. The equations are solved by the method of least squares, and the result for the constant of aberration is tolerably satisfactory, the resulting value being 20″·34. There results, however, a negative parallax amounting to 0″·24, which is larger than might have been expected, and would seem to denote some injurious effect, from atmospheric or other circumstances, on the observations.

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*Tables of the Developments of Functions in the Theory of Elliptic Motion.** By A. Cayley, Esq.

The notation made use of is as follows; viz., r, f, a, e, g denote

r, the radius vector,
f, the true anomaly,
a, the mean distance,
e, the excentricity,
g, the mean anomaly,

so that

$$\frac{r}{a}, = \text{elqr}\,(e, g), \text{ and}$$
$$f, = \text{elta}\,(e, g),$$

are known functions of e, g. Moreover, x denotes the periodic part of the quotient radius $\frac{r}{a}$, and y the equation of the centre, or periodic part of the true anomaly f; so that

$$\frac{r}{a} = 1 + x,$$
$$f = g + y,$$

and (x, y) are therefore also known functions of e, g.

Formulæ for the development in multiple cosines or sines, up to the terms in e^7, of the functions

$$(x^0, x^1, \ldots x^7) \begin{matrix}\cos \\ \sin\end{matrix} j\,y,$$

where j is an indeterminate symbol, are given by Leverrier in the *Annales de l'Observatoire de Paris*, tom. i. (1855), pp. 346–348. It occurred to me that it would be desirable

* The Tables will be printed *in extenso* in the *Memoirs* of the Society. Ed.

to form tables such as are contained in the present memoir, of various functions of the forms

$$x^m \begin{matrix}\cos\\ \sin\end{matrix} jf, \quad \left(\frac{r}{a}\right)^{\pm m} \begin{matrix}\cos\\ \sin\end{matrix} jf,$$

obtainable from Leverrier's formulæ by assigning particular integer values to the symbol j. The calculations were performed under my superintendence by Messrs. Creedy and Davis; and I have to acknowledge my obligation for a grant to defray the expenses made to me out of the Donation Fund at the disposal of the Council of the Royal Society.

A cosine series is in general represented in the form

$$\Sigma\, [\cos]^i \cos ig$$

where $[\cos]^i$ is a symbol used to denote the coefficient of $\cos ig$ in such a series, i extends from $-\infty$ to $+\infty$, and the coefficients $[\cos]^i$ satisfy the condition

$$[\cos]^{-i} = [\cos]^i;$$

and in like manner a sine series is represented in the form

$$\Sigma\, [\sin]^i \sin ig$$

where $[\sin]^i$ is a symbol used to denote the coefficient of $\sin ig$ in such a series, i extends from $-\infty$ to $+\infty$, and the coefficients $[\sin]^i$ satisfy the condition

$$[\sin]^{-i} = -[\sin]^i, \text{ and in particular}$$
$$[\sin]^0 = 0.$$

In the case of a pair of corresponding functions $x^m \cos jf$ and $x^m \sin jf$, or $\left(\frac{r}{a}\right)^{\pm m} \cos jf$ and $\left(\frac{r}{a}\right)^{\pm m} \sin jf$, one of them expanded in the form

$$\Sigma\, [\cos]^i \cos ig,$$

and the other in the form

$$\Sigma\, [\sin]^i \sin ig,$$

the sums and differences of the corresponding coefficients $[\cos]^i$, $[\sin]^i$, are represented by the notation

$$\{\cos \pm \sin\}^i,$$

that is $[\cos + \sin]^i$ stands for $[\cos]^i + [\sin]^i$, and similarly $[\cos - \sin]^i$ for $[\cos]^i - [\sin]^i$; and it is clear that we have

$$[\cos + \sin]^{-i} = [\cos - \sin]^i, \quad [\cos \pm \sin]^0 = [\cos]^0.$$

These coefficients $[\cos \pm \sin]^i$ are for many purposes equally useful with the coefficients $[\cos]^i$, $[\sin]^i$, and they are here tabulated accordingly.

The functions calculated are as follows :—

$$\left(x^1,\ x^2\ .\ .\ .\ .\ x^7\right) \qquad \text{to } [\cos];$$

$$\left(x^0,\ x^1,\ x^2\ .\ .\ .\ x^7\right)\begin{matrix}\cos\\ \sin\end{matrix} jf, \qquad j=1 \text{ to } j=7, [\cos], [\sin], [\cos \pm \text{si}$$

$$\left(\left(\frac{r}{a}\right)^{+4}, \ldots \left(\frac{r}{a}\right)^{+1}, \log\frac{r}{a}, \left(\frac{r}{a}\right)^{-1} \ldots \left(\frac{r}{a}\right)^{-5}\right) \qquad \text{to } [\cos];$$

$$\left(\left(\frac{r}{a}\right)^{+4} \ldots \left(\frac{r}{a}\right)^{+1}, \quad \left(\frac{r}{a}\right)^{-1} \ldots \left(\frac{r}{a}\right)^{-5}\right)\begin{matrix}\cos\\ \sin\end{matrix} jf, j=1 \text{ to } j=5, [\cos], [\sin], [\cos \pm \text{si}$$

all the developments being carried up to e^7, the limit of the formulæ from which they are deduced.

Observation of the Occultation of Jupiter by the Moon, on May 24, 1860. By Capt. W. Noble.

The brilliancy of the sunlight, and the disturbance of the atmosphere, necessitated the employment of a low power, to obtain a sharp and distinct view of the planet; I accordingly observed with a power of 42. I cannot speak with any great decision as to the instant of first contact, but it was perceptible to me at $8^h\ 45^m\ 32^s$ L.S.T.; the final disappearance took place at $8^h\ 46^m\ 32^s{\cdot}3$ L.S.T.

The first indication of the emergence of the planet was visible

at $9^h\ 57^m\ 34^s$ L.S.T., and its complete emersion took place

at 9 58 33 L.S.T.

When *Jupiter* had reappeared to the extent of about two-thirds of his area I received the impression for several seconds of a dark shadow or line bounding the moon's limb where it crossed

the planet, as described at pp. 70, 76, 77, and 80, of vol. xvii. of the *Monthly Notices*, by Messrs. Lassell, Mann, Dawes, Simms, &c. &c. It is a somewhat anomalous fact that I witnessed this same phenomenon during the occultation of *Mars* by the moon on 13th October, 1857; while, on the occasion of the occultation of *Saturn*, on the 8th of May last year, nothing whatever of the sort was visible (*Monthly Notices*, vol. xviii. p. 27, and vol. xix. p. 242). The moon and *Jupiter* were so apparently equal in brightness for a few seconds after the planet began to emerge, that he looked like a distorted portion of the lunar limb; as, however, the moon passed gradually off his disk he became sensibly paler; and when he was fairly clear of our satellite, his light was noticeably fainter than hers, his two principal belts were well seen throughout.

The telescope employed was my Ross equatoreal of 4·2 inches aperture and 61 inches focal length, with a power (as stated above) of 42, adjusted on *Jupiter*.

Forest Lodge, Maresfield, Sussex, May 29, 1860.

On Projection in Lunar Occultations.

By the Rev. T. W. Webb.

The attention of astronomers having recently been directed to the phenomenon of projection on the lunar disk at the time of occultations, I beg to point out an instance, which, as far as I know, has not been alluded to; where this strange anomaly seems to have been perceived by the unassisted eye. The date of this occurrence was 1794, March 7, and it is described at length in the *Philosophical Transactions* for that year. I have not at present access to the volume, but from an abstract in my possession it appears that three persons at Norwich, and one in London, saw a star that evening in the dark part of the moon, which had not then attained the first quadrature; and from the two representations which are given, the star must have appeared very far advanced upon the disk. On the same evening there was an occultation of *Aldebaran*, which Dr. Maskelyne thought a singular coincidence, but which would now be acknowledged as the cause of the phenomenon.

In the *Cométographie* of Pingré, tom. i. p. 321, is the following passage. [A.D.] "547. Une étoile entra sur le disque de la lune. Quelques écrivains rapportent ce phénomène à l'an 553; d'autres le font arriver entre 547 et 549." The references are to Aimoinus, the chronicles of St. Denys, Gregory of Tours, and Toromachus. This looks like a similar occurrence to the one just mentioned; but in the uncertainty of the date it would be a fruitless task to endeavour to ascertain

whether the occultation of any great star might possibly be thus recorded.

Without presuming to offer any opinion as to the cause of this anomaly, the inconsistencies of which seem to present a great obstacle to any mode of explanation, I may be permitted to mention that the effect of irradiation upon an object out of focus is greater than might be supposed by those whose vision is perfect; of this I have been made aware through my own near sight, in consequence of which luminous spaces are enlarged at the expense of adjacent dark ones, to an extent which might hardly have been anticipated.

Hardwick Parsonage, May 28, 1860.

RECENT PUBLICATIONS.

An Account of the Total Eclipse of the Sun, on September 7, 1858, *as observed near Olmos, Peru.* By Lieut. J. M. Gilliss. (Smithsonian Contributions to Knowledge.)

The author having proposed to Professor Henry, the Secretary of the Smithsonian Institution, to proceed to the west coast of South America to observe the eclipse of September 7, 1858, and his offer having been accepted, no time was lost in making the necessary arrangements for carrying his project into effect.

Prof. A. D. Bache, Superintendent of the U.S. Coast-Survey, contributed the instruments for determination of time, and the geographical position of the station, and a tent for shelter at night; Commander M. F. Maury, Superintendent of the U.S. Naval Observatory, furnished two excellent pocket-chronometers; and Mr. Henry Fitz, of New York, who had made the object-glass of the equatoreal of the U.S. Naval Astronomical Expedition, with great liberality specially completed and lent a $4\frac{1}{4}$-inch achromatic telescope, mounted on an equatoreal stand suitable for all latitudes.

The author was accompanied by a young friend from New York, Mr. C. H. Raymond, who was desirous of observing the phenomenon. They left New York on the 5th of August, and traversing the Isthmus of Panama, reached Payta in Peru on the 21st of the same month.

On the day of the author's arrival at Payta he received, through Capt. D'Aignan of the French war-steamer Mégère, a message from the admiral of the French fleet in the Pacific, courteously offering to convey him in the Mégère to any point on the coast, or to facilitate in any other manner in his power the observations which were the object of his journey.

A residence of eight days at Payta having shown that the probabilities of a favourable morning near the coast were against them, the author resolved to proceed to the interior and select some favourable place for observing the phenomenon.

It was arranged that Capt. D'Aignan should proceed with the Mégère and observe the phenomenon at Punta de la Aguja, some 60 miles south from Payta, where the central line of the shadow would first touch the Continent. M. De La Pinelas, one of the officers of the Mégère, was selected by his commander to make the physical observations.

Having ascertained from a recently published map that Olmos, a town within the outer Andes range, would certainly be included by the moon's shadow, and possibly be very near to its central line, he directed his course thither. The party consisted of the author, Mr. Raymond, two muleteers, one of whom acted as guide, and eight animals, two of which carried provisions for their subsistence. They arrived at Olmos on the morning of the 4th of September. The station chosen for observing the phenomenon was situated on a small level spot, on the northern ridge of a spur from the Andes, the line of which from the main chain is a segment of a circle. From a mean of meridian and circum-meridian altitudes of *α Lyræ*, *α Cygni*, and *α Gruis*, made at camp on the nights of the 5th and 6th of September, with sextant and artificial horizon, its latitude was found to be S. 6° 0′ 2″. The longitude was obtained by the aid of the chronometers. Its definitive value, as subsequently computed by Mr. F. G. Hesse, Assistant U.S. Astronomical Expedition, was found to be 79° 42′ 53″·4 West of Greenwich.

The following account of the phenomena is given in the author's own words:—

"In anticipation of the difficulty which would attend micrometrical measures of either kind during the very brief period the total eclipse would continue, a cross of wires, at right angles to each other, was inserted in a perforated card disk, and fitted to the eye-piece. Then, to facilitate the location of, and approximate to the elevation of such solar protuberances as might become visible, a diagram was prepared. Three concentric circles were drawn upon a page of the note-book. The inner one, to represent the moon, was of 4 inches diameter, a black continuous line; the outer ones, respectively, one-eighth and one-quarter of an inch greater, but with broken lines. The inner circle was divided into equal segments of $22\frac{1}{2}°$ each, the vertical diameter on the page to correspond with the zenith and nadir of the station. The outer circles were to facilitate estimations in altitude of the protuberances, by comparison with the visible lunar diameter, all of which was in the field of the telescope.

"The night of the 6th and until daylight of the morning of the 7th September was bright and faultless; but just before sunrise, a few banks of heavy cumuli formed over and near the

distant mountains to the eastward, the part of the sky least obscured being in the direction of the sun. Some of the intermediate peaks became enveloped in fog-banks at the same time, whilst thin cirri stretched to near the zenith. The wind was then from the westward, and the atmosphere humid, under the influence of which the lower strata of clouds increased in numbers and density, and the sun continued invisible until after the eclipse had made considerable progress. By $6^h\ 30^m$ all the fog-banks to the north and west were drifting towards the valley of Olmos, and at 7^h there was no portion of the sky entirely unclouded. Whenever the sun became momentarily visible during that period, the cusps could not be satisfactorily seen because of the masses of vapour continuously rolling across its disk. At about $7^h\ 15^m$ there was a striking change; the whole mass of cumuli to the eastward was lifted towards the zenith, leaving that portion of the sky clear, and it was only by close examination that a delicate film of vapour could be detected between us and the sun. At this period, the general light did not greatly differ in intensity from that of the moon when nearly full, though its colour resembled more that from burning spirit than that of the sun. Both intensity and colour changed under the feeble and gloomy illumination with perceptible rapidity, and for a minute before the first internal contact of the limbs, the hills near us appeared of a *black* hue.

"At from twelve to fifteen seconds before the beginning of totality the entire lune, then perhaps 35° in extent, broke up into masses of unequal length, showing detached portions wholly separated from the rest by dark lines. The occurrence was so sudden as to startle me, and although within the ten following seconds, the remaining visible fragments had become brilliant globules of nearly equal size, they differed from Baily's beads, as seen by me during the annular eclipse of September, 1838, by the darker spaces which separated them from one another.

"When the total obscuration took place at $7^h\ 30^m\ 42^s{\cdot}5$, four masses of luminous clouds became immediately visible at as many positions of the lunar circumference. Perhaps that to the left on the drawing was the first one seen, because my attention was directed there in watching the disappearance of Baily's beads, but I cannot say that they were not all simultaneously perceptible. The screen of glass was hurriedly removed, and in the brief instant of doing so I found, to my surprise, that all the phenomena were distinctly visible to the unassisted eye. Every effort has been made by the artist to represent in the accompanying lithograph, drawn from the sketch and notes, the scene then before me, and which still remains strongly impressed upon my memory.

"Of the four cloud-masses which had become visible, that on the upper border was much the most extended and serrated. It was more than 30° in extent, with an altitude, in the highest

part, of about 30″. All of them were of as nearly the form and at the relative positions given in the plate, which the most careful estimations enabled me to assign to them, and sketch roughly in the note-book, whilst they were still in sight. Neither of them exceeded 1′ or 1′ 10″ in height. I could not discern any rose or pink colour on either of them, but they all resembled irregular masses of illuminated clouds of leaden hue, fringed with bright light, more especially at the edges furthest from the sun. Surprised at the absence of red—a colour I fully expected to witness—and for the moment supposing my physical condition might have some influence, Mr. Raymond was requested to note the colours carefully.*

"A corona-light flashed out at the instant of totality. It extended furthest from the sun, in lines drawn from the centre through the solar clouds, but was nowhere traceable more than 15′ or 16′ beyond the lunar disk. There were no radial streamers, or bundles of rays, but only a uniformly diminishing, and slightly orange-tinted light, whose brightness and extent were apparently influenced by the mist-film, as the colour of the clouds also may have been. Beyond the corona-light, the colour of the sky was of a grayish-black.

"It was a far more imposing sight without than with the telescope; and long as has been my experience in the observation of celestial phenomena, and calm and unimpassioned, at such times, as my temperament has become, the sublime majesty of the scene thrilled me with excitement and humble reverence. Nor was it less effective upon others. Two citizens of Olmos stood within a few feet of me, watching in silence, and with anxious countenances, the rapid and fearful decrease of light. They were wholly ignorant that any sudden effect would follow the total obscuration of the sun. At that instant, one exclaimed in terror, '*La Gloria!*' and both, I believe, fell to their knees, filled with awe. They appreciated the resemblance of the corona to the halos with which the old masters have encircled their ideals of the heads of our Saviour and the Madonna, and devoutly regarded this as a manifestation of the Divine presence.

"Every second of the time during which the total obscuration continued—*only sixty and a half*—was fully occupied in storing my memory with facts, and in tracing outlines of the solar clouds and corona. They did not permit me to turn away for the observation of collateral phenomena, before it was necessary to watch for the sun's reappearance. This took place at $7^h\ 31^m\ 43^s$. The emersion was sudden. But though the limb was then sharp, and without the least appearance of dislocation, during the next two or three seconds there were

* The author had a severe attack of ague on the 4th of September, and on the 6th was labouring from excessive fever, accompanied by violent pains in the head and vertebræ. Before daylight of the 7th, however, he was up, and fortunately felt quite able to observe the phenomenon.

faint vertical striæ over the lune which gave to it a slight oscillating motion. Assuredly neither the illuminated clouds nor the corona disappeared at the actual instant of the return of light, but there was a brief period afterward when their images were still upon my retina.

"Though Mr. Raymond found a candle necessary to enable him to read satisfactorily the seconds-dial of the chronometer, and vernier-scale of the barometer, the darkness during totality could not have been very great, for my sketches were completed without the aid of artificial light. For some minutes previous all work in the valley below us had ceased, and even the strains of martial music, which the Governor of Olmos employed to cheer labourers digging for water, two or three miles from the town, were no longer audible. Superstition is still dominant here, and we could hear the solemn toll of the church-bell, whose sounds were intended to drive evil spirits from its vicinity. Neither at Olmos nor Piura did any *enceinte* woman leave her room during the eclipse, whilst some from curiosity, but more through fear, were in the streets, yet not daring to look upon the sun, lest malady should befall them. The sombre greenish light gave them the appearance of corpses, and they apprehended that a plague might be visited upon them. Afterwards the muleteers told us that their animals stopped eating, and huddled together in evident alarm."

The author has given a drawing of the phenomenon seen around the moon's limb during the totality of the eclipse. We certainly do not find in it such decided indications of complexity of structure as the Brazilian drawing presents. He has also appended to his own observations brief accounts of the eclipse as observed by Mr. Raymond and the French observers.

"*Observations of Mr. Raymond.*—Four solar protuberances were plainly visible to the naked eye, but they appeared to be of very nearly the same colour as the corona, making it look more intensely bright. There was no pink or red colour exhibited." Mr. Raymond looked at the sky to ascertain if any stars were visible, but clouds effectually concealed from view all those which might have been perceived.

"*Observations of the French Officers.*—Capt. D'Aignan states that although the sky was somewhat cloudy, several stars were seen during the totality of the eclipse. The obscurity was very sensible, and it seemed to pass off more rapidly than it came on. M. Pinelais had translated all the important portions of Mr. Carrington's pamphlet, containing 'Suggestions to Observers, &c.,' and made copies of its diagrams previous to the author's departure to the interior. In his brief account of the phenomenon he states that the spots appeared of a fine bright rose colour."

Précis de l'Histoire de l'Astronomie aux Etats-Unis d'Amérique, par Ed. Mailly, Aide à l'Observatoire Royal de Bruxelles (extracted from the *Annuaire de l'Observatoire Royal de Bruxelles pour l'Année* 1860), 12mo, Brussels, 1860, (88 pages). This little volume contains an interesting sketch of the history of astronomy in North America, from the foundation, in the year 1743, of the American Philosophical Society at Philadelphia, and the transit of *Venus* in 1769 (the occasion of the first recorded astronomical enterprise in the United States) up to the present time. The author mentions, as existing in 1856, inclusive of the two great observatories of Washington and Cambridge—twenty-five astronomical observatories (public and private)—in the United States; sixteen of them founded since the year 1843.

Annales de l'Observatoire Impérial de Paris. Publiées par U. J. Leverrier, Directeur de l'Observatoire, tom. v. 4to, Paris, 1859.

This volume contains the theory and tables of *Mercury* by M. Leverrier; the discrepancy as regards the secular motion of the perihelion which is found to exist between theory and observation, led, as is well known, to the suggestion by M. Leverrier of the existence of a planet or group of small planets interior to *Mercury*. The volume contains also a memoir by M. Foucault, on the "Construction of Telescopes with Silvered Glass Mirrors;" a memoir by M. Serret, "On the Theory of the Motion of the Earth about its Centre of Gravity;" a note by the same author, "On the Theory of the Elliptic Motion;" and "Researches on the Atmospheres of Comets," by M. Roche. The article of the *Comptes Rendus* which contains Dr. Lescarbault's letter in relation to his observation of a planet, and M. Leverrier's remarks on it, is reproduced in the appendix.

The Berlin *Astronomisches Jahrbuch* für 1862 has appeared. The Appendix contains a memoir by M. Encke on mechanical quadratures, and also a list of new elements and ephemerides for the year 1860, of fifty of the small planets.

ERRATUM.

Page 252, line 12, *for* December, *read* November.

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The Rev. R. MAIN, President, in the Chair.

Sanford Gorton, Esq., Stamford Villas, Hackney;
Francis Abbott, Esq., Hobart Town, Tasmania; and
Louis P. Casella, Esq., 23 Hatton Garden,

were balloted for and duly elected Fellows of the Society.

Réponse aux Observations présentées par M. Adams sur différentes Objections élevées contre sa Théorie de l'Accélération du moyen Mouvement de la Lune. Par M. G. de Pontécoulant.

M. Adams, dans une note insérée aux *Monthly Notices* (tome XX. No. 6), a essayé de repousser à la fois les observations critiques qui avaient été soulevées contre son analyse par MM. Plana et Hansen, et par moi ; je ne repondrai ici qu'à ce qui m'est personnel ; les deux savants astronomes auxquels j'ai l'honneur de me trouver accolé en cette circonstance, défendront leur cause beaucoup mieux que je ne pourrais faire ; je dois d'ailleurs tâcher d'être aussi succinct que possible sur un sujet dejà plusieurs fois traité dans ce recueil, et je laisserai à l'intelligence du lecteur le soin de suppléer à ce que j'aurais pu omettre dans la seule crainte de le fatiguer par des répétitions fastidieuses.

M. Adams s'excuse d'abord de n'avoir pas répondu plutôt aux observations contenues dans ma note insérée dans le numéro des *Monthly Notices* de Juillet dernier, parceque, dit-il :—"I had some hope that M. de Pontécoulant might be led to see and acknowledge *the errors* into which he had fallen."

Nous étions sur ce point encore une fois bien loin de nous entendre, car j'avais cru de mon coté que le silence de M. Adams provenait de ce qu'il avait reconnu lui-même les fautes que je lui avais signalées et je me reprochais dejà de les lui avoir indiquées peut-être avec trop peu de ménagements. Je vois que je m'étais trompé, je me trouve donc forcé de reprendre la question en entier, et de montrer que M. Adams, en prétendant redresser les *erreurs* contenues dans ma première note, en ajoute lui-même à celles de son mémoire de 1853 de nouvelles, qui montrent l'oubli de tous les principes suivis par les géomètres qui se sont occupés jusqu' ici de la théorie de la lune, et ce qui est plus grave encore, qu'il commet une faute essentielle contre les *premiers principes du calcul infinitésimal*, dans l'établissement de la formule qui sert pour ainsi dire de fondement à toute cette discussion. Comme il arrive toujours lorsqu' entré dans une mauvaise voie on se refuse à écouter les avertissements qui vous sont donnés pour en sortir.

1°. A l'argument sur lequel M. Adams s'appuie de nouveau, tiré de la concordance de ses résultats avec ceux qu'a trouvés de son coté M. Delaunay, par un procédé de calcul différent du sien, je repondrai ce que j'ai dejà en plusieurs fois l'occasion de dire à ce dernier, c'est que cet accord prouve seulement que ces messieurs sont partis, si ce n'est des mêmes formules, du moins des mêmes principes, et que si par conséquent nous prouvons que l'un des deux s'est trompé, il en résultera évidemment que l'autre est tombé dans la même erreur ; car on ne saurait supposer qu'une formule exacte et une formule fautive puissent conduire au même résultat, ce serait ébranler les bases de toutes les sciences fondées sur l'analyse mathématique.

2°. J'ai dit, ou plutôt j'ai fait préssentir, à la fin de ma note citée plus haut, que lorsqu'on calcule le coefficient de l'équation séculaire du moyen mouvement lunaire par les formules où la longitude vraie de la lune était prise pour la variable indépendante et par celles où l'on avait choisi le temps t pour cette variable, on obtenait des résultats analytiques très différents l'un de l'autre, bien que leurs valeurs numériques dussent sans doute présenter entre elles très peu d'écart; cette assertion a paru beaucoup surprendre M. Adams, bien qu'il fut arrivé le premier à cette même conclusion, comme on le voit par le dernier paragraphe de son mémoire de 1853, la seule différence entre nous étant que M. Adams a cherché à faire disparaître cette discordance par des moyens qui lui sont propres, et que nous aurons à apprécier bientôt, tandis que moi je l'ai acceptée comme un résultat irréfutable du calcul. M. Adams va plus loin, dans un autre passage de son dernier écrit, il dit très nettement : — "I confess that it appears to me *absurd* to suppose that the result of an investigation ought to be different when a different independent variable is employed in obtaining it." Prenons un exemple bien simple et qui se rattache, comme on va le voir, à notre sujet; supposons qu'on veuille déterminer

la partie non périodique de la fonction $\frac{1}{r}$, en désignant par r le rayon vecteur de la lune, et en faisant abstraction des forces perturbatrices; si l'on a pris pour variable indépendante la longitude vraie, on aura par les formules du mouvement elliptique $\frac{1}{r} = \frac{1}{a}(1+e^2+e^4+e^6+\text{etc.})$ et si l'on choisit pour variable le temps t on aura simplement $\frac{1}{r} = \frac{1}{a}$; ces deux valeurs sont très différentes, quoiqu'elles expriment la même chose dans les deux cas. Que l'on veuille bien réfléchir maintenant que le coefficient de l'équation séculaire du moyen mouvement lunaire, dépend précisement de la combinaison de la partie non périodique de la fonction $\frac{1}{r}$ dans l'orbite troublé, avec la partie correspondante de la fonction qui exprime la différentielle de la longitude vraie, prise par rapport au temps t, ou reciproquement, on verra que le fait que nous avons avancé, n'avait rien qui put exciter la surprise que M. Adams en a témoignée. Mais d'ailleurs il faut observer que ce qu'on appelle communement *l'équation séculaire* de la lune, n'est pas précisement la même chose dans les deux cas que nous venons de considérer, les formules directes en effet, donnent immédiatement et telle qu'elle doit être employée dans les tables, *l'équation séculaire* qui affecte le moyen mouvement de la lune, dans l'expression de sa longitude vraie en fonction de la longitude moyenne ou du temps; les formules de Laplace, au contraire, donnent *l'équation séculaire* qui affecte la longitude vraie dans l'expression de la longitude moyenne en fonction de cette même longitude, on en déduit ensuite, par une inversion et des suppositions tout-à-fait arbitraires, l'expression de *l'équation séculaire* du moyen mouvement proprement dite, il ne serait donc pas bien surprenant que deux quantités qui ne sont pas absolument identiques, fussent exprimées analytiquement d'une manière différente; et enfin, c'est là, comme l'a dit Laplace en une autre occasion, une de ces questions où les inductions les plus vraisemblables du raisonnement doivent céder à un guide plus sur et plus infaillible, les déductions du calcul.

3°. Voyons maintenant comment M. Adams a prétendu faire accorder les résultats que nous avons obtenus par les deux méthodes que nous avons successivement employées dans la recherche du coefficient de l'équation séculaire, c'est-à-dire la méthode suivie par MM. Plana et Damoiseau, et celle que nous proposons définitivement de lui substituer, comme plus directe et plus exacte. C'est ici qu'il est curieux d'observer à quel point la préoccupation d'une idée qu'on veut faire triompher, peut égarer un esprit qui s'est montré d'ailleurs éclairé et judicieux dans d'autres occasions.

M. Adams donne dans sa note (*Monthly Notices*, vol. xx. No. 6, p. 231) pour déterminer le moyen mouvement n de la

lune dans son orbite troublée, les trois équations suivantes, dérivées, dit-il, de nos propres formules:—

$$\frac{dn}{n\,dt} = -3\frac{d\mathrm{h}}{\mathrm{h}\,dt} + \frac{d(e'^2)}{dt}\left(-\frac{3}{2}m^2 + \frac{2187}{128}m^4\right)$$

$$\frac{dn}{n\,dt} = -3\frac{dh}{h\,dt} + \frac{d(e'^2)}{dt}\left(-\frac{3}{2}m^2 - \frac{297}{31}m^4\right) \qquad (1)$$

$$\frac{dn}{n\,dt} = -\frac{3}{2}\frac{da_,}{a_,dt} + \frac{d(e'^2)}{dt}\left(-\frac{3}{2}m^2 + \frac{5337}{128}m^4\right)^*$$

Puis il ajoute:—"It is obvious that these several values of $\frac{dn}{n\,dt}$ *contradict* each other, and the reason is that the quantities h, h, and a are really variable, and that therefore $\frac{d\mathrm{h}}{\mathrm{h}\,dt}$, $\frac{dh}{h\,dt}$, and $\frac{da}{a\,dt}$ have been wrongly neglected. In order to find the true value of $\frac{dn}{n\,dt}$, we must therefore determine the values of the last-mentioned differential coefficients, and substitute them in the several expressions for $\frac{dn}{n\,dt}$ given above."

En conséquence dans une lettre qu'il m'avait fait l'honneur de m'adresser par l'entremise de M. Lubbock, notre ami commun (en date du 17 Octobre, 1859), M. Adams ajoutait:—"Now it is easily shown, without involving any point of dispute, that the following relation exists between h and h (ces deux quantités étant essentiellement différentes comme on le verra plus bas):—

$$\frac{\mathrm{h}}{h} = 1 + \frac{75}{64}m^4 + \frac{127}{32}m^5 + e'^2\left(\frac{1125}{128}m^4 + \frac{3429}{64}m^5\right) \qquad (2)$$

"Putting, however, for $\frac{d\mathrm{h}}{\mathrm{h}\,dt}$ et $\frac{dh}{h\,dt}$ their proper value, I find exactly the same value of $\frac{dn}{n\,dt}$ from the two expressions." Et c'est probablement à cette concordance *ainsi obtenue*, que M. Adams faisait allusion dans le dernier paragraphe de son mémoire de 1853, lorsqu'il disait:—"I have the satisfaction of finding a perfect agreement between the results."

Or, il n'y a à ce raisonnement qu'une petite difficulté, c'est que le rapport $\frac{\mathrm{h}}{h}$ donné par la formule (2), étant précisement tiré de la comparaison des deux valeurs de n rapportées par M. Adams dans la page 231 de sa note, et dont la différentiation a produit les deux premières formules (1), l'accord trouvé

* M. de Pontécoulant writes $a_,$ instead of the a of Mr. Adams' Note.—Ed.

ne prouve rien du tout, si ce n'est qu'on a fait un *cercle vicieux*, et qu'on est revenu précisement au point d'où l'on était parti: M. Adams, il est vrai, n'a pas reproduit cet étrange argument dans sa dernière note insérée aux *Monthly Notices;* mais il ne parait pas cependant que l'observation que je lui en ai faite, ait produit tout le fruit que j'en attendais, car M. Adams après avoir reproduit l'équation (2), page 234, ajoute:—"Now this relation at once shows that if e' be variable, h and h cannot both be constant; and since no *à-priori* reason can be given why one of these quantities should be constant rather than the other, we are not justified in *assuming* that either of them is so.

"This argument, however, does not appear *convincing* to M. de Pontécoulant."

Oh! certes, M. Adams ne se trompe nullement dans cette dernière assertion. En effet les quatre lignes, que je viens de rapporter et que j'ai extraites exactement de sa Note, ne prouvent qu'une chose, et justifient complètement ce que j'ai avancé au commencement de cette dissertation, à savoir que M. Adams avait complètement mis en oubli, du moins pour le moment, tous les principes sur lesquels sont fondées les différentes solution du problème des perturbations lunaires, données par les géomètres qui se sont occupés de cet objet. En effet ne semble-t-il pas croire que l'on est libre de donner dans la formule (2) à h et à h les valeurs arbitraires que l'on voudra choisir, de les regarder comme constantes ou comme variables, c'est-à-dire comme des fonctions indépendantes ou dépendantes de e', selon les convenances de la question qu'on traite; mais il ne faut pas oublier que la détermination de l'équation séculaire n'est qu'une question toute particulière de la grande théorie des perturbations lunaires, les valeurs des quantités h et h sont fixées d'avance par cette théorie, elles entrent dans les expressions de toutes les inégalités périodiques ou à longues périodes, et l'on voit que l'argument, que M. Adams s'étonne que je n'aie pas trouvé *convincing*, aboutissait, en définitive à la proposition d'une altération complète de toutes les inégalités déterminées théoriquement par le calcul des perturbations du mouvement de notre satellite.

Revenons aux vrais principes, et l'on verra aussitôt que la discordance, que M. Adams a cru remarquer entre les trois valeurs de $\frac{dn}{ndt}$ rapportées plus haut, et qui résultent en effet de mes recherches, en négligeant bien entendu les termes d'un ordre supérieur à m^4 et les termes dépendant de l'excentricité et de l'inclinaison à l'écliptique de l'orbite lunaire, n'a jamais existé que dans son imagination, ces valeurs ne se contredisent nullement, et viennent au contraire à l'appui de ce que j'ai avancé précedemment.

D'abord il faut remarquer, comme je l'ai dit plus haut, que les lettres h et h représentent des quantités essentiellement différentes, la première que j'ai désignée par h pour la distin-

guer de la seconde, est celle que Laplace a employée dans sa théorie, elle représente le double de l'aire décrite pendant l'unité de temps par la projection du rayon vecteur de la lune sur le plan de l'écliptique, le mouvement étant supposé elliptique ; en sorte que si l'on néglige pour plus de simplicité l'inclinaison de l'orbite lunaire sur l'écliptique, on aura

$$h = \sqrt{a, (1 - e^2)},$$

a, étant le demi-grand axe de l'ellipse que décrirait la lune autour de la terre sans l'action du soleil, h est donc une quantité indépendante de e', et par conséquent rigoureusement constante dans ce cas ; et c'est ainsi que l'ont toujours supposé Damoiseau, M. Plana, et tous ceux qui ont employé en les étendant les formules données par Laplace dans le 7ᵉ livre de la *Mécanique Céleste ;* et si M. Adams insistait pour savoir les raisons de cette signification donnée à la constante h, nous repondrions que c'est qu'il a plu à Laplace de la choisir ainsi, mais que, ce choix une fois fait, toutes les arbitraires du problême ont été déterminées de manière à satisfaire aux équations différentielles du mouvement troublé sous la forme adoptée par Laplace, et qu'on ne saurait maintenant altérer en aucune manière la valeur de h, à moins de donner une théorie nouvelle des mouvements lunaires, sous peine d'altérer de la manière la plus grave toutes les expressions des coefficients des inégalités périodiques déterminées par Laplace, Damoiseau, ou M. Plana.

Cela posé, il est évident qu'on doit faire $\frac{d\,h}{h\,d\,t}$ égal à zéro dans la première des formules (1) et l'on a alors simplement

$$\frac{d\,n}{n\,d\,t} = \frac{d\,(e'^2)}{d\,t}\left(-\frac{3}{2}\,m^2 + \frac{2187}{128}\,m^4\right),$$

valeur qui coincide, dans l'ordre de termes que nous considérons, avec celle donnée par M. Plana dans sa Théorie de la Lune, ce qui devait être, puisque c'est celle que nous avons obtenue en suivant, pour vérifier les résultats de M. Plana,* la méthode où l'on prend la longitude vraie de la lune, pour la variable indépendante.

* Il n'est ici question que des résultats qui sont rapportés dans le grand ouvrage de M. Plana (*Théorie de la Lune*, 3 vols. in 4to.) et dont nous avons par la comparaison aux résultats de nos propres calculs vérifié la parfaite exactitude. M. Plana est depuis revenu plusieurs fois sur ce sujet, il a cru d'abord devoir rectifier ses premières valeurs pour se conformer aux idées de M. Adams, puis il est revenu sur cette décision, puis il a adopté une partie de ses premières corrections et en a rejeté une autre, nous n'avons point à nous occuper ici de ses variations qui montrent peu de fixité dans les idées de ce géomètre distingué, nous n'y voyons qu'une preuve de plus de la nécessité de détruire *sans ménagement* cette fois, toutes les vues *fausses* et *erronées* qui se trouvent dans le mémoire de M. Adams de 1853, puisqu'elles peuvent avoir une si funeste influence même sur des esprits très éclairés.

Il ne nous reste donc qu'à examiner les deux dernières formules (1), qui dérivent des nouvelles recherches que j'ai faites sur la détermination du coefficient de l'équation séculaire ; c'est-à-dire du développement des formules employées dans ma théorie analytique où le temps t a été pris pour la variable indépendante. Or, loin de se contredire, on va voir que ces deux formules coincident parfaitement ensemble, seulement il faut faire ici une observation, qui semble, *par une singulière inadvertence*, avoir échappé à M. Adams ; la constante qui entre dans la troisième des formules (1) et qui sert à compléter l'équation différentielle du rayon vecteur, comme le dit M. Adams, page 231, n'est autre, comme on le voit par cette équation, que le demi-grand axe de l'orbite elliptique de la lune, c'est pour cela que nous l'avons désignée dans les formules (1) par $a_{,}$, cette quantité est donc nécessairement indépendante de l'excentricité e' de l'orbe terrestre. Quant à la constante h qui entre dans l'équation de la longitude

$$\frac{r^2 d\nu}{dt} = h + \int \left(\frac{dR}{d\nu}\right) dt,$$

l'inspection seule de cette formule montre ce qu'elle représente. En effet $\int \left(\frac{dR}{d\nu}\right) dt$ ne contenant que des termes périodiques, si l'on néglige ces termes, on voit qu'on aura $h = \frac{r^2 d\nu}{dt}$. C'est-à-dire que $2h$ représente, dans ce cas, la partie non périodique de l'aire décrite pendant l'unité de temps par le rayon vecteur de la lune dans son orbite troublée, de même que dans les formules de Laplace 2 h représentait la même aire dans l'orbite elliptique. On conçoit maintenant pourquoi dans nos formules la quantité h doit dépendre nécessairement des forces perturbatrices, et contient par conséquent dans son développement l'excentricité e' de l'orbite solaire ; h dans ce cas est donc une quantité essentiellement variable, et il faut par conséquent avoir égard à sa variation dans la seconde des formules (1). Or la valeur de h n'est nullement arbitraire, elle est complétement déterminée par les approximations successives, par les conditions à remplir pour satisfaire aux équations différentielles du mouvement troublé ; nous avons trouvé ainsi, en ne portant les approximations que jusqu' aux termes de l'ordre m^4 (*Théorie Analytique*, No. 50, livre 7),

$$h = \sqrt{a}\left[1 - \frac{m^2}{3} + \frac{11}{144}m^4 - \left(\frac{m^2}{2} + \frac{185}{96}m^4\right)e'^2\right]$$

où a représente le demi-grand axe de l'orbite correspondant à n en sorte qu'on ait $a^3 n^2 = 1$; ou trouve d'ailleurs en vertu de cette équation, et par la formule rapportée par M. Adams (page 231, ligne 12):

$$\sqrt{\frac{a}{a_1}} = 1 + \frac{1}{3}m^2 - \frac{1285}{576}m^4 + \left(\frac{m^2}{2} - \frac{5657}{384}m^4\right)e'^2,$$

d'où l'on conclut:

$$h = \sqrt{a_1}\left(1 - \frac{145}{64}m^4 - \frac{2175}{128}m^4 e'^2\right),$$

valeur remarquable où l'on voit que les termes en m^2 et m^3 ont totalement disparu.

Si l'on substitue cette valeur et sa différentielle dans la seconde des formules (1) on trouve

$$\frac{dn}{n\,dt} = \frac{d(e'^2)}{dt}\left(-\frac{3}{2}m^2 + \frac{5337}{128}m^4\right),$$

c'est-à-dire une valeur de $\frac{dn}{n\,dt}$ identique avec celle que donne la troisième des formules (1) lorsqu'on y suppose, comme on doit le faire, $\frac{da_1}{a_1\,dt} = o$, puisqu'en effet a_1 est une quantité essentiellement constante. Ainsi donc nos trois formules (1) sont parfaitement exactes, seulement la première ne concorde pas avec les deux autres, à partir des termes de l'ordre m^4, ce qui n'a rien qui doive surprendre d'après ce que nous avons dit plus haut, puisqu'elles se dérivent de deux méthodes totalement différentes. On peut remarquer encore que lorsqu'on a remplacé $\frac{dh}{h\,dt}$ par sa valeur dans la seconde des formules (1), le second terme de la valeur de $\frac{dn}{n\,dt}$ ne diffère *plus de signe* du terme correspondant de la même valeur, donnée par la première de ces formules, comme l'avait remarqué M. Adams dans le dernier paragraphe de son mémoire de 1853, et qui n'était qu'une assertion tout-à-fait erronée, puisqu'il n'avait point tenu compte de tous les termes qui doivent concourir à former la quantité dont il s'agit. Ainsi disparait l'un des principaux appuis sur lequel reposait l'argumentation de M. Adams.

Nous voici arrivés maintenant au point capital de la discussion, puisqu'il s'agit de l'existence même des nouveaux termes que M. Adams a proposé d'introduire dans le coefficient de l'équation séculaire du moyen mouvement lunaire; nous avions cru avoir prouvé dans notre première note (*Monthly Notices*, Juillet, 1859) que de pareils termes étaient tout-à-fait *inadmissibles*, mais puisque la démonstration n'a pas paru suffisante à M. Adams, nous allons reprendre la question sous toutes ses faces et la couler tellement à fond cette fois, qu'il n'est pas probable que personne soit jamais tenté de la relever de ses ruines.

M. Adams pour retablir, dit-il, un accord, qu'il croit nécessaire, entre les trois formules (1), accord qui existait parfaite-

ment, sans qu'il s'en doutât, du moins pour les deux dernières, prescrit d'introduire outre les termes périodiques, trois termes *non périodiques* de la forme $H e'^2$ dans les trois intégrales

$$\int r^2 \frac{dR}{d\nu} dt; \quad \int \frac{dR}{d\nu} dt; \quad \int d' R$$

qu'ils doivent servir à compléter. Voyons donc comment M. Adams prétend procéder à la formation de ces termes. Pour la première formule il nous renvoie à l'article 11 du mémoire de 1853, où il dit avoir prouvé que la fonction $2\int r^2 \frac{dR}{d\nu} d\nu$ contient les termes non périodiques

$$h^2\left(-\frac{285}{8} m^4 e'^2 + \frac{495}{64} m^4 e'^2\right) = h^2\left(-\frac{1785}{64} m^4 e'^2\right).$$

Or c'est précisement de l'introduction de ces termes que nous avons démontré le vice et *l'inadmissibilité* à la page 315 de la note insérée aux *Monthly Notices* (Juillet, 1859), reprenons donc la démonstration et rendons la, s'il se peut, plus péremptoire cette fois.

Admettons que la fonction $r^2 \frac{dR}{d\nu} d\nu$ contienne dans son développement un terme de cette forme $H e' \sin(l\nu + C)$, on aura, en ne considérant que ce terme, et en ayant égard en effectuant l'intégration à la variabilité de e':—

$$\int r^2 \frac{dR}{d\nu} d\nu = -\frac{H}{l} e' \cos(l\nu + C) + \frac{H}{l}\int \frac{de'}{d\nu} \cos(l\nu + C) d\nu.$$

Si l'on substitue dans le dernier terme, avant d'effectuer l'intégration indiquée, à la place de $\frac{de'}{d\nu}$ sa valeur tirée de l'équation $e' = f + f'\nu$, selon l'expression que l'on doit supposer à la variation de l'excentricité e' quand on fait usage des formules où la longitude vraie de la lune a été prise pour la variable indépendante,* il est évident qu'on aura

$$\int r^2 \frac{dR}{d\nu} d\nu = -\frac{H}{l} e' \cos(l\nu + C) + \frac{Hf'}{l^2} \sin(l\nu + C)$$

$$= -\frac{H}{l} e' \cos\left(l\nu + C + \frac{f'}{l}\right)$$

c'est-à-dire un terme périodique dans lequel l'angle $l\nu + C$ sera simplement augmenté de la quantité constante $\frac{f'}{l}$, ce qui n'altérera nullement la forme de la série, et l'on pourra, par con-

* Voir ce qui a été dit (*Monthly Notices*, Juillet, 1854, p. 313) et *Mécanique Céleste*, liv. 7e, No. 10.

séquent, comme $\frac{f'}{l}$ est d'ailleurs une très petite fraction, continuer, sans erreur sensible, à ne pas tenir compte, dans le développement de la fonction $\int r^2 \frac{dR}{dv} dv$, ainsi qu'on l'a fait jusqu'à présent, de la variabilité de l'excentricité e' de l'orbite terrestre.

Tel est le résultat indiqué par une exacte analyse de la question, et ce résultat est conforme à ce qu'ont fait Laplace, Damoiseau, M. Plana, et tous les géomètres qui s'étaient occupés jusqu'ici de la théorie des perturbations de la lune. Voyons comment M. Adams a pu se trouver conduit à un résultat tout opposé. Pour cela, dans la valeur précédente de $\int r^2 \frac{dR}{dv} dv$, M. Adams introduit sous le signe intégral, au lieu de $\frac{de'}{dv}$ sa valeur $\frac{de'}{dt} \cdot \frac{dt}{dv}$, il obtient ainsi, en négligeant les quantités simplement périodiques, le terme suivant

$$\int r^2 \frac{dR}{dv} dv = \frac{H}{l} \int \frac{de'}{dt} \frac{dt}{dv} \cos(lv + C)\, dv.$$

Puis il remplace $\frac{dt}{dv}$ par sa valeur développée en série de cosinus d'angles proportionnels à la longitude vraie v de la lune, et qui est donnée par les approximations successives; parmi les termes de ce développement il s'en trouve nécessairement un qui a le même argument $lv + C$ que le terme $H e' \sin(lv + C)$ que l'on a considéré dans $r^2 \frac{dR}{dv}$, en représentant par $H' e' \cos(lv + C)$ ce terme, on aura, en négligeant les quantités simplement périodiques, et substituant $n\,dt$ à la place de dv:—

$$\int r^2 \frac{dR}{dv} dv = \frac{H H' n}{2l} \int \frac{e'\, de'}{dt} \cdot dt = \frac{H H' n}{4l} e'^2.$$

Il est donc évident que le terme non périodique que nous venons d'introduire dans l'expression de la fonction $\int r^2 \frac{dR}{dv} dv$ qui d'après l'analyse précédente ne devint renfermer, en vertu de la variabilité de e', que des quantités périodiques, ne doit son existence qu'à la supposition que M. Adams a faite, qu'on peut supposer $\frac{de'}{dv} = \frac{de'}{dt} \cdot \frac{dt}{dv}$; puis traiter $\frac{de'}{dt}$ comme une constante lorsqu'on substitue pour $\frac{dt}{dv}$ sa valeur développée en série de termes périodiques. Or, non seulement, cette supposition n'est pas justifiable, *elle est*, il faut le dire, *contraire aux premiers principes du calcul différentiel.* En effet pour qu'on put substituer à la place de $\frac{de'}{dv}$ sa valeur $\frac{de'}{dt} \cdot \frac{dt}{dv}$ il fau-

droit supposer que l'on a remplacé $\frac{de'}{dv}$ non pas par une constante, mais par sa valeur complète déduite de la théorie de la variation des constantes arbitraires, $\frac{de'}{dt}$ devient alors, après y avoir substitué à la place de t sa valeur en v, une fonction de cette dernière variable, et l'on a par conséquent $\frac{de'}{dv} = \varphi v \cdot \frac{dt}{dv}$; on conçoit donc que si l'on substitue pour $\frac{dt}{dv}$ sa valeur dans cette expression et qu'ensuite on développe la fonction résultante, on aura $\frac{de'}{dv} = f + P$, où P représente une suite de termes périodiques, termes qu'on doit négliger dans l'expression de $\frac{de'}{dv}$ puisqu'il ne faut avoir égard dans la valeur de e' qu'aux *termes séculaires*, et que tous les termes *périodiques* sont déjà supposés compris dans les trois co-ordonnées du soleil, conformement à la théorie ordinaire de la variation des constantes arbitraires. Il est donc évident d'après cette analyse qu'il suffira de substituer, à la place de $\frac{de'}{dt} \cdot \frac{dt}{dv}$ dans l'expression de $\int r^2 \frac{dR}{dv} dv$, la premier terme de sa valeur f, et alors on reconnaîtra, d'après ce qui a été dit plus haut, qu'il n'en saurait résulter dans la valeur de cette fonction que des termes périodiques.

Ce que nous venons de dire relativement à l'intégrale $\int r^2 \frac{dR}{dv} dv$ s'applique naturellement à l'intégrale $\int \frac{dR}{dv} dt$, qui est supposée développée en série de sinus d'angles proportionnels au temps t; il nous semblait même que *sur ce point* il ne pouvait y avoir aucune contestation possible, car comme M. Adams ne s'explique pas très clairement à cet égard, on ne conçoit pas même de quelle manière des termes non périodiques pourraient y être introduits par l'effet de la variabilité de e'. En effet supposons que la fonction $\frac{dR}{dv}$ contienne le terme suivant:—

$$\frac{dR}{dv} = A e' \sin (lt + C),$$

on aura en multipliant par dt et intégrant

$$\int \frac{dR}{dv} dt = -\frac{A}{l} e' \cos (lt + C) + \frac{A}{l} \int \frac{de'}{dt} \cos (lt + C).$$

Or il est évident que si l'on se borne à considérer dans l'expression de e' la partie séculaire, ce qui permet de supposer $\frac{de'}{dt}$ égal à une constante, le second terme de l'expression pré-

cédente ne produira qu'un terme périodique qui s'ajoutera au premier (voir *Monthly Notices*, Juillet, 1859, p. 311) et qui n'altérera en aucune manière la forme de la série. On pourra donc satisfaire dans ce cas aux équations du mouvement troublé par les mêmes valeurs dans lesquelles on altérera seulement les constantes qui servent à compléter les arguments des différentes inégalités, les co-ordonnés de la lune conserveront les mêmes valeurs quelque loin que l'on pousse les approximations, et c'est par cette raison que nous avions supposé, que dans le cas au moins où l'on prend le temps t pour la variable indépendante, tout le monde était d'accord qu'on peut, dans l'intégration des formules différentielles du mouvement troublé, n'avoir aucun égard à la variation de l'excentricité de l'orbe terrestre. Je vois d'après ce que dit M. Adams dans sa note que je m'étais encore trompé sur ce point, mais comme il n'explique pas la méthode qu'il veut suivre pour calculer les nouveaux termes que la cause dont il s'agit introduirait, selon lui, dans les trois co-ordonnées de notre satellite, qu'il n'entre dans aucun détail qui puisse même nous rien faire préjuger à cet égard, nous attendrons qu'il se soit expliqué plus catégoriquement pour lui repondre si ce que nous venons de dire ne suffisait pas encore pour l'éclairer.

Il serait inutile de pousser plus loin cette discussion, du moment qu'il est prouvé, et je crois cette fois d'une manière incontestable, que l'introduction des nouveaux termes que l'on avait considérés dans les expressions des trois co-ordonnées lunaires, *tenait à l'oubli d'un des premiers principes du calcul différentiel* et que personne en insistant ne voudra se rendre complice d'une faute si grave. M. Adams levera lui-même toutes les difficultés qu'il a pu rencontrer dans ses recherches sur *l'équation séculaire* du moyen mouvement lunaire, et trouvera entre les formules, celles de M. Plana et les notres, et même avec celles de M. Hansen, s'il veut suivre la méthode des substitutions successives au lieu de celle des dévéloppements en série, les plus parfait accord, du moment qu'il consentira à supposer la valeur variable de l'excentricité de l'orbe terrestre exprimée par une série de cette forme $e'=f+f'\,\tau+f''\,\tau^2+\text{etc.}$, dans le cas où l'on emploie les formules ou la longitude vraie de la lune a été prise pour la variable indépendante, ou par une série de cette forme $e'=g+g'\,t+g''\,t^2+\text{etc.}$, dans le cas où l'on emploiera comme je l'ai fait dans ma théorie de la lune, les formules directes ou le temps t a été choisi pour cette variable. C'est donc à cette seule proposition que doit aboutir définitivement toute cette discussion, et je ne puis sur ce sujet que renvoyer à ce que j'ai dit dans ma note précédente (*Monthly Notices*, Juillet, 1859) et en dernier lieu dans les *Comptes Rendus de l'Académie des Sciences*, tome l. No. 18.

Paris, 19 *Mai*, 1860.

On the Importance of making Observations on Thermal Radiation during the coming Eclipse of the Sun. By William Thomson, Prof. of Natural Philosophy at the University of Glasgow.

(*Extract from a Letter to the Editor.*)

"Prof. Grant thinks I ought to call the attention of the Astronomical Society to the importance of making observations on thermal radiation during the eclipse of the sun, by writing to you on the subject.

"All that I have to suggest is, that differential observations on the effects experienced by three or four thermometers with their bulbs placed, one in the centre of the image of the moon, in the focus of a lens or speculum, and the others in different positions round her disk, during the time of total obscuration of the sun, would probably give decided results as to the difference of radiation from the moon, and from the portion of space round the sun. The 'photosphere,' which I suppose is always seen round the moon's disk during a total eclipse of the sun, and which must be due to her light reflected from atmosphere or dust round the sun itself, must radiate heat, and would probably show a sensible effect, as compared with radiation from an equal angular area of the moon.

"The thermometers should have their bulbs blackened with soot or lamp-black, and be all of the same size. The bulb of each should be considerably smaller than the sun's image. This, if the focal length of the lens or mirror is 57 inches, would be a circle half an inch diameter, so that very ordinary thermometers would answer. I would recommend spirits-of-wine thermometers for superior sensitiveness; but, perhaps, mercury, with a rather fine bore, would be sensitive enough, and surer.

"A mirror would be better than a lens, because the hot dust round the sun must produce radiant heat of such colour as that of a hot stone or metal not at a bright red heat; and which, therefore, would be much absorbed by glass.

"There would need good arrangements to preserve the thermometers from disturbing influences. Until the totality, the radiation from the sun would need to be strictly excluded. At the instant of complete obscuration the cover would be removed, and the thermometers all observed. One observer could note them in succession. The sketch represents the positions such as I would have the thermometers in, the circle denoting the moon's unseen disk.

"There seems no doubt but that the thing is practicable, and would lead to results; but it is to be considered whether arrangements can be made, and an observer or observers secured, to execute it. Will you consult members of the Astronomical

Society on the occasion of its meeting on Friday, if you find any who are disposed to consider the subject?

"From what Prof. Grant says, it appears that radiation experiments, as hitherto made or contemplated, have not attempted any definite concentration by means of a lens or otherwise. This appears to be as discriminating a way of investigating the phenomenon as it would be to attempt to record visible appearances by a photograph made by diffused light, with no lens to make a picture.

"*Birkenshaw Cottage, Thornliebank, Glasgow, June* 5, 1860."

Mr. Carrington has received the following letter from the Superintendent of the U. S. Coast Survey:—

"It may interest the members of the Royal Astronomical Society to know what progress has been made in the United States for observing the solar eclipse of July 18th.

"1. The Coast Survey will have the services of Lieut. Gilliss, U. S. N., to observe near Olympia or Steilacoom, in Washington Territory, the central line of shadow passing near both towns. Two assistants have been detailed for him, and he has been fully equipped for the work, and has taken passage in the last California steamer.

"2. The Superintendent of the *Nautical Almanac* will send one or two assistants to Cumberland House, near Lake Winipeg, having received the cordial co-operation of the liberal Governor of the Hudson's Bay Company in the matter.

"3. A resolution has passed the Senate of the United States, and awaits the action of the House of Representatives and of the President, authorising the Superintendent of the Coast Survey to send an expedition to the vicinity of Cape Chudleigh, where the central line of the shadow leaves the North American continent, to observe the eclipse. This will be composed of astronomers, and of a photographer volunteering for the expedition, and of the requisite assistants. The party is now making up, so as to be duly prepared if authority is given. Several institutions of learning and patrons of science have contributed the necessary funds for the incidental expenses of this expedition. (Signed) A. D. Bache.

"*Coast Survey Office, Washington, May* 22, 1860."

A letter from Lieut. Gilliss, of nearly the same date, states that he would leave New York on May 21, and expect to reach Puget Sound, *viâ* Panama and San Francisco, by June 22. He proposes to proceed from Steilacoom to an eminence of the Cascade Mountains, in order to command a good eastern horizon. The expedition to Cape Chudleigh would probably be under the command of Dr. Gould or Prof. Stephen Alexander.

M. Bulard, Director of the Observatory at Bou-zarea, near Algiers, has selected as his post of observation for July 18 the mountain Djebel Mhammel, in lat. 35° 31′, long. E. of Paris 3° 57′, nearly on the central line, and having an elevation of 7500 feet. The road lies from Phillipville through Constantine and Bathna.

On certain Babylonian Observations of the Planet Venus.

By the Rev. Dr. Hincks.

(*Inclosed in a Letter addressed to the Secretaries.*)

There is a tablet of baked clay in the British Museum, the inscription on which, if I interpret it aright, contains a series of observations of the planet *Venus*, and a series of predictions grounded on the observations. The latter are of no value; but the former may in great measure, if not altogether, determine the law by which the Assyrio-Babylonian lunar year was regulated in respect to its intercalary months. The knowledge of this law, again, will either establish or disprove the view which I have long entertained, and repeatedly expressed, that the era of Nabonassar was an astronomical, and not a political one; and, I may add, it is not impossible that it may furnish a test of the genuineness of the works attributed to Quthami and other supposed ancient Babylonian writers.

For these reasons I am desirous that the observations which I suppose to be recorded should be submitted to astronomers. I now offer two, which will suffice to test the correctness of my interpretation of the records. If any astronomer will take the trouble to calculate whether what is here stated to have happened would have actually happened, and will communicate the result to me, "Rev. Doctor Hincks, Rectory, Killyleagh," I will, if he desire it, communicate to him other records of observations; as to the interpretation of some of which I feel less confidence than I do as to these.

I observe that the Babylonian months are expressed by monograms, for which I substitute Hebrew names of months. The Babylonian day began at noon; and that day in the evening of which the new moon was first seen was considered to be the first day of the month. I suppose, but am not very confident, that the year of the first observation was −685. The month of Thamuz would begin in the spring. The second observation was some years later.

"On the 25th of Thamuz, *Venus* ceased to appear in the west, was unseen for seven days, and on the 2d of Ab was seen in the east."

"On the 26th of Elul, *Venus* ceased to appear in the west, was unseen for eleven days, and on the 7th of the second Elul was seen in the east."

This being an embolismatic year, the day last mentioned was necessarily its 184th day, and was 200 days before the first day of the new year. If, then, this day can be determined from what is recorded of *Venus*, the commencement of two Babylonian years out of a cycle of eight will be determined.

Killyleagh, Co. Down, 6 *June*, 1860.

Mr. Hodgson's attention having been drawn to the remarks of Mr. Dawes, in the *Monthly Notices* for April, upon the solar eyepiece, he wrote to him upon the subject, and in correspondence Mr. Dawes says he did not intend the remarks to have been printed, but merely wished to draw the attention of the Editor to the quotations he enclosed.

Mr. Hodgson, however, still maintains his claim for the effective novelty of the arrangement, which has never before been questioned by Mr. Dawes, nor in his paper upon solar eyepieces, read before the Society in 1851, is this or any similar arrangement even hinted at.

Comparison of Burckhardt's and Hansen's Lunar Tables with the Greenwich Observations from 1847 *to* 1858. By G. B. Airy, Esq. Astronomer Royal.

More than a year ago I communicated to the Society a comparison, in detail, of the places given for the moon by Burckhardt's and Hansen's Lunar Tables, with those given by the observations made at the Royal Observatory, Greenwich, for the year 1852; and I expressed the anxiety with which I looked forward to the completion of the comparisons for the whole period, from 1847 to 1858.

Since that time, by Mr. Hind's continued attention, the computation of the places from Hansen's Tables has been finished, and the whole have been placed in my hands; and I have now prepared the complete comparison of both Hansen's and Burckhardt's Tables with the observations.

The places deduced from observation are in all cases computed with Professor Adams' parallax.

The entire series of comparisons forms a rather extensive table, which I propose to print in the *Greenwich Observations*; but I have thought that an epitome of the results might not be unacceptable to the Society.

A general idea of the relative merits of the two collections of tables may be derived from an exhibition of their apparent extreme errors in + and —, in the two co-ordinates of longitude

and E.N.P.D., and for all the admitted observations made by both instruments, transit-circle and altazimuth; but in interpreting these extreme errors, it must be borne in mind that, in general, they are affected by extreme errors of observation with the sign opposite to that of the apparent error of tables. Suppose, for instance, that an ordinary extreme error of observation is 3″, then all the apparent maximum + errors of tables ought to be diminished numerically by 3″, and all the apparent maximum — errors ought to be diminished numerically by 3″; and these corrections will be the same for both Hansen's and Burckhardt's Tables. The effect of this application will be, to increase considerably the apparent inequality of merit of the tables.

A more accurate estimate will probably be formed by comparison of the sums of the squares of apparent errors. These, however, are on the whole increased by errors of observation, in such a manner as to make the apparent inequality of merit less than the true inequality.

In the tables which follow, the symbols B — O and H — O are used for the excess of the numbers derived from Burckhardt's and Hansen's Tables respectively above those derived from observation. The first table exhibits the extreme + and — values of B — O and H — O in each year, divided under the heads of Longitude and E.N.P.D., and also divided under the heads of Meridional and Altazimuth Observations. The second table exhibits the sums of squares of B — O and H — O for each year, and also their proportion, divided into similar groups; with the number of observations in each year.

It will be seen that this comparison for the twelve years entirely supports the opinion which I had formed on the relative merits of the tables from the comparison for the single year 1852.

Royal Observatory, Greenwich, 1860, *May* 30.

Maximum Positive and Negative Values of B — O *and* H — O, *in each Year, from* 1847 *to* 1858.

1847, *Meridional Observations.*

Longitude.				E.N.P.D.			
B — O.		H — O.		B — O.		H — O.	
+	—	+	—	+	—	+	—
July 29.	Feb. 28.	Oct. 29.	Aug. 20.	Sept. 26.	Nov. 30.	July 27.	July 29.
″	″	″	″	″	″	″	″
17·4	2·6	7·2	3·8	11·5	11·9	7·6	5·1

1847, *Altazimuth Observations.*

Nov. 10.	May 16.	June 30.	Dec. 23.	June 8.	May 24.	June 8.	Oct. 2.
″	″	″	″	″	″	″	″
24·8	6·3	11·0	8·9	16·5	15·1	7·3	7·6

1848, *Meridional.*

Longitude.				E.N.P.D.			
B—O.		H—O.		B—O.		H—O.	
+	—	+	—	+	—	+	—
Sept. 20.	Sept. 6.	Sept. 20.	Feb. 12.	Sept. 8.	Nov. 28.	June 11.	Oct 3.
′	′	′	′	′	′	′	′
19·1	1·9	8·0	8·1	9·2	13·4	8·8	9·0

1848, *Altazimuth.*

Sept. 12.	Mar. 31.	Sept. 12.	July 11.	Jan. 13.	Nov. 20.	Mar. 14.	May 24.
′	′	′	′	′	′	′	′
19·5	8·3	13·1	9·0	14·1	14·6	13·2	9·3

1849, *Meridional.*

Dec. 29.	Aug. 3.	July 8.	Mar. 2.	June 13.	Dec. 6.		Apr. 27.
′	′	′	′	′	′	′	′
16·7	3·3	7·4	10·7	15·9	12·7	...	6·7

1849, *Altazimuth.*

Sept. 19.	May 17.	Aug. 25.	Feb. 1.	June 13.	Dec. 5.	Feb. 28.	Nov. 4.
′	′	′	′	′	′	′	′
22·8	9.9	7·8	12·8	12·6	17·4	9·4	7·1

1850, *Meridional.*

Nov. 28.	Feb. 18.	Aug. 28.	{Jan. 23 / Feb. 21}	June 2.	Nov. 24.	June 2.	Oct. 11.
′	′	′	′	′	′	′	′
19·9	6·1	5·8	10·4	17·8	13·2	10·0	6·9

1850, *Altazimuth.*

Oct. 8.	Nov. 27.	Oct. 11.	Feb. 19.	June 5.	Sept. 4.	Apr. 18.	Sept. 2.
′	′	′	′	′	′	′	′
36·9	18·6	9·9	11·4	10·8	19·8	10·1	10·9

1851, *Meridional.*

Nov. 17.	Nov. 29.	Oct. 10.	Mar. 11.	Jan. 26.	May 10.	Aug. 3.	May 23.
′	′	′	′	′	′	′	′
21·7	3·6	7·0	9·6	9·6	10·7	6·0	6·4

1851, *Altazimuth.*

Oct. 26.	Apr. 25.	Oct. 26.	May 8.	Jan. 27.	Nov. 16.	Dec. 3.	Oct. 14.
′	′	′	′	′	′	′	′
43·3	7·3	11·1	8·6	9·6	12·9	9·1	8·7

1852, *Meridional.*

Sept. 29	{Jan. 25 / Feb. 25}	Aug. 7.	Apr. 27.	July 2.	Oct. 6.	Feb. 2.	Aug. 7.
′	′	′	′	′	′	′	′
18·2	1·7	8·6	9·2	8·3	10·8	4·6	4·5

1852, *Altazimuth.*

Longitude.				E.N.P.D.			
B—O.		H—O.		B—O.		H—O.	
+	—	+	—	+	—	+	—
Sept. 16.	Mar. 22.	Aug. 7.	Jan. 1 / Nov. 30	Jan. 18.	Oct. 11.	July 22.	Oct. 15.
″	″	″	″	″	″	″	″
37·0	7·3	8·9	10·9	11·5	14·5	8·0	11·1

1853, *Meridional.*

Sept. 16.	Oct. 23.	Oct. 26.	July 15.	Apr. 1.	Jan. 21 / Apr. 17	Jan. 31.	June 23.
″	″	″	″	″	″	″	″
22·9	4·1	6·0	9·6	14·1	9·5	5·0	7·9

1853, *Altazimuth.*

Sept. 5.	Mar. 11.	Dec. 16.	Nov. 26.	Sept. 16.	Sept. 5.	Sept. 16.	Feb. 1.
″	″	″	″	″	″	″	″
36·4	5·5	10·3	12·8	12·0	12·5	11·0	10·7

1854, *Meridional.*

Sept. 6.	Apr. 9.	July 12.	Sept. 28.	Apr. 18.	Jan. 8.	Apr. 1.	Sept. 28.
″	″	″	″	″	″	″	″
26·5	4·1	5·6	13·0	10·2	8·3	7·9	4·8

1854, *Altazimuth.*

Sept. 8.	May 9.	Oct. 8.	May 9.	Jan. 30.	Sept. 16.	Jan. 30.	July 14.
″	″	″	″	″	″	″	″
26·3	11·0	7·2	18·9	15·5	12·4	11·0	10·3

1855, *Meridional.*

Aug. 28.	Mar. 2.	Aug. 28.	Nov. 20.	July 25.	Dec. 20.	Dec. 21.	Dec. 29.
″	″	″	″	″	″	″	″
24·8	5·6	5·8	7·9	8·9	10·6	4·5	5·5

1855, *Altazimuth.*

Dec. 12.	Mar. 2.	Oct. 3.	July 19.	Dec. 12.	Dec. 20.	Dec. 12.	Oct. 4.
″	″	″	″	″	″	″	″
25·1	6·5	11·1	9·2	13·5	13·2	13·4	8·8

1856, *Meridional.*

Oct. 21.	May 18.	Oct. 21.	Apr. 16.	July 17.	June 26.	Apr. 9.	June 17.
″	″	″	″	″	″	″	″
19·0	0·2	7·8	6·7	8·0	11·5	2·8	5·6

1856, *Altazimuth.*

Jan. 10.	Mar. 30.	Sept. 22.	Aug. 10.	June 17.	June 28.	Jan. 10	Sep. 20. / Dec. 16.
″	″	″	″	″	″	″	″
22·4	7·8	6·5	10·7	9·2	15·7	6·6	9·3

1857, *Meridional.*

Longitude. B—O. +	Longitude. B—O. −	Longitude. H—O. +	Longitude. H—O. −	E.N.P.D. B—O. +	E.N.P.D. B—O. −	E.N.P.D. H—O. +	E.N.P.D. H—O. −
Oct. 9.	June 5.	Sept. 5.	Aug. 29.	Dec. 10.	June 15.	Feb. 15.	Mar. 16.
″	″	″	″	″	″	″	″
20·8	1·3	5·4	7·5	7·1	9·0	4·4	3·9

1857, *Altazimuth.*

July 23.	Apr. 20.	Oct. 7.	Mar. 28.	May 5.	June 14.	Oct. 4.	Aug. 12.
″	″	″	″	″	″	″	″
21·5	8·5	4·8	10·7	8·7	12·2	6·0	6·8

1858, *Meridional.*

Mar. 25.	Sept. 16.	Aug. 28.	Feb. 22.	Nov. 26.	Aug. 20.	Jan. 1	{June 1. Aug. 4.
″	″	″	″	″	″	″	″
21·7	3·9	4·8	9·0	8·1	9·5	4·5	7·9

1858, *Altazimuth.*

June 13.	June 7.	June {8 25}	June 7.	Dec. 1.	June 7.	Feb. 19.	Dec. 24.
″	″	″	″	″	″	″	″
23·8	10·8	3·7	10·6	7·8	15·9	6·7	7·8

Sums of Squares of B — O *and* H — O, *in each Year, from* 1847 *to* 1858, *and their Proportions; with the Number of Observations in each Year.*

1847, *Meridional Observations.*

Number of Observations.	Longitude. B—O.	Longitude. H—O.	E.N.P.D. B—O.	E.N.P.D. H—O.
107	7426·94	708·42	3174·96	564·05
	10·5		5·6	

1847, *Altazimuth Observations.*

126	11489·62	1616·07	4353·99	1130·72
	7·1		3·9	

1848, *Meridional.*

113	7662·82	1132·64	2366·69	810·21
	6·8		2·9	

1848, *Altazimuth.*

199	18880·81	2567·76	6497·61	1989·89
	7·3		3·3	

1849, *Meridional.*

Number of Observations.	Longitude. B—O.	H—O.	E.N.P.D. B—O.	H—O.
109	6724·43	1083·60	3577·12	1006·45
	6·2		3·6	

1849, *Altazimuth.*

217	15170·75	2428·15	8154·33	1678·73
	6·2		4·9	

1850, *Meridional.*

115	8384·50	1156·54	4337·91	673·75
	7·2		6·4	

1850, *Altazimuth.*

215	24451·41	2903·65	7896·17	2543·45
	8·4		3·1	

1851, *Meridional.*

110	11005·70	1383·80	2375·05	452·16
	8·0		5·3	

1851, *Altazimuth.*

209	31952·11	3052·09	6576·60	1812·79
	10·3		3·6	

1852, *Meridional.*

109	9552·94	1221·76	2417·09	276·22
	7·8		8·7	

1852, *Altazimuth.*

210	28366·06	2876·17	7116·04	1737·40
	9·9		4·1	

1853, *Meridional.*

107	9603·01	1312·16	2768·48	336·75
	7·3		8·2	

1853, *Altazimuth.*

194	23630·65	2856·70	6843·10	1748·47
	8·3		3·9	

1854, *Meridional.*

Number of Observations.	Longitude. B—O.	H—O.	E.N.P.D. B—O.	H—O.
121	13072·49	1672·84	2254·02	388·59
	7·9		5·8	

1854, *Altazimuth.*

204	23141·97	3431·22	5885·34	2405·24
	6·8		2·4	

1855, *Meridional.*

94	11395·78	723·55	1629·55	286·09
	15·5		5·7	

1855, *Altazimuth.*

187	20173·67	2304·23	5747·57	1859·22
	8·7		3·1	

1856, *Meridional.*

94	8863·97	881·59	1828·90	179·68
	10·1		10·2	

1856, *Altazimuth.*

182	16115·46	2488·05	6537·15	1718·45
	6·5		3·8	

1857, *Meridional.*

115	11994·96	1458·47	1295·12	229·29
	8·4		5·7	

1857, *Altazimuth.*

217	18940·23	4438·30	4702·54	1422·40
	4·3		3·3	

1858, *Meridional.*

130	16844·30	1795·76	2634·72	524·08
	9·4		5·0	

1858, *Altazimuth.*

201	21369·01	3440·31	5054·45	1735·24
	6·2		2·9	

Right Ascensions of the Moon and Moon-culminating Stars, observed at the Astronomical Observatory, Victoria. By R. L. J. Ellery, Esq., Director of the Observatory.

(*Communicated by the Astronomer Royal.*)

Date.	Object Observed.	App. R.A. h m s
1859.		
Nov. 7	☽'s 1st Limb	0 19 57·07
	ι Piscium	0 55 41·94
	ξ Piscium	1 6 26·58
9	β Arietis	1 46 56·03
	☽'s 1st Limb	1 58 19·60
1860.		
Feb. 2	υ^1 Tauri	4 17 57·85
	☽'s 1st Limb	4 26 39·16
	τ Tauri	4 33 52·46
	χ Orionis	5 46 7·90
6	☽'s 1st Limb	8 47 29·29
	83 Cancri	9 11 12·40
Mar. 3	☽'s 1st Limb	7 10 53·66
	☽ Geminorum	7 11 48·18
	υ Geminorum	7 27 20·23
4	☽'s 1st Limb	8 14 10·18
	η Cancri	8 24 39·20
	79 Cancri	9 2 20·92
5	79 Cancri	9 2 20·84
	83 Cancri	9 11 12·48
	☽'s 1st Limb	9 15 16·78
6	ω Leonis	9 52 51·50
	α Leonis	10 0 57·57
	☽'s 1st Limb	10 14 0·83
Mar. 7	φ Leonis	11 9 35·35
	☽'s 2d Limb	11 13 17·10
31	ι Geminorum	7 36 1·83
	φ Geminorum	7 44 57·78
	☽'s 1st Limb	7 48 7·52
	η Cancri	8 24 38·91
Apr. 1	ι Leonis	9 37 56·63
	☽'s 1st Limb	8 47 55·73
	ω Leonis	9 52 51·29
	α Leonis	10 0 57·41
2	☽'s 1st Limb	9 45 41·68
	ω Leonis	9 52 51·29
	α Leonis	10 0 57·30
5	χ Virginis	12 32 4·00
	☽'s 1st Limb	12 33 23·01
	ψ Virginis	12 47 7·28
7	☽'s 2d Limb	14 32 34·39
	56 Hydræ	14 39 37·89
	α^2 Libræ	14 43 11·11

Corrected for all errors and clock-rate.

Results of Meridional Observations of Small Planets; Occultation of a Star by the Moon; and Phenomena of Jupiter's Satellites: observed at the Royal Observatory, Greenwich, in the month of May 1860.

(*Communicated by the Astronomer Royal.*)

Calliope (22).

Mean Solar Time of Observation.	h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1860, May 21	12 51 24·9	16 50 54·76	113 45 15·45
23	12 41 45·4	16 49 6·79	113 49 24·29

Thetis (17).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1860, May 1	11 41 21·2	14 21 48·46	93 22 45·37
2	11 36 32·8	14 20 55·80	93 18 48·63
3	11 31 44·6	14 20 3·36	93 14 59·82
9	11 3 5·3	14 14 58·72	92 55 46·12
21	10 7 25·2	14 6 28·13	92 38 22·53
22	10 2 55·2	14 5 53·92	92 38 41·05
23	9 58 26·5	14 5 21·08	92 38 27·27
(*a*) 26	9 45 8·4	14 3 50·21	92 40 17·24
(*b*) 26	9 45 10·3	14 3 52·32	92 40 11·98

Mr. Dunkin observed two objects for *Thetis* on May 26, which were so close to each other, that it was impossible to decide which was the planet; and the weather, as well as the increasing daylight, prevented re-observation of the star on the meridian. Mr. Dunkin, however, thinks that the object marked (*b*) was the planet, as it agreed more nearly in magnitude with *Thetis* than the other.

All the observations of N.P.D. have, as usual, been corrected for parallax.

Occultation of a Star by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
			h m s	
1860, May 2	*q* Virginis, reapp.	Bright	7 49 32·2	C.

The star was invisible at the disappearance. It was somewhat separated from the moon's limb when first seen at the reappearance. The initial C. is that of Mr. Criswick.

Occultation and Transits of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time.	Observer.
1860.			h m s	
May 1	I	Occ. disapp. first cont. (*a*)	8 22 18·1	J. C.
1	I	,, central bis. (*a*)	8 23 47·9	J. C.
3	III	Ingress, bisected	9 55 11·1	E.
9	I	Egress, last cont.	10 2 39·0	C.

(*a*), The planet very tremulous and diffused.

The initials E., C., J. C., are those of Mr. Ellis, Mr. Criswick, and Mr. Carpenter.

Observations of the Occultation of Jupiter by the Moon, made at the Royal Observatory, Greenwich, 1860, May 24.

(*Communicated by the Astronomer Royal.*)

Phenomenon.	Greenwich Mean Solar Time of Observation. Hour and Minute.	Seconds observed by M.	D.
	h m	s	s
First Contact	4 34	49·0	...
Disappearance	4 35	53·4	54·2
Reappearance	5 46	17·6	12·6
Last Contact	5 47	20·4	20·4

Notes by Mr. Main [M.]. The observations were made with the Sheepshanks' equatoreal. At the reappearance of the first limb of *Jupiter*, the time recorded is probably a second or two late, the planet having emerged when first seen; all the other observations are perfectly satisfactory.

By Mr. Dunkin [D.]. The instrument used was the new S.E. equatoreal. At the first contact the occultation of the first limb of the planet evidently took place before I noticed any alteration in the appearance of *Jupiter* at $4^h\,34^m\,50^s$. The remaining observations are considered very satisfactory.

Notwithstanding the daylight and the excessive faintness of *Jupiter* at the reappearance at $5^h\,47^m$, I was surprised to notice the visibility of the belts, which appeared as several exceedingly thin parallel lines.

The satellites of *Jupiter* were not visible.

Observations of the Occultation of Jupiter by the Moon on the 24th May, 1860. By Thomas Gaunt, Esq.

(*Extract from a Letter to one of the Secretaries*).

"I send you the following account of the occultation of *Jupiter* by the moon on the 24th ult., as seen by me at Stoke Newington. The observation was made with an achromatic of 3·3 inches aperture, 50-inches focus, the immersion with a power of 50, and the emersion with a power of 70.

"At the immersion I could not see the dark limb of the moon until the planet appeared to touch it, and then only to the extent of the diameter of the planet; but what I was most struck with was the appearance on the moon as it passed over the planet. It appeared as though the planet was a dark object, and glided on to the moon instead of behind it, and the appearance continued until the planet was hid, when I suddenly lost the dark limb of the moon altogether. The equatoreal belts were visible up to the edge of the moon.

"At emersion the planet was but a little paler and greyer than the moon, and when about one-third out there appeared a dark broad line down the bright limb of the moon over the planet, and it remained visible until the whole of the planet was seen, it was about double the width of the equatoreal belts, but much darker. The belts were very distinct up to the dark band down the moon's bright limb.

"A lady observed the occultation at the same place with an achromatic of 3·25 inches aperture, 42 inches focus, and a power of 45, but she did not see the appearance I did at immersion, when the planet touched the moon the dark limb became visible to the extent of about three diameters of *Jupiter*, as though it was enlightened. At emersion she also saw the dark band down the bright limb of the moon. No distortion of the planet was noticed by either of us. The sun was very brilliant both at immersion and emersion.

"I enclose you a sketch of the appearances as I saw them.

"*June* 6, 1860."

Observations of the Occultation of Jupiter by the Moon, made at Highbury, 1860, *May* 24. By T. W. Burr, Esq.

		Local Sid. Time. h m s	Moon's Limb.
The Planet's 1st Limb disappeared at		8 44 6·7	Dark
,, 2d ,, ,,		8 45 4·9	
,, 1st ,, reappeared at		9 55 48	Bright
,, 2d ,, ,,		9 56 44·7	

The times of first contact and first reappearance are uncertain, and probably one or two seconds late. The observations of last contact in each case are very good. The clock-error was checked by a transit of *Regulus* immediately after the emersion. The planet was well seen, notwithstanding the strong sunlight ($4^h\ 34^m$ G. M. T.), but, of course, without any belts. The moon's dark limb could not be detected until it touched the planet, when it was seen very sharply defined and black; and as it passed, the disk of *Jupiter* in front appeared to brighten, so that the moon's limb was preceded by a bright band of light; doubtless an effect of contrast. At the reappearance nothing unusual was noticed, but the difference of hue and brilliancy between the planet and our satellite was very marked, *Jupiter* being, as noted in prior occultations, of a faint indigo tint.

The telescope used was by Ross, of $3\frac{1}{8}$ inches aperture; focal length, 52 inches; power, 58. Longitude, 23°·8 W.

Highbury, June 4, 1860.

Observations of Jupiter. By Sir William Keith Murray.

(*Extract from a Letter to one of the Secretaries.*)

"I enclose four diagrams of *Jupiter*,* selected from my note-book, showing some of the changes displayed in the belts.

"As really good definition is rarely to be expected, except for about half an hour before dark, the appearance of the belts varies exceedingly, according to the time of night they are observed.

"I find it generally hopeless to see the white spots or the minute detail of the belts at any other hour of the night, at this season and position of the planet. On April 26th, the third satellite appeared so dark that had I not looked in the *Nautical Almanac*, I should have had no doubt of its being the shadow, instead of the satellite.

"On May 1st I saw the spots and markings on the belts in great perfection. I never happened to observe so many on the northern belt; but I find it very difficult to make a perfectly faithful sketch of the position and dimensions of the spots; therefore the sketch must rather be considered to give a general idea of the appearance of the planet than an absolutely true representation. Even in the best states of the atmosphere there are flickerings which make it almost impossible to produce a perfect representation of the minute detail of the belts, as seen with the 9-inch object-glass, and powers of from 200 to 360.

"The weather, generally, this winter, in this quarter, has been most unfavourable for delicate observation for the last five months; there has, till lately, hardly been a night without frost; the waters have been covered for nearly four months with 7 or 8 inches of ice; and there are still deep wreaths of snow on the hills within two miles of the observatory.

Ochtertyre, Crieff, 11th May, 1860."

New Variable Star in Ophiuchus. By Norman Pogson, Esq., Director of the Hartwell Observatory.

(*Communicated by Dr. Lee.*)

A small star of the eleventh magnitude was detected with the Hartwell equatoreal, on April 16th, in a position in which most certainly no such object had been previously seen, although in a portion of the heavens regularly examined each spring since 1853.

It was at first supposed to be a new planet; but ten micro-

* These diagrams were exhibited at the Meeting.—Ed.

metric comparisons with a star north preceding established its fixity, and gave for its mean place, reduced to 1860,—

R.A. = $16^h\ 25^m\ 43^s{\cdot}85$, N.P.D. = $105°\ 49'\ 49''{\cdot}8$.

The comparison star employed was

A. Z. 205,89 = A. Z. 297,91 = 30055 Lalande,

and its assumed place for the same epoch,—

Mag. 7·7 R.A. = $16^h\ 24^m\ 39^s{\cdot}35$, N.P.D. = $105°\ 40'\ 43''{\cdot}9$.

Moonlight and cloudy weather prevented its changes from being closely watched; but the following records suffice to establish its variability, and to show that the maximum is of brief duration; a circumstance also corroborated by the numerous occasions on which, for eight successive years, I have swept this part for new planets, without having entered it upon my chart, although much smaller stars in the immediate vicinity are all duly inserted:—

1860, April 16,	11·0 mag.	May 26,	Quite gone, and under 12·5 mag.
17,	11·0	28,	,, ,, 13·0
20,	10·8	Carefully sought for on this last occasion,	
21,	10·5	under very favourable circumstances.	

The occultation of *Jupiter*, on May 24, was a complete failure here, owing to passing clouds, although two hours before the phenomenon the sky was so clear that the planet was well seen by Mrs. Pogson and a visitor, in full sunshine, with the 5-foot Smythian telescope, when the aperture of the object-glass was reduced to 1½ inch. Had it been as distinct with the equatoreal at the requisite moments, good observations might have been secured, instead of the two following dubious records, which are scarcely worth publishing, but all I could obtain with power 93:—

	h	m	s	
Disappearance of Jupiter's western limb	4	33	15	Greenwich M.T.
Reappearance ,, ,,	5	43	49	,,

Hartwell Observatory, June 6, 1860.

Note on a Grey Stripe extending from Burckhardt northward, and coincident with the Eastern Flank of Geminus, both Lunar Craters. By W. R. Birt, Esq.

The various tints reflected from the surface of the moon, considered apart from shadows, are dependent on three con-

ditions: some may result from the colour of the lunar surface, properly so called, arising from the material of which that surface may be composed, and the different tints thus reflected will be as numerous as the materials forming the surface; others may be dependent not so much on the surface itself as on various superficial coverings by which many parts of the surface may be obscured: if these coverings possess much continuity, and are sufficiently thick to obstruct or prevent reflection from the surface beneath, the colour of the superficial covering only will be reflected; while, on the other hand, if the superficial covering be scanty, leaving portions of the surface here and there bare, a mixture of colour will be produced, giving rise to a tint, the result of the blending of the two.

The mid-day illumination of *Geminus* reveals a stripe of a greyish hue which skirts the eastern border of the crater; it extends from *Burckhardt* to a small crater marked a* on the diagram illustrating my paper on the mid-day illumination of *Geminus* (*Report of the British Association for the Advancement of Science*, 1859, p. 33 of the Transactions of the Sections). It is the topography and general features of this stripe that I am at present desirous of settling, so far as my observations during the past summer and present spring will enable me to do. My observations are not yet sufficiently extensive to enable me to say if this stripe extends further, either in a northerly or a southerly direction.

Under the morning illumination of *Geminus*, the external shadow of the eastern wall of the crater is not so strong nor so well defined as the internal shadow of the western. About the middle portion of the eastern flank (externally) there are strong indications of a small crater: they are not however constantly visible at the same ages of the moon. One of the most important features connected with this stripe is the existence of a mountain chain on which the crater a* is situated: this is seen in bold relief, both under the morning and evening illuminations, and is probably equal in length to the diameter of *Geminus*. This mountain range, as such, disappears entirely under the mid-day illumination, the crater presenting a lucid ring with a *grey* interior. So far as my observations have yet proceeded, I am disposed to come to the conclusion that the grey stripe passes clear, or very nearly so, of the mountain range, just skirting its western edge: it is not visible under the *early* morning illumination.

Under a suitable angle of illumination, the mountain range presents a very interesting feature: it is a very *narrow* shadow bounding the western base, as if the entire mass rose from a depressed but very shallow basin, and by which it is isolated from the surrounding country.

There is a peculiarity of the grey stripe which merits attention: the external shadow of the eastern wall of *Geminus*, in the neighbourhood of the indicated crater, is replaced by the

darkest portion of the stripe; and long before the shadows are obliterated by the advance of the lunar day, the general direction of the stripe is seen; indeed the stripe itself appears much earlier than the features that characterise the mid-day illumination.

How far this stripe may be dependent on any of the above-named conditions future observations alone can decide. Next to the separation of *Geminus* into a dusky and lucid portion, and the lighter area with which the lucid portion is connected, it is the most interesting feature in the locality. If it derive its colour from the material forming the surface hereabout, it is likely to be persistent; but should it result from a *superficial* covering, there may be a probability of its undergoing some change in the lapse of time. The character of the markings eastward of it deserves attention.

Many portions of the moon's surface deepen in tint as the lunar day advances, particularly some of the large grey plains: the *Mare Serenitatis* is an instance; also the dark floor of the crater *Plato*, which, from having a dash of green under the early morning illumination, passes from a greenish through a blackish grey; and when the sun has just passed its meridian, the prevailing tint is a diluted inky black, with a branching oblique streak at the present epoch.

Observations of Donati's Comet, made at the Royal Observatory, Cape of Good Hope, between October 11th, 1858, and March 4th, 1859. By Sir Thomas Maclear, Director of the Royal Observatory, Cape of Good Hope.*

(*Communicated by the Astronomer Royal.*)

The clouded state of the sky prevented the detection of this splendid comet at the Cape Observatory before the 10th of October. On the evening of that day there was a complete view of it for a few minutes, afterwards the tail only could be seen through occasional breaks in the clouds. Rain fell on the forenoon of the 11th, after which the clouds dispersed, and as the evening twilight closed in, there is no exaggeration in the statement that the westward view from the observatory was magnificent beyond description. *Venus* in full splendour, the young moon, and the comet, presented an imposing display of celestial objects rarely witnessed, while the feelings were rendered more sensitive to the sublime and the beautiful by one of the serene evenings so delicious in the Cape climate when the atmosphere is calm and moist.

Observations for orbit being the primary consideration, and

* Ordered to be printed *in extenso* in the *Memoirs* of the Society.—Ed.

the time short for making them during the first few evenings of the appearance, the 8½-feet equatoreal could not be spared for elaborate physical examination. This, however, is of little importance, since more powerful instruments were available in Europe and America; and the decision has been verified by the published observations, up to the 20th of October, which have reached the Cape, and which are in accordance with our own so far as the latter were carried. After that date there was little to record beyond the gradual fading away of the image.

Two series of observations for orbit were made, viz., one by Mr. Mann, with the 8½-feet equatoreal, which comprehends the whole interval of visibility, commencing on the 11th of October and ending on the 4th of March following. The other with the 46-inch telescope by myself and G. W. H. Maclear conjointly, commencing on the 16th of October and ending on the 15th of November, when the comet became too faint to be observed with that instrument armed with a spider-line micrometer.

From the commencement of the observations with the 8½-feet equatoreal to Nov. 16th, the measures were taken with a position micrometer fitted with flat wires 10″ in breadth—the spider-line micrometer belonging to the instrument being then in Mr. Simms' hands for the execution of certain alterations. On November 17th it was received from England, and employed in observation up to December 29th, when the flat wire was again substituted, as the comet had then become too faint to bear the slightest illumination of the wires.

The powers generally employed were 126 with the flat wire micrometer, and 161 with the spider-line micrometer.

The differences in R.A. were obtained by direct micrometer measurement on the parallel, using the "driving clock;" except on November 18th and 22d, and December 11th and 14th, when the transits of both objects were observed with the system of fixed lines in the spider-line micrometer. The observations with the 46-inch telescope were made by means of a spider-line micrometer, having one line fixed and two moveable; and the differences in R.A. were measured in time by transits over the fixed wire—the differences in polar distance, by the micrometer screws at the instants of transiting the fixed wire.

The observations are of inferior value compared with those made with the more powerful instrument and the facilities afforded by its superior mounting; however, they are useful as vouchers for the interval they embrace, as the same stars were seldom used with both instruments.

Of the 114 stars of comparison, several are of very small magnitude—a matter of no moment with respect to relative accuracy of position, as the whole list has been observed with the large transit-circle, each star at least three times.

As the headings of the several columns are sufficiently explicit, and the observations are given in the usual form, no

further explanation is needed, other than that the corrections for refraction have been applied, and that the columns headed $\log \frac{p}{P}$, $\log \frac{q}{P}$, contain the logarithms of the parallax factors in right ascension and polar distance respectively.

Royal Observatory, Cape of Good Hope,
1860, April 20th.

Eléments de la Comète découverte à Olinda le 26 Février, 1860, d'après les Observations faites à l'Observatoire de la Commission Scientifique, du 26 Février au 13 Mars. Par M. Emm. Liais, Astronome à l'Observatoire Impérial de Paris en Mission Scientifique du Gouvernement Français, et Président de la Commission Scientifique chargée par le Gouvernement Brésilien de réviser la Carte de la Côte du Brésil.

(*Inclosed in a Letter addressed to the President.*)

Distance Périhélie	= 1·197342	
	° ′ ″	
Inclinaison	= 79 35 54·5	
Longitude du Nœud	= 324 3 25·4	Equinox Moyen, du 1 Janvier, 1860.
Longitude du Périhélie	= 173 45 21·1	

Passage au Périhélie le 16 Février, à $13^h\ 50^m\ 6^s·5$ Temps Moyen d'Olinda.
Sens du Mouvement, *Direct.*

Ces éléments s'appliquent à la grande nébulosité de la Comète, qui offrait le singulier aspect de deux nébulosités visibles à la fois dans le même champ. Ils représentent les observations avec une assez grande approximation. La plus grande différence sur l'A.R., est de $1^s·34$, le 10 Mars, et sur la déclinaison de 21″·2, le 12 Mars, 1ère comparaison.

La seconde nébulosité suivait la première,—

Le 27 Février, à $10^h·25$ de 25′ en A.R.
Le 3 Mars, à $11^h·16$ de 23′
Le 10 Mars, à $8^h·25$ de 21′

Sa Déclinaison S. était moindre les mêmes jours respectivement de 1′ 8″, 46″, et 21″.

Sa lumière était très foible, et a présenté des variations assez notables. Le 13 Mars, il a été impossible de l'apercevoir. Elle était circulaire, tandis que la grande nébulosité était allongée dans le sens du rayon vecteur au soleil.

Olinda, 12 Mai, 1860.

Notice of some Errors in Baily's Tables and Formulæ.

By J. O. Farrell, Esq.

The late Mr. F. Baily's *Astronomical Tables and Formulæ* (London, 1827) being a work of considerable reputation and of frequent reference by the practical and theoretical astronomers of this country; perhaps it may be useful to point out an error in one of the formulæ, as it has led the author to insert, in the *Elements of the Solar System*, an erroneous value of the excentricity of the earth's orbit. I refer to formulæ xlvi. (p. 119), the first two of which give the relations between the excentricity (e) and the greatest equation of the centre (E), viz.—

$$E = 2e + \frac{11}{2^4.3}e^3 + \frac{599}{2^{10}.5} + \frac{17219}{2^{16}.7} + \ldots.$$

$$e = \tfrac{1}{2}E - \frac{11}{2^4.3}E^3 - \frac{587}{2^{16}.3.5} - \frac{40583}{2^{23}.5.7.9} - \ldots.$$

The first equation is correct; but in the second equation the coefficient of E^3 should be $-\frac{11}{2^8.3}$, instead of $-\frac{11}{2^4.3}$. As the author appears to have followed Delambre very closely (see note, p. 229; also p. 274, line 2), it is very likely that these formulæ have been copied from Delambre's *Abrégé d'Astronomie*, p. 256, where the very same error occurs.

In pages 19 and 66 the excentricity of the earth's orbit (1 Jan. 1801) is stated to be ·016783568. The author, having remarked that it differs from that given by Laplace (·01685318), informs us that he has deduced this value from the greatest equation of the centre ($1^\circ\,55'\,27''{\cdot}3$), by means of the formula $\frac{1}{2}E - \frac{11}{48}E^3$. The correct formula, however, as above stated, is $\frac{1}{2}E - \frac{11}{768}E^3$; and hence the excentricity corresponding to the greatest equation of the centre here assumed is 0·016791707. That this is the proper value is confirmed by Bessel's value for the same epoch (·016791822 6), given in the Appendix, p. 270. With respect to Laplace's value of this quantity (·01685318), it would seem to be that corresponding to a previous epoch; but not having hitherto had the advantage of consulting any of Laplace's works, I am unable to verify the conjecture.

Again, assuming the values of greatest equation of the centre given in p. 66, I have calculated the corresponding values of the excentricities; they are inserted in the last column of the following table; from which it appears that values of these quantities, given by Mr. Baily, for *Mercury*, *Venus*, *Earth*, and *Mars*, are very inconsistent.

Planet.	Greatest Equation of Centre. Baily, p. 66.	Excentricity. Baily, p. 66.	True Excentricity corresponding to Greatest Equation.
	° ′ ″		
Mercury	23 39 51·0	·205514940	·20549252
Venus	0 47 15·0	·006860740	·00687220
Earth	1 55 27·3	·016783568	·016791707
Mars	10 40 50·0	·093307000	·09311252
Jupiter	5 31 13·8	·048162100	·04816264
Saturn	6 26 12·0	·056150500	·05615019
Uranus	5 20 57·0	·046679380	·04666863

The elements of the orbits of *Jupiter* and *Saturn* are very nearly consistent. The error in that of the *Earth* has been accounted for above; but in those of *Mercury*, *Venus*, and *Mars*, the inconsistency is so great that it is difficult to account for it without reference to the original works from which they may have been extracted or deduced.

As, however, these wrong or inconsistent excentricities have been inserted by Sir John Herschel in all the editions of his treatise on Astronomy (*Lardner's Cyclopedia*, new edition, 1842; *Outlines of Astronomy*, 3d edition, 1850), and thence into almost all our popular works on Astronomy, it seems very desirable that an authoritative synopsis of the elements of our system should be prepared, and thus prevent, if possible, the perpetuation of the erroneous elements referred to.

It may be worth while to notice, briefly, another error in the formulæ which also appears to have led the author and others into error.

In formulæ xliii. p. 116, for computing the relative length of a degree of longitude and latitude, &c. on the surface of the earth, the expression for the length of a degree of latitude is wrong; it should be multiplied by the length of a degree of latitude at the equator. This mistake makes all the numbers in the column "Degree of latitude" (Table xxxiv. p. 181) erroneous. This wrong formula also causes a similar error to exist in Table viii. appended to F. W. Simms's treatise on Mathematical Instruments (London, 1844), where the lengths in feet of a second of latitude are erroneous.

A somewhat similar error occurring in the last edition of Biot's *Traité Elémentaire d'Astronomie Physique* (Paris, 1857) may be noticed. I refer to the Table of the Elements of the Orbits of the Asteroids, given in tom. v. pp. 531, 532, of the above work, where the numbers inserted in the column "Moyen mouvements" are altogether erroneous.

Ordnance Survey Office, Southampton,
19th May, 1860.

Note on the History and Present Condition of the Problem "To find the Latitude by Observations of the Pole-star at any time." By John Riddle, Esq.

In the *Philosophical Magazine* for June 1822, the late Francis Baily, Esq. drew the attention of English astronomers to a method of deducing the latitude from altitudes of the pole-star at any time, which had been published by M. Littrow in 1817, in the *Zeitschrift für Astronomie*, vol. iii. p. 208: the formulæ being identical with those which occupy so prominent a place year after year in the preface to the *Nautical Almanac*. This paper is followed by "Further Remarks" by the same author in July 1822, in which the history of the problem is given, with the conclusion that, though it appears that a general discussion of such observations most probably originated in our own country, the first *correct* solution is that of M. Littrow.

Subsequently to these papers the problem was dignified with a very prominent place in the title-page and tables of Schumacher's *Ephemeris*, and after the change in the form of the *Nautical Almanac* (1835) Schumacher's tables were adopted in its pages.

Until 1845 the following statement relating to them is annually repeated in the preface:

"The tables for finding the latitude of a place by observations of the pole-star (*α Ursæ Minoris*) at any hour of the day *are similar to those published annually by Professor Schumacher in his Ephemerides of the Planetary distances*, and are founded, &c. &c."

In 1845 and afterwards the sentence in Italics is omitted; in all other respects the statement has remained unchanged to the present time. Whatever may have been the advantages afforded by these tables at the time that their celebrated author gave them so prominent a place, it has long been my opinion, founded on a larger experience of nautical matters than falls to the share of most people, that they do not worthily occupy the large space annually devoted to them in the *Nautical Almanac*.

The first table contains the values of

$$p \cos h,$$

calculated with mean values of the polar distance and right ascension of the star. This renders necessary Table III., which gives the corrections due to the fluctuations of the true values from the mean values; a minute or two being added to render the contents of the table positive; still further obscuring the principles of the problem.

The second table contains the value of

$$\frac{1}{2} (p \sin h)^2 \tan \text{alt.}$$

generally a comparatively small quantity, not sharing in any degree in the complements of Table III.

From their complicated and unpractical character I have long ceased to employ these tables, and I am sure that they are rarely if ever used by anybody.

In order to obtain the results given in the example of their use which is also annually furnished in the explanation of the contents of the *Nautical Almanac*, a troublesome interpolation is necessary with Table I., and, if possible, a more vexatious one with Table III.; while the *Nautical Almanac* furnishes the means of finding the correct value of $p \cos h$ by the addition of two logarithms. These two tables deserve no place in the *Nautical Almanac;* they contribute to obscure the problem, and afford very little if any facility to its solution.

The second table which gives

$$\frac{1}{2}(p \sin h)^2 \tan \text{alt}.$$

might with advantage be extended and given for shorter intervals of sidereal time, and rendered more compact by condensing the minutes and seconds into seconds only.

I should prefer the meridian distance (h) for the argument, instead of the sidereal time, as showing to students more directly the connexion between the table and the formula on which it depends.

The late Dr. Inman gave in his Tables the total mean correction with latitude and sidereal time as arguments; this, as an approximation, may not be without its value to seamen who prefer the "rough and ready" to more laborious accuracy, and, it must not be omitted, in a sketch of the present position of this interesting problem, that the Board of Admiralty have recently ordered the re-calculation of Dr. Inman's table, the tables contained in the *Nautical Almanac* for the same purpose notwithstanding.

A Memoir on the Problem of the Rotation of a Solid Body.

By A. Cayley, Esq.*

The present memoir was written for the sake of the further elaboration of the analytical theory of the rotation of a solid body, upon principles similar to those of my "Memoir on the Problem of Disturbed Elliptic Motion," *Mem. R. Ast. Soc.* vol. xxvii. pp. 1–29 (1858); the like elements are adopted, and the course of the investigation corresponds precisely to that of

* Ordered to be printed *in extenso* in the *Memoirs* of the Society.—Ed.

the memoir just referred to. The formulæ for the variations of the elements in the two problems (the motion being in each case referred to a fixed plane of reference and origin of angles therein) are found to be, as it is known they should be, identical in form; an investigation, in the present memoir, of the transformation of the system to the case of a variable plane of reference and departure-point as an origin of angles in such plane, would have been a mere repetition of that contained in the former memoir, and it was therefore unnecessary to give it. A point in the present memoir to which attention may be called is the definition of the angle g (varying uniformly with the time, but used as an element) which corresponds to the mean anomaly in elliptic motion. Besides the ultimate system of formulæ for the variations of the elements in terms of the differential coefficients of the disturbing function with respect to the elements, it appears to me that the intermediate formulæ for the variations in terms of the differential coefficients with respect to the co-ordinates (which are in the ordinary investigation altogether passed over) are not without interest, and that it is possible that they might be employed with advantage in the integration of the equations of motion for the purposes of physical astronomy.

M. Otto Struve, present at the meeting, directed the attention of the Society to the results of his measures made on artificial double stars. These researches prove that the angles of position of close double stars, as measured by M. Struve with the filar micrometer of the great Pulkowa refractor, are subject to systematic errors, depending for their greater part on the angle formed by the direction of the two stars with the vertical. The amount of these errors, expressed in linear distances, exceeds for some directions considerably that of the constant or systematic errors, as they have been found to exist in measures of distances, either by direct observation of artificial stars or by comparison of measures made by different observers. Of course these angular errors must exercise a great influence on the elements of orbits of double stars, when calculated from M. Struve's observations, and, therefore, it would be almost irrational not to take account of them in the computation of orbital motions. But there might arise the question, whether the same formula, which has been deduced by M. Struve from observations made in nearly a horizontal direction, ought to be likewise applied to observations nearer the zenith. To decide this question M. Struve has made during the last five years a series of observations on northern double stars under very different horary angles; which, though not yet

completely reduced, leave him no doubt that the coefficients of the variable members of the formula of correction are pretty nearly the same for all elevations above the horizon. But these observations cannot answer for the first member of the formula, which, being independent of the angle with the vertical line, varies merely with the distance or the visual angle (the distance multiplied by power) of the two compared stars. With a view of inquiring into this matter, a series of simultaneous observations has been made on different nights in the present spring by M. Struve himself and six of his collaborators at the Observatory of Pulkowa, — MM. Döllen, Wagner, Winnecke, Schiaparelli, Oom, and Oblomievski. The results of these observations were exhibited to the Society. They show that, after application of the corrections by the given formula, the angles measured by M. Struve agreed in all instances so nearly with the mean of the measures of the other observers, that the remaining differences nowhere exceed the order of admissible accidental errors. At the same time these measures exhibit very strikingly similar systematic errors, but following different laws for several other observers. Hence it must be concluded, 1st, that the correction formula, as deduced from M. Struve's observations of artificial double stars, has its full value for all inclinations of the telescope; 2d, that, though perhaps in M. Struve's case, the systematic errors are extraordinarily great, analogous errors may be supposed to exist more or less for all other observers who follow the same rules of observation as himself.

In pointing out these results, M. Struve remarked that it could hardly be expected that every observer of double stars would have the time, and might wish to spend so much time and labour on measures of artificial double stars as he had done himself; but certainly every one would be anxious to ascertain the existence and amount of similar errors in his own observations. For this purpose he suggested that approximate values of these errors might be deduced if the observer would take care to note the sidereal time of his observation and the magnifying power, and if, at the same time, he would attend to the rule, that for all observations his head was kept as nearly as possible in a vertical position. In that way each longer series of observations would certainly offer a considerable number of measures that might be compared with others for which the correction formula is deduced from direct measures on artificial double stars; and when made for the same objects under very different horary angles, would probably contain in themselves sufficient data for an approximate deduction of the variable members of the correction formula.

From what he had said M. Struve derived the general conclusion that only when the measured angles were thus cleared from their inherent systematic errors, they really would merit, in comparison with the distances, that preference which

many astronomers have been in the practice of attributing to them for the deduction of the elements of orbital motions. In his opinion many discrepancies between theory and observation that have been pointed out by several computers, would disappear when the indicated corrections were applied.

In connexion with the preparations for the solar eclipse, M. Otto Struve availed himself of the opportunity to acquaint the English astronomers with the contents of two Reports on the observations of the eclipse of 1851, one of which never has been published; the other only in the Russian language.

The first of these Reports was from the late Major-General Wrontchenno, a man who, with extraordinary qualifications for accurate observation, joined a rare combination of different talents. Having chosen his station in the plains of Texaterinoslaw, on the shores of the Dnieper, General Wrontchenno, being also an eminent painter, directed his attention particularly to the colours of the prominences, and was enabled to ascertain, during the few minutes of total eclipse, at least in one of the prominences a gradual transition from white, through rose, to dark pink. This observation is of a particular interest, as it indicates that the reason why on different stations quite different colours have been indicated for the same prominences, is to be attributed to the nature of the prominences. Therefore M. Struve recommended this sort of observations to the attention of amateur astronomers, who, not being sufficiently provided with means for exact measures, would by a careful repetition of this kind of observation render a valuable service to science, and perhaps would essentially contribute to explain the nature of the prominences.

The other Report was from Colonel (now Major-General) Chodzko, the Surveyor-General of the Caucasian provinces, who for his station had selected an isolated mountain of the Caucasus, called Galawdour, about 11,000 feet high. At this height Mr. Chodzko saw the same prominences nearly in the same position on the moon's disk, and of the same dimensions and shape, as they have been seen on the most occidental stations of the central line (in Norway, Prussia, &c.), with only the difference that he judged their colour to be yellowish. This observation seems to contradict quite decidedly the supposition maintained by some astronomers, that the prominences are formed by clouds in the atmosphere of the earth, which certainly cannot be supposed to be of the same dimensions and figure, and therefore cannot give the same appearances, on the level of the sea and 11,000 feet above it. As to the corona, it appears from Mr. Chodzko's drawing that at such a height it was considerably less extended than has been observed at lower stations. Hence it might be concluded that the phenomena of the corona are in a great measure produced by our atmosphere.

After having exposed the principal contents of these two

papers, M. Otto Struve placed a verbal translation of the original Reports at the disposition of the Society, for reference on future occasions.

Lescarbault's Planet.

Mr. Ellery, Director of the Victoria Observatory, in a letter dated 1860, April 24, states that he had half-hourly observations of the sun made throughout the period indicated, without success.

Major Tennant, Director of the Madras Observatory, states that, with the exception of times when clouds prevented observation (which he gives in detail), the sun's disk was watched every few minutes from March 27 to April 10, without success. (Date of letter, April 13, 1860.)

Mr. Scott, Director of the Observatory at Sydney, states that "he looked for Lescarbault's Planet without success." (Date of letter, April 16, 1860.)

The following letters have been received in relation to the recently observed Comet.

Mr. J. M. Stothard, in a letter dated Dublin, 25th June, writes:—

"I observed the expected comet on last Friday and Saturday nights, at about 11¼ P.M., through a chink in the clouds, the rest of the sky being dark and starless, it was situated a little to the west of north, and most probably was in the constellation of *Auriga;* it was much brighter and more readily visible to the naked eye than Donati's Comet during the first few nights of its appearance. On Saturday night I observed it through my 3-inch refractor, and for a moment saw it pretty well with a distinct tail, with a power of 60."

Colonel De Rottenburg, in a letter dated Gibraltar, 24th June, writes:—

"I observed a brilliant comet in the constellation of *Auriga* this evening, a line from β through α *Geminorum* continued about three times their distance passed through its nucleus. It was very easily seen by the naked eye, the nucleus was a little less brilliant than *Castor.* Its altitude above the western horizon about equal to that of *Venus* at the same time. I turned my 6-feet equatoreal 4⅛ inches aperture, by Messrs. Cooke and Sons of York, on the comet. It has a bifid tail, very like that of the year 1846, as shown in Keith Johnstone's *Atlas of Astronomy*, which is edited by Mr. Hind. I used

powers of 26, 50, and 100 on the comet; the nucleus has a very sensible disk. It bore powers up to 100 very well; one portion of the tail is much longer than the other, the south preceding being the longer; I give rough drawings of the comet's appearance in the telescope.

"With 100 power the nucleus was situated within the nebulosity, and the nebulosity was more arched and prominent on the south preceding part. It was first seen by a gentleman here on Saturday evening 23d June.

"I have been examining the part of the heavens where it is situated lately, but did not see it till this evening."

Mr. Edwin Clarke, in a letter dated Observatory House, Forest Hill, 26th June, writes:—

"The comet was seen here on Sunday evening about $10^h{\cdot}25$ for a few minutes extremely well. As my large equatoreal unfortunately does not sweep north-west on account of houses, I lost my time in endeavouring to mount a 4-foot equatoreal out-of-doors. I had a brilliant view through the 4-inch for a minute or two only, when the clouds again covered it, the sky was extremely clear at this moment, and the nucleus was very sharply defined, having a large distinct disk, the brush was vertical nearly below the head. It was but slightly divergent and of great length. It was a very striking object to the naked eye, and of great brilliancy. As far as I can estimate it was in the constellation *Auriga*, about 10 degrees west of *Capella;* but this is a mere guess, as I had no time for any measurement either of the nucleus or position, and no recognisable stars were visible, as the clouds did not again clear away. I was equally unsuccessful last night, the motion is evidently extremely rapid, judging from the positions given by others, and unless visible by daylight, which is possible, it will I fear be only seen for a few evenings longer. The head of the disk was peculiarly bright and well defined."

MISCELLANEOUS INTELLIGENCE.

The Rev. R. Main, President of the Society, has been by the Board of Trustees of the Radcliffe Observatory at Oxford appointed Radcliffe Observer.

Mr. Main will be succeeded in the office of First Assistant at Greenwich by E. J. Routh, Esq., Fellow and Tutor of St. Peter's College, Cambridge.

RECENT PUBLICATIONS.

Mr. Stanford, of No. 6 Charing Cross, has lately published a reissue of the small edition of star-maps on the gnomonic projection, as originally prepared by Sir J. Lubbock for the Society for the Diffusion of Useful Knowledge. In the present edition the advantages of this very convenient arrangement of course remain, but the maps are now rather encumbered by the introduction of the constellations, which belong rather to the heavens of the ancients than to our own times. When introduced at all, it is desirable that these fanciful figures should, as the *Uranometria* of Professor Argelander, be very lightly drawn in outline, so as not to overlie the stars themselves. The details of the Milky Way are still very imperfectly given, as in the first edition, although the Southern Catalogue of Sir J. Herschel has some years since supplied the means of a far better delineation. Mr. Stanford has also an edition of the same maps printed in lithograph, in which the trajectories of the principal comets of this century have been drawn by Mr. James Breen. Both editions are sold at the same price, 3*s*. plain and 6*s*. with the constellations coloured.

The first volume of the Catalogue intended to accompany Professor Argelander's Charts has appeared, and contains nearly 111,000 positions of stars, arranged with the greatest possible condensation. The remark chiefly called for, before the completion of this great work for the Northern Hemisphere, now so rapidly and successfully progressing, is, as Professor Argelander has himself indicated, that a similar catalogue or series of maps is highly desirable for the Southern Hemisphere.

Observaciones Astronomicas hechas en el Observatorio Nacional de Santiago de Chile en los Años de 1853, 1854, *i* 1855. Por el D^{r}. Carlos Guill^{o}. Moesta, Director del Observatorio. t. I. 4^{o}. Santiago de Chile, 1859.

The formation of the Observatory dates from the 17th August, 1852, when the Government of the Republic purchased as national property the instruments and other articles brought to Chili by the United States Astronomical Naval Expedition under the command of Lieut. Gilliss. Other instruments have since been acquired or ordered, and a proper building for the observatory is in course of construction; but the temporary observatory consists of two wooden houses (one of them containing the meridian circle, the other the equatoreal), brought ready made from North America, and placed by Lieut. Gilliss

on the Cerro de S. Lucia, where they still remain, and where all the observations published in the volume have been taken. The Cerro de S. Lucia, formerly called Cerro de Huelen, is a crag rising in the centre of the city, abruptly from the plane, and composed of a metamorphic porphyry of columnar structure, very susceptible of atmospheric influences. The eastern view of the rock, with the two houses thereon, is shown in the vignette to the volume: the situation, exposed to all the noise and dust of a crowded city, and on account of the want of stability of the instruments, is stated to be an unfavourable one.

There is an endowment for an Assistant, and for two youths to study astronomy at the observatory: these are from time to time chosen by the Government from the pupils of the National Institute who have most distinguished themselves in mathematics. For want of a proper person, the post of Assistant was not filled up during the period of the observations in the volume; and all the meridian observations, and the greater part of the reductions, were made by the Director himself. The Director is, *ex officio*, Professor at the University, and gives there alternately an annual course on the differential and integral calculus, and on practical astronomy.

A chief part of the proposed plan was to take meridian observations of the stars. In the first place, it was necessary to collect data for the exact determination of the geographical position of the observatory; and for this purpose, particular attention was paid to the culminations of the moon, and to the occultations of stars by the moon, and observations were made of the zenith distances of the fundamental stars, and of various circumpolar stars. The other meridian observations contained in the volume relate chiefly to stars selected in the following order:—

1st. The southern stars of the British Association Catalogue, the positions of which appear to be doubtful or erroneous, a long list of which is given at the end of that catalogue.

2d. The stars of the catalogue of Lacaille (*A Catalogue of* 9766 *Stars in the Southern Hemisphere*, &c. London, 1847.) not observed since the time of Lacaille, or at least those which are not to be found in the catalogues of Brisbane, Taylor, and Johnson.

3d. The stars up to the 9th magnitude situate between the parallel of 62 degrees south and the zenith.

Besides the specified stars, meridian observations were made of the following planets:—*Venus*, *Mars*, *Ceres*, *Juno*, *Pallas*, *Vesta*, *Hebe*, *Iris*, *Parthenope*, *Psyche*, *Proserpine*, *Amphitrite*, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*.

The Director intends to devote special attention to the observations of *Venus*, believing that in time they will furnish a contribution to the more exact determination of the sun's parallax, regard being had to the favourable position of the observatory, as compared with those in the other hemisphere.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XX. *Supplemental Notice.* No. 9.

Observations sur un Calcul de M. Delaunay inséré dans les Additions à la Connaissance des Temps pour 1862, *et relatif à la Détermination du Coefficient de l'Equation Séculaire du Moyen Mouvement de la Lune.* Par M. le Comte G. de Pontécoulant.

L'auteur de la note dont il s'agit s'est proposé de déterminer par la méthode de la *variation des constantes arbitraires*, le coefficient de *l'équation séculaire* du moyen movement lunaire, en n'ayant égard qu'aux termes dépendants du carré de la force perturbatrice, et de vérifier par ce moyen la valeur attribuée à ce coefficient par M. Adams dans son mémoire bien connu de 1853. Mais M. Delaunay n'a point atteint le but qu'il s'était fixé, et nous avons démontré ailleurs,* que la *prétendue* identité qu'il avait cru exister entre le résultat auquel il était parvenu et celui de M. Adams, n'était que l'effet d'une singulière illusion, et n'avait été obtenue que par une fausse application de la méthode qu'il avait employée, jointe à une valeur erronée attribuée à la quantité *m*, par laquelle tous les géomètres, qui ont traité depuis Laplace la théorie de la lune, ont constamment désigné le rapport du moyen mouvement du soleil à celui de la lune, donné par l'observation. Nous nous proposons ici de montrer comment l'application de ces mêmes formules *de la variation des constantes arbitraires*, dont la première idée est due au grand Euler, et dont la théorie a été ensuite si heureusement perfectionnée par Lagrange, Laplace, et Poisson, en se conformant rigoureusement aux principes qu'ils ont posés, reproduit *identiquement* les différents termes du coefficient de *l'équation séculaire* du mouvement moyen de la lune, que nous avons présentés dans le Nombre des *Monthly Notices* du mois

* *Comptes Rendus de l'Académie des Sciences* (Séance du Lundi, 16 Juillet, 1860).

de Mai dernier.* Nous esperons que cette parfaite concordance entre des résultats déduits de deux méthodes si différentes, dissipera les doutes que la persistance de M. Adams à soutenir l'exactitude d'une analyse dont nous avons eu plusieurs fois l'occasion de signaler le défaut, aurait pu laisser dans quelques esprits.

Si l'on désigne comme à l'ordinaire par a, e, ε, ϖ, γ, θ, les six élements de l'orbite elliptique de la lune, ou de l'orbite qu'elle décrirait autour de la terre sans l'action du soleil, par R la fonction perturbatrice provenant de cette cause, qu'on représente de plus par n le moyen mouvement qui répond au demi-grande axe a, en sorte qu'on ait $a^3 n^2 = 1$, en supposant qu'on prend pour unité la somme des masses de la terre et de la lune, on aura pour déterminer la variation de la constante ε, on de ce que les géomètres appellent la longitude *de l'époque*, résultante de l'action des forces perturbatrices, la formule suivante :†

$$d\varepsilon = -2a^2 n\,dt\left(\frac{d\mathrm{R}}{da}\right) + \frac{a n\,dt}{2}\left(e\frac{d\mathrm{R}}{de}\right) + \frac{a n\,dt}{2}\left(\gamma\frac{d\mathrm{R}}{d\gamma}\right). \quad (a)$$

Nous négligeons pour plus de simplicité les termes qui dépendraient des puissances de l'excentricité et de l'inclinaison de l'orbite lunaire, d'un ordre supérieur au carré.

Bornons d'abord les approximations aux termes d'un ordre inferieur à m^4.

En dévéloppant la fonction R, soit par la méthode ordinaire, soit par la méthode de la variation des constantes arbitraires (voir *Théorie Analytique*, liv. 7, Nos. 6 et 97), et en observant qu'on a généralement $\left(a\frac{d\mathrm{R}}{da}\right) = 2\mathrm{R}$, on trouvera

$$a^2\frac{d\mathrm{R}}{da} = \left(\frac{9}{8}m^2 + \frac{825}{32}m^3\right)e^2e'^2 + \left(-\frac{9}{8}m^2 + \frac{93}{32}m^3\right)e'^2\gamma^2.$$

On a d'ailleurs (*Théorie Analytique*, liv. 7, Nos. 6 et 97),

$$\left(e\frac{d\mathrm{R}}{de}\right) = \left(\frac{9}{8}m^2 + \frac{825}{16}m^3\right)e^2e'^2,$$

$$\left(\gamma\frac{d\mathrm{R}}{d\gamma}\right) = \left(\frac{9}{8}m^2 + \frac{33}{16}m^3\right)e^2\gamma^2.$$

Si l'on substitue ces différentes valeurs dans la formule (a), on aura

$$d\varepsilon = \left\{\left[\left(-\frac{9}{4}+\frac{9}{16} = -\frac{27}{16}\right)m^2 + \left(-\frac{825}{16}+\frac{825}{32} = -\frac{825}{32}\right)m^3\right]e^2e'^2 + \left[\left(\frac{9}{4}-\frac{9}{16} = \frac{27}{16}\right)m^2 + \left(-\frac{93}{18}+\frac{33}{32} = -\frac{153}{32}\right)m^3\right]e'^2\gamma^2\right\}n\,dt.$$

* No. 7, vol. xx.

† *Théorie Analytique du Système du Monde*, livre 7, No. 93.

Portons maintenant l'approximation jusqu'aux termes de l'ordre m^4, en négligeant pour plus de simplicité les termes dépendants de l'excentricité et de l'inclinaison de l'orbe lunaire. Il suffira de supposer dans ce cas (*Théorie Analytique*, liv. 7, No. 102)

$$d.\delta\iota = -2\,dt\,\delta.a^2 n\left(\frac{dR}{da}\right) + \frac{dt}{2}\delta.an\left(e\frac{dR}{de}\right). \qquad (a')$$

On a d'ailleurs

$$\delta.a^2 n\left(\frac{dR}{da}\right) = an\delta.\left(a\frac{dR}{da}\right) + \delta an.\left(a\frac{dR}{da}\right), \qquad (b)$$

$$\delta.an\left(e\frac{dR}{de}\right) = an\delta.\left(e\frac{dR}{de}\right) + \delta an.\left(e\frac{dR}{de}\right).$$

Le dernier terme de la seconde équation ne pouvant produire que des quantités dépendantes de l'excentricité e, que nous négligeons, on peut en faire abstraction.

L'équation ordinaire $\left(a\frac{dR}{da}\right) = 2R$ donne évidemment $\delta\left(a\frac{dR}{da}\right) = 2\delta R$; on peut calculer cette dernière valeur en employant la méthode de la variation des constantes arbitraires, mais il sera plus simple de la deduire des résultats que nous avons obtenus par le dévéloppement ordinaire de la fonction perturbatrice. Nous avons trouvé (*Théorie Analytique*, liv. 7, Nos. 6 et 45), en n'ayant égard qu' aux termes que nous voulons considérer ici

$$aR = \frac{m^2}{4} - \frac{179}{96}m^4 + \left(\frac{3}{8}m^2 - \frac{799}{64}m^4\right)e'^2,$$

(a) dans cette formule désigne le demi-grand axe lunaire dans l'orbite troublée, a désignant ce que devient (a) relativement à l'orbite elliptique, on aura évidemment

$$aR = \frac{a}{a}(aR).$$

Mais nous avons trouvé par une première approximation (*Monthly Notices*, vol. xx. p. 272),

$$n = n\left(1 - m^2 - \frac{3}{2}m^4 e'^2\right),$$

d'où, en observant que $\frac{a}{a} = \left(\frac{n}{n}\right)^{\frac{2}{3}}$, on conclut

$$\frac{a}{a} = \left(1 - \frac{2}{3}m^2 - m^2 e'^2\right).$$

En multipliant par ce rapport la valeur précédente de a R, on trouvera la suivante

$$a\,\mathrm{R} = \frac{m^2}{4} - \frac{65}{32}m^4 + \left(\frac{3}{8}m^2 - \frac{831}{64}m^4\right)e'^2,$$

et l'on en conclura

$$an\,\delta.\left(a\,\frac{d\,\mathrm{R}}{da}\right) = -n\left[\frac{65}{16}m^4 + \frac{831}{32}m^4e'^2\right].$$

Considérons maintenant dans le dévéloppement de la fonction R (*Théorie Analytique*, liv. 7, No. 6) les termes suivants

$$a\,\mathrm{R} = m^2\,\mathrm{F}\cos 2\xi + m^2\,\mathrm{F}'\,e'\cos(2\xi - \varphi') + m^2\,\mathrm{F}''\,e'\cos(2\xi + \varphi'),$$

où l'on représente, pour abréger, par ξ la longitude moyenne de la lune moins celle de la soleil, par φ' l'anomalie moyenne de ce dernier astre, φ désignant l'anomalie correspondante de la lune, en sorte qu'on a

$$\xi = (1-m)\,nt + \varepsilon - \varepsilon'; \quad \varphi = mt + \varepsilon - \omega; \quad \varphi' = mnt + \varepsilon' - \omega'.$$

Les formules connues de la variation des constantes arbitraires, donnent pour déterminer la variation du grand axe, la formule suivante (*Théorie Analytique*, liv. 7, No. 87),

$$\delta a = 2a^2\int d'\mathrm{R}. \qquad (m)$$

En observant que l'on a $\int d'\mathrm{R} = \int\left(\frac{d\,\mathrm{R}}{n\,dt}\right)n\,dt$, en vertu de la valeur précédente de R on trouvera

$$a\int d'\mathrm{R} = m^2\left[\mathrm{F}\cos 2\xi + \mathrm{F}'\,e'\cos(2\xi - \varphi') + \mathrm{F}''\,e'\cos(2\xi + \varphi')\right].$$

Si l'on substitue cette valeur dans la formule (m) et qu'on combine la valeur résultante avec celle de la fonction $a\,\frac{d\,\mathrm{R}}{da}$ déduit de l'équation $\left(a\,\frac{d\,\mathrm{R}}{da}\right) = 2\,\mathrm{R}$, en n'ayant égard qu' aux termes non périodiques, on aura

$$\left(a\,\frac{d\,\mathrm{R}}{da}\right)\delta a = 2m^4(\mathrm{F}^2 + \mathrm{F}'^2e'^2 + \mathrm{F}''^2e'^2).$$

On a d'ailleurs (*Théorie Analytique*, liv. 7, No. 6)

$$\mathrm{F} = \left(1 - \frac{5}{2}e'^2\right); \quad \mathrm{F}' = \frac{21}{8}; \quad \mathrm{F}'' = -\frac{3}{8}.$$

Par conséquent

$$\left(a\frac{dR}{da}\right)\delta a = \left[\frac{9}{8}m^4 + \left(-\frac{45}{8}+\frac{441}{32}+\frac{9}{32}=\frac{135}{16}\right)m^4e'^2\right],$$

d'où en observant que $\delta a\, n = -\frac{1}{2}n\,\delta a$ on conclura

$$\left(a\frac{dR}{da}\right)\delta a\, n = -n\left[\frac{9}{16}m^4 + \frac{135}{32}m^4e'^2\right].$$

À l'aide de cette valeur et de celle de $an\delta\left(a\frac{dR}{da}\right)$, trouvée plus haut, on formera la suivante

$$\delta.an\left(a\frac{dR}{da}\right) = -n\left[\left(\frac{65}{16}+\frac{9}{16}=\frac{37}{8}\right)m^4 + \left(\frac{831}{32}+\frac{135}{32}=\frac{483}{16}\right)m^4e'^2\right].$$

Occupons nous maintenant de calculer la valeur de la fonction $\delta.\left(e\frac{dR}{de}\right)$. On a généralement

$$\delta.\left(e\frac{dR}{de}\right) = e\,\delta.\frac{dR}{de} + \frac{dR}{de}\delta e.$$

Soit

$$\begin{aligned} a\,R = {} & m^2 G e\cos\varphi + m^2 G' e\cos(2\xi-\varphi) + m^2 G'' e\cos(2\xi+\varphi) \\ & + m^2 H e e'\cos(\varphi-\varphi') \qquad + m^2 H' e e'\cos(\varphi+\varphi') \\ & + m^2 K e e'\cos(2\xi-\varphi-\varphi') \qquad + m^2 K' e e'\cos(2\xi-\varphi+\varphi') \\ & + m^2 K'' e e'\cos(2\xi+\varphi-\varphi') \qquad + m^2 K''' e e'\cos(2\xi+\varphi+\varphi'), \end{aligned}$$

d'où l'on tire

$$a\left(\frac{dR}{de}\right) = m^2 G\cos\varphi + m^2 G'\cos(2\xi-\varphi) + m^2 G''\cos(2\xi+\varphi) + \text{etc.}$$

Pour former la fonction $a\,\delta.\left(\frac{dR}{de}\right)$ il suffira donc de faire varier dans cette valeur la constante ω, qui se trouve comprise dans l'angle $\varphi = c\,n\,t + \varepsilon - \omega$, mais en observant qu'en n'ayant égard qu'aux termes que nous considérons dans R, on a $e\frac{dR}{de} = R$, on en conclura $e\frac{d^2R}{de\,d\omega} = \frac{dR}{d\omega}$, et par conséquent

$$\delta.\left(e\frac{dR}{de}\right) = \frac{dR}{de}\delta e + \left(\frac{dR}{e\,d\omega}\right)e\,\delta\omega. \qquad (n)$$

Les formules de la variation des constantes arbitraires

(*Théorie Analytique*, liv. 7, No. 87), en négligeant les termes qui seraient d'un ordre supérieur, par rapport à l'excentricité et à l'inclinaison de l'orbe lunaire, à ceux que nous considérons, donnent

$$\delta e = -\int \left(\frac{dR}{e\,d\omega}\right) n\,dt; \qquad e\,\delta\omega = \int \left(\frac{dR}{de}\right) n\,dt.$$

En substituant pour $\frac{dR}{e\,d\omega}$ et $\frac{dR}{de}$ les valeurs qui resultent de l'expression précédente de R et intégrant les formules résultantes, on trouve

$$\begin{aligned}\delta e = {} & m^2 G \cos\varphi - m^2 G' \cos(2\xi-\varphi) + \tfrac{1}{3} m^2 G'' \cos(2\xi+\varphi)\\ & + m^2 H e' \cos(\varphi-\varphi') \qquad + m^2 H' e' \cos(\varphi+\varphi')\\ & - m^2 K e' \cos(2\xi-\varphi-\varphi') \qquad - m^2 K' e' \cos(2\xi-\varphi+\varphi')\\ & + \tfrac{m^2}{3} K'' e' \cos(2\xi+\varphi-\varphi') \quad + \tfrac{m^2}{3} K''' e' \cos(2\xi+\varphi+\varphi').\end{aligned}$$

On aurait la valeur de $e\,d\omega$ en changeant simplement dans cette expression les cosinus en sinus et les signes des trois termes multipliés respectivement par G', K, et K'.

Si l'on substitue ces valeurs et celles de $\left(\frac{dR}{de}\right)$ et de $\left(\frac{dR}{e\,d\omega}\right)$ dans la formule (n), en n'ayant égard qu'aux termes non périodiques, on trouvera

$$a\,\delta.\left(e\frac{dR}{de}\right) = m^4\left[G^2 - G'^2 + \tfrac{1}{3}G''^2 + e'^2(H^2 + H'^2 - K^2 - K'^2 + \tfrac{1}{3}K''^2 + \tfrac{1}{3}K'''^2)\right].$$

Pour réduire cette formule en nombres, observons que par le dévéloppement de R (*Théorie Analytique*, liv. 7, No. 6) on a

$$G = -\tfrac{1}{2}\left(1+\tfrac{3}{2}e'^2\right);\ G' = -\tfrac{9}{4}\left(1-\tfrac{5}{2}e'^2\right);\ G'' = \tfrac{3}{4}\left(1-\tfrac{5}{2}e'^2\right),$$

d'où l'on tire

$$G^2 - G'^2 + \tfrac{1}{3}G''^2 = \left(\tfrac{1}{4} - \tfrac{81}{16} + \tfrac{3}{16} = -\tfrac{37}{8}\right) + \left(\tfrac{3}{4} + \tfrac{405}{16} - \tfrac{15}{16} = \tfrac{201}{8}\right)e'^2.$$

On a ensuite (même No.)

$$H = -\tfrac{3}{4};\ H' = -\tfrac{3}{4};\ K = -\tfrac{63}{8};\ K' = \tfrac{9}{8};\ K'' = \tfrac{21}{8};\ K''' = -\tfrac{3}{8},$$

d'où l'on conclut

$$H^2+H'^2-K^2-K'^2+\frac{1}{3}K''^2+\frac{1}{3}K'''^2=\left(\frac{9}{16}+\frac{9}{16}-\frac{3969}{64}-\frac{81}{64}+\frac{147}{64}+\frac{3}{64}=-\frac{957}{16}\right).$$

En vertu de la deuxième des formules (b), on aura donc

$$\delta.an\left(e\frac{dR}{de}\right)=an\delta.\left(e\frac{dR}{de}\right)=$$

$$n\left[-\frac{37}{8}m^4+\left(\frac{201}{8}-\frac{957}{16}=-\frac{555}{16}\right)m^4e'^2\right].$$

Si l'on substitue maintenant à la place de $\delta.an\left(a\frac{dR}{da}\right)$ et de $\delta.an\left(e\frac{dR}{de}\right)$ leurs valeurs dans la formule (a'), on aura

$$d.\delta\iota=\left[\left(\frac{37}{4}-\frac{37}{16}=\frac{111}{8}\right)m^4+\left(\frac{483}{8}-\frac{555}{32}=\frac{1377}{32}\right)m^4e'^2\right]ndt.$$

En joignant à ces termes ceux qui résultent de la première approximation, et qui ont été déterminés précédemment (*Théorie Analytique*, livre 7^e, No. 94) on aura donc pour la valeur complète de $d.\delta\iota$,

$$d.\delta\iota=\left[-m^2+\frac{111}{16}m^4+\left(-\frac{3}{2}m^2+\frac{1377}{32}m^4\right)e'^2\right]ndt. \qquad (a)$$

Si l'on réprésente par ζ le moyen mouvement lunaire dans l'orbite elliptique, ou (ce qui revient au même) si l'on fait $\zeta=\int ndt$, on aura pour déterminer la variation de ζ dans l'orbite troublée, la formule (*Théorie Analytique*, liv. 7, No. 92)

$$d.\delta\zeta=-3andt\int d'.\delta R+\frac{3a^2ndt}{2}\left(\int d'R\right)^2. \qquad (c)$$

Le premier terme ne peut produire des termes périodiques dans l'ordre d'approximation auquel nous nous arrêtons, conformément au théorème de l'invariabilité des grands axes planétaires ; considérons donc uniquement le second terme de cette formule. Nous avons trouvé précédemment

$$a\int d'R=m^2\left[F\cos 2\xi+F'e'\cos(2\xi-\varphi')+F''e'\cos(2\xi+\varphi'\right],$$

et, par suite, en n'ayant égard qu'aux termes non périodiques

$$e^2\left(\int d'\,\mathrm{R}\right)^2=\frac{m^4}{2}\left(\mathrm{F}^2+\mathrm{F}'^2e'^2+\mathrm{F}''^2e'^2\right).$$

En substituant cette valeur dans la formule (c), on aura donc

$$d.\delta\zeta=\frac{3}{4}m^4\left(\mathrm{F}^2+\mathrm{F}'^2e'^2+\mathrm{F}''^2e'^2\right)n\,dt,$$

ou bien, en vertu des valeurs précédentes de F, F′ et F″,

$$d.\delta\zeta=\left(\frac{27}{64}m^4+\frac{405}{125}m^4e'^2\right)n\,dt.$$

En joignant à cette valeur les termes correspondants de $d.\delta\epsilon$, on aura donc

$$\frac{d.\delta\zeta}{n\,dt}+\frac{d.\delta\epsilon}{n\,dt}=-m^2+\left(\frac{111}{16}+\frac{27}{64}-\frac{471}{64}\right)m^4$$

$$+\left[-\frac{3}{2}m^2+\left(\frac{1377}{32}+\frac{405}{128}-\frac{5913}{128}\right)m^4\right]e'^2.$$

En réunissant à cette expression les termes de la valeur de $d.\delta\epsilon$ qui dépendent des produits $e^2e'^2$, et $e'^2\gamma^2$, qui ont été déterminés précédemment, et en observant que ν représentant la longitude vraie de la lune dans l'orbite troublée, on a $\nu=\int n\,dt+\delta\zeta+\delta\epsilon$, et par conséquent $\frac{d\nu}{dt}=n\left(1+\frac{d.\delta\zeta}{n\,dt}+\frac{d.\delta\epsilon}{n\,dt}\right)$, que l'on a supposé d'ailleurs que l'on avait par le résultat des observations, $\frac{d\nu}{dt}=\mathrm{n}$; on trouvera enfin

$$\mathrm{n}=n\left[1-m^2+\frac{471}{64}m^4-\left(\frac{3}{2}m^2-\frac{5913}{128}m^4\right)e'^2\right.$$

$$-\left(\frac{27}{16}m^2+\frac{825}{32}m^3\right)e^2e'^2$$

$$\left.+\left(\frac{27}{16}m^2-\frac{153}{32}m^2\right)\gamma^2e'^2\right],$$

valeur qui coïncide identiquement avec celle que nous avons présentée dans le No. 7, vol. xx. p. 272, des *Monthly Notices*, et dans l'ordre d'approximation auquel nous nous arrêtons. La concordance de ces deux résultats obtenus par des méthodes tout à fait différentes montre l'exactitude de nos calculs.

Elements of Comet II., 1860. By Dr. H. Seeling, Assistant at the Glasgow Observatory.

I send the results of a short computation concerning the Comet 1860 (II.), in which, for a more accurate determination of the orbit, I availed myself of the observations at Kremsmünster, published in No. 1268 of the *Ast. Nach.*, these observations most likely extending quite as far as the comet could be observed at all.

The three positions on which I based my computation are,—

1860.	Greenwich M.T.	Geoc. Long.	Lat.
		° ′ ″	
Hamburg.....	April 17·42332	54 44 52·6	+30 46 44·5
Kremsmünster...	May 6·35057	74 30 22·2	+32 49 33·1
,,	,, 23·39302	91 26 19·9	+32 50 2·8

Reduced already to the Mean Equinox 1860·0, and free from aberration and parallax.

The variation of the curtate distances furnishes the following elements:—

T 1860, March 5·71694, G.M.T.

$$\left.\begin{aligned} \pi - \Omega &= 41\ 19\ 56''.4 \\ \Omega &= 8\ 56\ 8''.5 \end{aligned}\right\} \text{Mean Eq., 1860·0.}$$

$$i = 48\ 13\ 3''.8$$

$$\text{Log } q = 0·1167062.$$

Motion direct.

The first and last positions are, of course, accurately represented. For the middle position the direct computation gives—

	C. – O.
	″
In long.....	0·0
In lat.	+ 1·2

From this it can be inferred, with some probability, that the parabola suffices for the representation of the existing observations within the limits of their faults.

I add the logarithms of the curtate distances for the first and last observations, which might perhaps be applied to a definitive determination of the orbit:—

$$\text{Log } \varrho = 0·2515281$$

$$\text{Log } \varrho_2 = 0·2955208$$

Glasgow Observatory, July 30, 1860.

Comet III., 1860.

The conspicuous comet, of which passing notice was taken at the end of the last Number of the *Notices*, appears to have been first seen by Professor Caswell from the deck of the steamship Arabia on June 20, while on his voyage to England, and first observed on the 21st of June, and announced by Mr. Tuttle, of the observatory of Harvard College, Massachussetts.

Several communications have been received relating to this comet.

Professor Bond in a letter dated Harvard College, Mass. (U.S.), June 27, 1860, writes:

"Long before this reaches you the new comet will have been seen in Europe: it was discovered at this observatory on the 21st, at 9 P.M., by Mr. H. P. Tuttle. The nucleus is as bright as a star of the fourth magnitude, with a tail of 6° or 7°, visible to the naked eye. Changes are going on in its physical aspect resembling those exhibited by the great Comet of 1858.

"On the evening of the 26th the nucleus was surrounded on its sunward side by a faint dark halo, 1″ or 2″ broad, distant 18″ at its apex. Within it, in the south preceding quadrant of the envelope, was an irregular clouded mass, while the nucleus itself presented the appearance of a streak of very bright nebulosity, curiously elongated in the direction of right ascension. On the 22d it had been round, and only 2″·5 in diameter; on the 26th it was spindle-shaped, the longer axis 10″ or 15″, the shorter 3″. The greater part of this change occurred between the 25th and the 26th.

"The dark hollow of the axis of the tail, the contrast in brightness of the two branches of nebulosity issuing from the head, the luminous jets from the nucleus, and the envelopes, afford interesting points of comparison with the great Comet of 1858."

Mr. W. Scott in a letter dated Observatory, Sydney, July 20th, 1860, writes:

"On the 6th of July a large comet was seen in different parts of Australia, setting soon after the sun. I was not aware of its discovery, the weather at Sydney being cloudy, until the 9th, when I immediately made all the preparation in my power for its observation. The only instrument at my disposal for such purposes (until the arrival of an 8-inch refractor, which has been ordered) is an old 3¼-inch telescope on a Smeaton's block in a very dilapidated state, and with no serviceable micrometer. I, therefore, had a ring micrometer made of rather

large dimensions, in order that I might always get the comet and some tolerably bright star to pass through the field within a short interval. The time of transit of an equatoreal star across the diameter is $99^s{\cdot}71$.

"On the 9th the evening was cloudy, but I caught sight of the comet for a few minutes not far from *Saturn*, but before I could make an observation it was obscured by clouds. The comet was then a brilliant object, and being near the horizon its tail extended upwards at an angle of about 60°.

"On the following day the comet could not be seen; on the 11th it was again visible for a very short time, but no micrometrical measures could be made; by a comparison with *α Leonis* its R.A. was $10^h\ 18^m\ 22^s$, and Declination $11°\ 24'$ at $6^h\ 32^m$, Sydney mean time.

"On the 12th, 13th, 15th, 16th, 17th, 18th, a number of transits were taken, the results of which I now send. On the 18th the comet was still visible to the naked eye, guided by the telescope. Throughout the whole series of observations I have been unable to distinguish any marked peculiarity in its appearance. There was a decided brilliant nucleus, but not sufficiently well defined to form a good subject for observation, around which the light appeared diffused very uniformly. In consequence of the want of definition of the nucleus I consider all the transits to be liable to errors of at least one second, I have, therefore, in computing the declinations rejected all observations in which the time of transit exceeded 90^s. After this precaution has been adopted, it will be seen from the results that the determined right ascensions are more to be relied on than the corresponding declinations. On the 19th I had just succeeded in finding the comet with the telescope when it became obscured by clouds and was no more visible that night. On the 20th the comet was again hidden by clouds; rainy weather has now set in, and I fear I shall not be able to obtain any more observations."

The observations accompanying Mr. Scott's letter are as follows:—

Results of Observations of Comet and Stars.

Sydney Mean Time.	Star.	Comet — Star R.A.	Comet — Star Decl.
d h m s		m s	′ ″
July 12 6 14 1·5	48 Leonis	+2 5·25	+17 32
6 22 43·5	,,	+2 9·00	+16 26
7 18 54·5	,,	+2 35·25	Too near diameter of micrometer.
7 25 45·25	,,	+2 39·75	
7 32 30·25	,,	+2 41·25	
7 37 43·50	,,	+2 44·00	
7 42 51·25	,,	+2 47·50	+4 28

Sydney Mean Time.				Star.	Comet — Star R.A.	Comet — Star Decl.	
d	h	m	s		m s	′ ″	
July 12	7	48	6·00	48 Leonis	+2 55·00	− 3 [illegible]	
	7	53	31·75	,,	+2 53·00	+ 2 [illegible]	
	8	3	4·25	,,	+2 55·25	− 1 [illegible]	
	8	8	50·75	,,	+2 59·00	− 0 [illegible]	
	8	13	53·00	,,	+3 0·50	− 0 [illegible]	
	8	19	51·50	,,	+3 4·00	− 0 46	
	8	25	53·00	,,	+3 6·00	,,	
	8	31	0·00	,,	+3 8·75	− 2 39	
13	6	23	23	34 Sextantis	+5 31·50	−14 53	
	6	32	15	,,	+5 36·00	,,	
	6	39	22	Star of 7th or 8th magnitude not in any of the catalogues in my possession. Approximate R.A. 10h 42m 12s Decl. 4° 10′ N.	−1 5·00	−20 32	
	6	44	44		−1 3·00	+19 49	
	7	18	54		−0 49·25	+14 18	
	7	25	12		−0 46·00	−14 21	
	7	28	9		−0 44·25	+14 22	
	7	31	2		−0 43·25	,,	
	7	34	14		−0 41·75	,,	
	7	37	14		−0 40·25	,,	
	7	42	17		−0 37·75	,,	
	7	45	24		−0 36·50	+11 43	
	7	50	13		−0 34·25	+12 7	
	7	53	7		−0 33·00	,,	
	7	56	5		−0 32·50	,,	
	7	59	15		−0 29·75	,,	
	8	3	33		−0 29·00	,,	
15	6	26	19	61 Leonis	+7 20·40	−13 20	
	6	38	58	,,	+7 26·50	,,	
	7	20	31	,,	+7 46·25	−21 15	
	7	49	40		+0 52·00	−11 23	
	8	4	28		+0 57·75	−13 31	
	8	20	51		+1 4·50	−14 49	
	8	24	7	Star of 8th magnitude. Approximate R.A. 11h 1m 44s Decl. 1° 58′ S.	+1 6·00	−15 47	
	8	27	12		+1 7·00	−16 20	
	8	31	0		+1 9·25	,,	
	8	34	37		+1 11·00	−16 56	
	8	37	44		+1 12·25	−17 35	
	8	40	57		+1 11·75	−17 48	
	8	44	25		+1 16·00	−18 35	
	8	47	25		+1 16·50	−18 46	

Star since identified with No. 1109, Hour 10, in Bessel's Zones, Weisse 1846.

Sydney Mean Time. d h m s	Star.	Comet — Star R.A. m s	Comet — Star Decl. ′ ″	
July 16 5 58 43		+0 25·50	+12 56	
6 6 28		+0 28·25	,,	
6 20 49		+0 35·00	,,	
6 23 38		+0 35·00	,,	
6 26 55		+0 36·75	,,	
6 30 4		+0 39·00	,,	
7 2 42		+0 50·25	,,	
7 10 7		+0 54·50	+ 2 6	
7 13 11		+0 55·25	+ 1 37	
7 16 24		+0 57·00	+ 1 18	
7 25 49		+1 1·00	+ 0 17	
7 30 16		+1 1·50	+ 0 35	
7 34 26		+1 4·00	− 0 24	
8 14 14		+1 19·50	,,	
8 23 21		+1 23·25	,,	
8 28 40		+1 24·50	,,	
8 32 16		+1 27·00	,,	
8 37 26		+1 30·50	,,	
17 6 52 3	Weisse,	6 4·75	,,	
7 48 14	No. 250,	6 25·00	+ 2 9	
9 2 11	Hour 11.	6 52·25	,,	
18 6 5 42		−1 23·50	− 4 9	
6 8 55		−1 22·50	,,	
6 12 29		−1 20·15	,,	
7 0 42		−1 4·25	,,	
7 4 29		−1 3·50	−12 3	
7 7 46		−1 1·40	−12 6	
7 10 48		−0 59·50	,,	
7 14 27		− 58·75	,,	
7 17 37		− 58·10	−13 28	
7 20 44		− 57·75	−13 44	
7 23 29		− 56·60	−14 2	
7 29 55		− 53·25	−14 25	
7 32 52		− 52·00	−14 24	
7 35 56		− 51·00	−15 0	
7 38 46		− 49·75	−15 25	
7 41 28		− 48·75	−15 48	
7 44 9		− 48·25	−15 44	

Star of 8th magnitude. Approximate R.A. 11h 12m 5s Decl. 5° 10′ S.

Weisse, No. 544, Hour 11.

Star of comparison believed to be No. 190, Hour 11, Weisse.

Mr. R. J. L. Ellery, Director of the Observatory, Williamstown, in a letter dated Victoria, July 24th, 1860, writes:—

"I enclose a few places of a comet that made its appearance here on the 7th ultimo. Our only extra-meridional instrument (the 18-inch altazimuth) was unfitted for observation during its apparition, so that we could only obtain sextant observations. Such as they are, I believe the observations to be as correct as is possible with such an instrument.

"The comet was very bright at its first apparition; its tail extended over about four degrees the first night; the head appeared very nebulous throughout the time of its visibility here."

Date. 1860.	h m	Comet's R.A. h m s	Comet's Declination. ° ′ ″
July 10	6 8	10 6 5·6	14 43 57 N.
11	6 40	10 18 22·5	11 10 16 N.
12	6 6	10 29 50·4	8 3 0 N.
13	6 35	10 41 20·3	4 25 30 N.
15	7 36	11 2 54·2	2 11 40 S.
16	7 15	11 12 33·7	5 11 14 S.
18	7 43	11 31 1·7	10 47 16 S.
19	8 3	11 39 36·9	13 20 0 S.
20	7 38	11 47 38·3	15 35 36 S.

Capt. T. Spratt in a letter to Admiral Smyth, dated H.M.S. Medina, Crete, 3 July, 1860, writes:—

"I suppose the twilight in England has prevented your seeing the new comet, which promises from its first appearance to be a splendid one; but between twilight and moonlight (now advanced to nearly full) its tail has become less and less distinct to us.

"It was first noticed on board the Medina, on June 25th, and was visible to the naked eye for about 20 minutes only, the light of the moon, as it succeeded the twilight, obliterating its visibility; and as the moon's power increased every subsequent night, the comet became less and less distinct; so that now it is hardly visible, except the nucleus, which increases in size, although changing its position slowly, and appears about equal to a star of the fourth or fifth magnitude.

"On the night of the 25th, when it was first seen during the short interval of dusk between twilight and moonlight, at 8·40 P.M. apparent time of place, in lat. 35° 23′, long. 24° 28′, off the town of Retuno, Crete, the tail appeared to be about 15 degrees in length, and having a slight curve, but inclined towards the two Bears in a direction nearly midway between the Pointers and Pole-star. The following were its angular measurements:—

To Pole-star.....	47°·18
Saturn.....	43·13

"On the 26th, at the town of Candia, in lat. 35° 21′, long. 25° 8′, its angular distances were, 48° 17′ to Pole-star; ditto to *Saturn*, 42° 5′, or 41° 43′, being indistinct for measurement with sextant.

"At Spina Longa, on July 1st, in lat. 35° 15′ and long. 25° 48′, its angle from Pole-star, at 8·30 P.M., was 54°."

The following Elements were computed by M. Powalky, from observations of June 23, 26, and 29:—

T 1860, June 16·0395, Berlin M.T.

π	161° 26′ 23″·5	App. Equinox, June 26.
ϑ	84 47 17	
i	79 19 6	
Log q.....	9·465480.	

Motion direct.

Elements slightly different from the above, obtained by several computers, are published in the *Astronomische Nachrichten* and in Gould's *Astronomical Journal*.

Discovery of Minor Planet ⑯.

M. Leverrier's *Bulletin* of September 14 contains the observations of this new Minor Planet by M. Chacornac, at Paris, on the 12th and 13th September.

	Paris M.T.	R.A.	Decl.	Comps.
	h m s	h m s	° ′ ″	
Sept. 12	13 43 28·07	0 39 34·72	.....	8
	13 46 8·68	.....	+1 9 25·9	6
13	13 49 32·87	0 38 59·55	.....	3
	14 3 43·64	.....	+1 0 35·8	4
	14 27 54·96	0 38 58·49	.....	3

Equal to a star of 9th to 10th magnitude.

Approximate Elements of Minor Planet ⑯, *deduced from observations extending from Sept.* 12 *to Sept.* 19. By Mr. W. Ellis, Assistant at the Royal Observatory, Greenwich.

Mean Anom.	15° 51′ 43″	Sept. 15, 12h G.M.S.T.
Long. of Perih.	337 51 49	Mean Equinox 1860·0
,, Node	167 8 47	
I	6 35 55	
φ	11 19 18	
Log a	0·4158610	
μ	843″·753	

Approximate Ephemeris, calculated from the Elements above.

For Greenwich Mean Midnight.

1860.	Apparent R.A.	Apparent N.P.D	Log. Δ
	h m s	° ′	
Sept. 12	0 39 37·9	88 49·8	0·0592
13	39 2·9	88 58·7	
14	38 26·9	89 7·7	
15	37 50·0	89 16·7	0·0567
16	37 12·2	89 25·9	
17	36 33·5	89 35·1	
18	35 53·9	89 44·3	0·0551
19	35 13·5	89 53·5	
20	34 32·5	90 2·8	
21	33 51·0	90 12·2	0·0542
22	33 8·8	90 21·5	
23	32 26·2	90 30·8	
24	31 43·5	90 40·2	0·0542
25	31 0·6	90 49·5	
26	30 17·5	90 58·9	
27	29 34·2	91 8·2	0·0551
28	28 50·5	91 17·3	
29	28 6·4	91 26·2	
30	27 22·2	91 35·0	0·0567
Oct. 1	26 38·7	91 43·9	
2	25 55·4	91 52·7	
3	25 12·6	92 1·4	0·0592
4	24 30·2	92 9·9	
5	23 47·9	92 18·3	
6	23 6·5	92 26·5	0·0625
7	22 25·7	92 34·6	
8	21 45·3	92 42·5	
9	21 5·9	92 50·3	0·0666
10	20 27·3	92 57·8	
11	19 49·4	93 5·2	
12	19 12·6	93 12·4	0·0715
13	18 36·8	93 19·4	
14	18 1·9	93 26·1	
15	17 28·3	93 32·7	0·0771
16	16 55·9	93 39·0	
17	16 24·4	93 45·1	
18	15 54·5	93 50·9	0·0834
19	15 26·0	93 56·5	
20	14 58·7	94 1·8	
21	14 32·9	94 7·0	0·0902

1860.	Apparent R.A.	Apparent N.P.D.	Log. Δ
	h m s	° ′	
Oct. 22	14 8·6	94 11·8	0·0902
23	13 45·6	94 16·4	
24	13 24·2	94 20·8	0·0976
25	13 4·4	94 25·0	
26	12 46·0	94 28·8	
27	12 29·4	94 32·4	0·1056
28	12 14·4	94 35·7	
29	12 0·9	94 38·8	
30	0 11 49·2	94 41·6	0·1139

By an observation on Sept. 25, the ephemeris was found to be still correct.—G. B. AIRY.

Discovery of Minor Planet (80).

M. Leverrier's *Bulletin* of Sept. 23 contains the communication of this discovery by letter, dated Sept. 20, from Châtillon-sous-Bagneux.

The new star was first seen by M. Goldschmidt at 8h P.M. on the 9th, at 49 seconds east of the star Lalande 44384, and recognised as a planet on the 19th, when it appeared equal to a star of the 11th magnitude.

The following approximate positions are given:—

1. For the star:—

	R.A.	Decl.
	h m s	° ′ ″
Lalande 43981	22 24 8	3 37 0

2. For the planet:—

Sept. 19	8h 2m Par. M.T.	22 25 43·83	3 48 43

Motion in the 10 Days.

In R.A. — 10 minutes of time.

In Decl. + 24 minutes of arc.

The Editor is informed by the Astronomer Royal that the statement in the last number of the *Notices*, in regard to Mr. Main's successor in the office of First Assistant at the Greenwich Observatory, is incorrect, and that no successor to Mr. Main has yet been appointed.

The communications received in relation to the Solar Eclipse of 18 July last are reserved until after the meeting of the Society in November, when some further ones will probably come in.—ED.

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